Integrating Gender into Benefit Incidence and Demand Analysis

A Report Prepared by

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Executive Summary

1. The question of whether the benefits of public expenditures in developing countries are equitably distributed by gender has received considerable attention in recent years (World Bank, 2001; Cagatay, et.al. 2000; Elson, 1998). As policy makers and stakeholders become increasingly concerned about gender inequality in society as a whole, it is natural to ask two related questions. First, to what extent does public spending mitigate or exacerbate these gender inequities? Second, how can existing allocations of public expenditure be changed to improve gender equity? This report addresses each of these questions, through a detailed review and interpretation of the existing literature and through primary analyses on a large sample of developing country data sets.

2. Regarding the first question, we consider the question of public expenditure equity in the two dimensions of gender and welfare or well-being, where the latter is measured by the level of household expenditures. To keep the work to a manageable size, we focus on the interaction between these two dimensions and consider several forms of public social spending, in particular, education and health services and water supply infrastructure. A large amount of evidence has been amassed on the incidence of public expenditures by gender, on the one hand, and across the welfare distribution on the other. But very little existing work has studied both simultaneously. One way of rephrasing our first question succinctly, then, is to ask: *how do gender gaps in benefits vary across the welfare distribution?* In choosing this topic, we do not want to diminish the importance of studying gender gaps in general (i.e., without considering the welfare distribution). Instead, we intend to avoid repeating the already large literature on gender gaps and to shed light on a less-studied topic.

3. The question of how public expenditures can more effectively reduce gender inequities is one that can be addressed by benefit incidence analysis only in a very limited manner. What is required is demand analysis or program evaluations that measure the impacts on females and males of changes in specific policy levers, for example, fee levels at health clinics or the provision of better qualified teachers in primary schools. Surprisingly in view of the interest in gender issues – and the particular focus in policy discussions on gender gaps in access to schooling and health care – relatively few demand studies have tried to see whether policies in these sectors affect girls and boys, or women and men, differently. In this study we address this gap by discussing the appropriate methodological approaches to such an analysis as well as adding to the literature with a detailed econometric analyses using data from two countries.

4. In going for breadth of country coverage, we must forego detailed analysis and interpretation for particular countries. Analysts familiar with any one of the countries in our sample will certainly be able to provide more insight into the why and how of our results for that country that we do here. Others may be frustrated by the lack of in depth country-level explanations for the patterns that we observe. Nevertheless, we hope that

our application of a standardized set of methods to many countries will give useful insight into the broad question of the gender and welfare equity of public services, and we hope that the work will provoke practitioners to analyze these results and others like them in greater detail for the country in which they specialize.

Methods

Benefit Incidence

5. Benefit incidence analysis is concerned with the share of benefits received by different groups from a given public expenditure. As such, the only data necessary are (1) a variable that defines the groups, and (2) an estimate of the benefits that each group receives. The most common source of these data is a nationally representative household survey such as a Living Standards Measurement Survey. We define our groups by quantiles of the distribution of household expenditure per capita, which is the standard approach, and also by gender. Thus, rather than ask, “what is the poorest quintile's share of the benefits of public schooling?” we ask “what is the share of girls’ (boys’) benefits in the poorest quintile?” We define “benefits” as a simple 0/1 indicator of whether someone receives the public service in question. This is at odds with much of the literature, but we argue that, given the poor quality of most public expenditure data, it is a sensible simplification.

6. Another difference from much of the existing literature on benefit incidence and gender is the way that we calculate shares of benefits. Rather than calculate female benefits in each quintile as a share of female benefits in all quintiles, we use both female and male benefits in the denominator. This allows us to capture large average differences in the gender gap that other papers miss, and also highlights the difference between relative and absolute gender gaps. Finally, we are careful to include not just shares estimates, but also their standard errors, with which we conduct statistical tests for the difference between gender and across time, by quintiles.

The Demand for Public Services

7. An important limitation of benefit incidence analysis is that it is a purely descriptive analysis of the existing distribution of public expenditures. Demand analysis allows us to go beyond these descriptions to analyze, by gender, the impacts of specific forms of public spending or more generally, specific policies. These include, for example, fee levels at health clinics, the provision of better qualified teachers in primary schools, and construction of new facilities that are more accessible to rural residents.

8. Data requirements for demand analysis generally include both a standard household survey and a complementary community or facility survey that collects detailed data on characteristics of local education and health care providers. Many existing data sets meet these requirements to some degree. However, it is recommended

that community or facility surveys be designed with more of a gender focus than has usually been the case. Information could usefully be collected on factors that may differentially affect female and male utilization of services – for example, the presence of female teachers or separate latrines for girls and boys in schools, or hours of operation of health clinics. To analyze the impacts of water investments, which potentially affect female and male time use, it is necessary to have detailed data on individual time use in water collection and other productive activities or leisure. Community level data on water infrastructure (availability, cost) are necessary for a complete analysis, and household level data on distance to each relevant water source should be collected as well.

9. Gender differences in demand responses to changes in provider characteristics, cost, and distance are captured by interaction terms or, more flexibly, by estimating separate models for female and male samples. Since the demands for services are usually discrete (e.g., a child is or is not enrolled in school) they are estimated using probit or logit techniques, and the appropriate gender comparisons of impacts are in terms of marginal effects, that is, the change in the probability of using the service or a specific provider resulting from a unit change in the explanatory variable.

<u>Measuring Benefits of Public Infrastructure Investments: Time Allocation Effects</u>

10. There are often significant gender differences in the time allocated to certain activities. Public spending can potentially affect these differences, for example through the provision of local sources of clean water. The analysis of the distributional effects of such investments raises several issues not encountered in the analysis of health and education services. Household access to publicly provided water supply is not the appropriate indicator to capture gender specific impacts. These impacts come in the form of time savings and reallocation of time, so one needs to look directly at individual level time use. For descriptive benefit incidence analysis, one can make the assumption that the benefit is the reduction in the individual's time spent in water collection made possible by the service. Then one can simply compare hours per day in this activity by gender and across the income distribution.

11. However, this measure is limited in that it ignores household time reallocations that occur when water supply becomes more (or less) convenient. The time savings may be completely reallocated to other work activities, so that there is no reduction in an individual's overall burden of work even with closer access to clean water. Or, overall work time of women may not fall but there may be substitution to income-generating activities that confer significant individual benefits. Therefore it is important to look not just at changes in female and male water collection times brought about by water supply improvements, but also the effects on overall domestic work, market activities, and leisure. Examination of these outcomes requires a regression framework that allows one to control for other determinants of these time uses, and requires having variables in the data that are reasonable representations of local water infrastructure.

Review of Existing Research

Benefit incidence

12. Despite the ease with which the standard benefit incidence methods can be extended to include gender, the literature is sparse, remarkably so in light of the attention that benefit incidence by gender and by welfare have received individually. A search of published and publicly available research yields only five studies that actually carry out a systematic analysis by expenditure quantile and gender: the seminal works by Selden and Wasylenko (1995) in Peru and by Demery and his colleagues (Demery, et.al, 1995, and Demery, Dayton, and Mehra, 1996) in Ghana and Côte d'Ivoire; a study by Sahn and Younger (2000) for eight African countries; and a large international comparison study by Filmer (1999) that uses 57 DHS samples from 41 countries. Further, a review of World Bank Public Expenditure Reviews that were completed during the past two years finds only two, for Ghana and Malawi, respectively, that examine the correlation between gender and expenditure incidence. Of these studies, only two, Demery, Dayton, and Mehra (1996) and the Ghana PER, looks at how the gender/expenditure incidence of a public service (education) has changed over an extended period of time. So, while we review these studies here, the most important observation is that there appears to be a gap in the literature that calls for the sort of analysis that we undertake in this report.

13. All of the studies that we review focus on the education and health sectors. A few benefit incidence studies look at the incidence of expenditures on infrastructure (van de Walle, 1998 and 2003) or other social sector expenditures (Younger, 2002), but none of these also consider gender differences, or gender difference by income level.

14. It is difficult to draw any general conclusions from the existing literature about how the incidence of public expenditures on health and education varies by gender and expenditure levels. We have few studies, and they are not always in agreement. While it is true that the most comprehensive study, Filmer's (1999) analysis of the DHS data, does show that countries that have large gender gaps also tend to have larger gaps for the poor than the rich, that finding is based on differences in point estimates, not tests for differences. Filmer's own regression analysis finds far fewer cases in which a significant gender gap is accompanied by a significant decrease in that gap across the welfare distribution. Thus, rather than draw firm conclusions from the existing literature, we defer our discussion to our own empirical work.

Evidence of Differential Gender Impacts of Public Expenditure Choices in Education, Health, and Water Sectors

15. Public policy in the social sectors and in infrastructure goes well beyond the determination of the level of expenditures. It includes pricing and subsidy policies, improvements in access through construction of new facilities, and investments in provider quality. Evidence of whether these factors differentially affect females and males comes primarily from analyses of the demand for education and health care and, to

a lesser extent, from program evaluations. Although there are gaps in the literature, several significant patterns emerge with regard to the impacts by gender of public investments in these sectors.

16. Many studies find that girls' school enrollments are constrained more than boys' by distance to schools. Where this occurs, public investments that increase the local availability of schools therefore are likely to disproportionately raise girls' enrollments. There is also some evidence that girls' schooling – and possibly their use of health services – is more sensitive to changes in fees and other direct costs. Where this is the case, programs that subsidize households' schooling costs or that reduce the costs of using health facilities will also have larger benefits for girls than boys. There is more limited evidence as well that the demand for girl's schooling is more responsive than that of boys to improvements in school quality.

17. A few program evaluations of explicit gender targeting – through subsidies to girls' secondary schooling as in Bangladesh, or the construction of separate primary girls schools staffed by female teachers as in rural Pakistan – suggest that these approaches can be highly successful in reducing gender enrollment gaps. Other possible gender-based education policies include the training of more female teachers, the redesign of teacher training to improve attitudes toward girl students, and offering more flexibility in school schedules.

18. Many studies, especially in education, indicate that increases in household resources disproportionately benefit girls. A number of others do not, however, and Filmer's (1999) large multi-country study using comparable data did not find this pattern. There is some evidence that girls do gain more from increases in income in countries where girls suffer a large disadvantage on average. The lack of a strong pattern in the relation of income level and gender gaps in schooling is similar to the conclusion based on the review of benefit incidence studies. A more general point is that one should be wary of making broad generalizations about differential female and male responses to policy and other factors.

Benefit Incidence Analysis Results

<u>Benefit Incidence</u>

19. From the perspective of this report, the most important generalization is that no matter which method we use, we find no consistent correlation between gender gaps in public health and education services and welfare as measured by per capita expenditures. While there certainly are cases in which the gender gap differs from one quintile to the next, they are fewer than we expected, and the correlation is not consistently negative. Even for time collecting water in Madagascar and Uganda, where the gender gaps are very large, there is no evidence that the gender gap is worse in the poorer quintiles. If anything, the reverse seems to be true in Madagascar. The one exception to this

generalization is public employment, where gender gaps are large and, in many cases, increase strongly with expenditures.

20. The gender gaps per se that we observe are consistent with the literature reviewed. Secondary education has many significant gender differences favoring males in all expenditure quintiles. Post-secondary education also has many gaps, though the rarity of post-secondary students in these samples yields large standard errors. Primary education is, with a few notable exceptions, more closely balanced. While there are still many quintiles where boys hold a statistically significant advantage, the reverse is also true in a few cases.

21. These results do not bear out our prior expectation of consistent gender gaps in favor of boys in public schools. Rather, only somewhere between one-fourth and one-half of the quintile-specific comparisons show a statistically significant gap in favor of boys, depending on the level and type of service. Further, we find that the significant differences are highly concentrated in three countries – Ghana, Uganda, and Pakistan. Other countries have relatively few significant differences.

22. Over time, changes in the gender gap for schooling tend to favor girls. The improvement in equity in primary schooling for Uganda between 1992 and 1999 is noteworthy and came in the wake of fee elimination, other educational reforms, and information campaigns. Overall, however, there are relatively few cases where the change in gender gaps is statistically significant, which might lead us to believe that progress is not as rapid as one might hope. But taking into account the many cases where the gap is already small, so that large changes are not necessary, the results look somewhat more promising. In many cases, the significant reductions in gender gaps occur in the same countries and quintiles where the gaps were large to begin with. But the fact that significant gaps remain in the second survey implies that this process remains incomplete.

23. Health care consultations usually display gender gaps in favor of females, in all quintiles of the expenditure distribution. However, if we limit our attention to age ranges for which reproductive health care needs are not a factor, there are very few significant gender gaps, nor are there significant changes over time. Similarly, vaccination rates are almost always similar for boys and girls. Thus, unlike education, gender gaps in health care are of limited importance in these countries.

24. By far the largest and most consistent gender gaps that we found are in two areas that benefit incidence studies do not typically examine: public employment and time spent collecting water. With the notable exception of Bulgaria, men have significantly higher public employment rates than women in all countries and almost all quintiles, and there is no sign that this is improving over time. Lower rates of female public employment reflect in part lower female participation in the formal sector, and it is probably true as well that the public sector is less discriminatory in hiring than private sector employers. Still, if we consider public jobs as a public expenditure 'benefit', this is an example of a clear male advantage.

25. Our data for time spent collecting water are limited to two African countries in our sample (Madagascar and Uganda), both poor. The gender gaps in the burden of this activity in these countries are very large, and point to a means by which governments can at least potentially promote gender equity (in the burden of work or enjoyment of leisure) while pursuing a standard public infrastructure investment in potable water. Perhaps surprisingly, the large gender gap in time dedicated to collecting water does not narrow with income level in Madagascar. It does so, but not dramatically, in Uganda.

Gender Differentiated Demand Analysis: Education and Health Services in Madagascar and Uganda

26. Two case studies of demand for education and health services are conducted using data from Madagascar and Uganda. Somewhat in contrast to expectations from the literature, with relatively few exceptions we do not find gender differences in the effects of a range of provider cost and quality-related indicators. Further, where the null hypothesis that female and male marginal effects were equal could be rejected, it is as often in favor of showing a stronger response of male demand than female demand.

27. Of particular note, distance to education and health facilities consistently emerges as a deterrent to the use of these services in the estimations, but no significant gender differences are found. For direct (monetary) costs of services, there are for the most part no gender differences in impacts. Few significant impacts of non-cost provider characteristics–provider 'quality' – are found. In the one case where quality has strong effects on demand – public primary school enrollments in Madagascar – the effects are generally similar for girls and boys. However, we are unable to investigate a number of factors that might be expected to have different impacts by gender, especially for education: for example, the presence of female teachers, or of separate bathroom facilities for girls and boys.

28. The level of household resources influences schooling and health care utilization in Uganda and Madagascar in almost all subsamples considered (women, men, girls, and boys). In a few cases gender differences exist but – as in the demand for schooling or children's curative health care – these are as likely to favor males as females. On the other hand, girls' secondary schooling in both Madagascar and Uganda appears to be constrained by domestic responsibilities, namely, the need to care for younger siblings. Public initiatives to provide substitute childcare services may function indirectly to target girl's secondary enrollments. Overall, our multivariate findings underscore the point that investigation of gender differences in the impacts of various policy levers must be conducted on a country-by-country basis.

Water Infrastructure Investments and Time Allocation in Madagascar and Uganda

29. In Madagascar and Uganda, as in most developing countries, the burden of water collection time falls very disproportionately on women and girls. Also as in most developing countries, overall hours of work (home and market) are higher for women than men. In an econometric analysis, we address the question, will public investments in water supply serve to reduce the work burden on women absolutely and relative to men? We construct water source availability indicators from the household data to assess the effects of public investments in providing these sources.

30. The results suggest that, in these two countries, such investments can have at best only limited impacts on time use and the gender distribution of work and leisure. In rural areas, where the time burden of water collection is largest, the most feasible large-scale investments would be in well construction. However, the estimates indicate that in both countries, this will not lead to time savings over the alternative of using natural sources such as lakes or rivers. In large part this is because the distances to these two sources of drinking water tend to be similar, as well as not large: the median reported distances traveled to lake/river sources and wells in rural Madagascar are each about 100 meters.

31. In urban areas of both countries, the availability of interior taps (and outdoor taps in Uganda) leads to average reductions in time in water collection. Yet these savings generally do not amount to more than a few hours per week relative to alternative sources sources, as the latter are already fairly close at hand for most urban residents. Hence the effects on time use and the overall burden of work of investments that make interior taps feasible for urban households (or for that matter, that actually provide free piped connections to all domiciles) will be limited. Even in rural areas of Madagascar and Uganda, the time in water collection of women and girls is usually is no more than 3 to 4 hours per week, which puts limits on the time-related benefits to public water supply investments. Time savings may be larger in other countries, especially in more arid climates, and of course investments in clean water supply potentially have very important health benefits for all household members. Nonetheless, our findings caution against assuming that investments in water infrastructure, especially wells, will have dramatic effects on time of females and males and on the division of the overall burden of work between them.

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1 Introduction

The question of whether the benefits of public expenditures in developing countries are equitably distributed by gender has received considerable attention in recent years (World Bank, 2001; Cagatay, et.al. 2000; Elson, 1998). As policy makers and stakeholders become increasingly concerned about gender inequality in society as a whole, it is natural to ask two related questions. First, to what extent does public spending mitigate or exacerbate these gender inequities? Second, how can existing allocations of public expenditure be changed to improve gender equity? This report addresses each of these questions, through a detailed review and interpretation of the existing literature and through primary analyses on a large sample of developing country data sets.

The report presents and uses two main empirical methods: benefit incidence analysis, and econometric analysis of the demand for public services. While our focus on these methods necessarily limits the scope of the report, we chose this focus because the methods are standard tools for public expenditure analysis that practitioners should be able to interpret and may wish to use. The report explores the extent to which these methods can shed light on the impact of public expenditure on gender and income equity, both conceptually and in practice through the application of the methods to data from nine developing countries.

With respect to benefit incidence, we consider the question of public expenditure equity in the two dimensions of gender and welfare or well-being, where the latter is measured by the level of household expenditures. To keep the work to a manageable size, we focus on the interaction between these two dimensions and consider several forms of public social spending, in particular, education and health services and water supply infrastructure. A large amount of evidence has been amassed on the incidence of public education and health expenditures by gender, on the one hand, and across the income/welfare distribution on the other. But very little existing work has studied both simultaneously. One way of phrasing our question succinctly, then, is to ask: *how do gender gaps in benefits vary across distribution of income?* We would like to know, for example, whether the difference in the probability that girls and boys attend school is larger or small for poorer households than it is for better off households. In focusing on this question, we do not wish to suggest that gender inequality is not important in and of itself, or that income inequality in itself is not important. But there are large existing literatures on each of these topics, with relatively little literature on their interaction. The report begins to fill that gap.

Benefit incidence analysis is a descriptive exercise. It is useful to policy makers because it describes something important for them to know, the distributional consequences of public expenditures. These descriptions are most useful when they are surprising, telling us something about the distribution of such spending that we did not expect and that may, in turn, influence our perception of problems that policies need to address. As we will see, some of the results presented in this report are, indeed, surprising.

However, benefit incidence analysis by and large is unable to answer perhaps the most essential question of policy analysis: if we change this policy or introduce that one, how will outcomes of interest in the population change?[1] To address this kind of question, we must go beyond descriptions and attempt to understand the causal link between policies and outcomes by income and gender. In the context of the public provision of services (the services that a benefit incidence study typically examines), this can be achieved with demand analysis or program evaluations that measure the impacts on females and males of changes in specific policy levers, for example, reducing fees at health clinics or providing better qualified teachers in primary schools. Surprisingly in view of the interest in gender issues – and the particular focus in policy discussions on gender gaps in access to schooling and health care – relatively few demand studies have tried to see whether policies in these sectors affect girls and boys, or women and men, differently. In this study we address this gap by discussing the appropriate methodological approaches to such an analysis as well as adding to the literature with a detailed econometric analyses using data from two countries.

Our objectives in this report are therefore: (1) to discuss methods of benefit incidence analysis and demand analysis that can be used to analyze the impacts of public expenditure on gender inequalities; (2) to critically review the existing literature on these subjects; and (3) to present new results for a relatively large (nine) sample of several developing countries. The first two objectives involve, essentially, in-depth reviews of methods and literature, aimed primarily at practitioners who have a background in economics and statistics but who may be unfamiliar with some of the tools used for—and common pitfalls encountered in—this type of analysis. The last objective, in contrast, is to present findings from original research.

The remainder of the report is structured as follows. Section 2 describes methods that are useful for gender-focused analysis of public expenditure incidence. We begin with the basic benefit incidence analyses commonly applied in developing country studies (van de Walle and Nead, 1995, Younger, 2003, and the first three chapters of Bourguignon and Pereira da Silva, 2003). While in theory these methods are applicable to any population sub-groups of interest, the overwhelming majority of benefit incidence analyses define groups based on their poverty status (poor/non-poor) or their welfare status (usually measured by quantile of the per capita household expenditure distribution). We show that it is straightforward to extend these methods to groups defined on two dimensions, gender and welfare. We next discuss econometric tools for gender analysis, showing how results from demand analysis can lead to inferences about the distribution of the benefits of new public expenditures, and of other public policies, across socio-economic groups (including specifically, across genders). Particular attention is given to statistical methods for comparing impacts of changes in specific factors on the utilization of services by females and males. We also discuss methodologies for assessing gender-

[1] Even though it is descriptive, there are a few cases in which benefit incidence analysis may be used in this way, which we discuss in Section 2.

specific benefits of infrastructure investments that may affect female and male time allocations differently.

Section 3 begins by reviewing the existing evidence on the incidence of public expenditures by gender and welfare status. To keep the scope to manageable dimensions, we focus on (the smaller number of) benefit incidence studies that consider the impact of public policies in both dimensions simultaneously. This is followed by a review and interpretation of empirical demand studies and program evaluations in the education, health, and water sectors, focusing on research that has investigated gender differences in utilization of services in response to various policy variables. We consider also the econometric evidence for differential impacts by gender of changes in household income, typically proxied by expenditures, on the use of services. These studies address in a multivariate context the same question considered in the benefit incidence studies we review: do gender gaps in the benefits of public social spending – i.e., in the use of social services – narrow as the level of household resources increases?

Section 4 presents new results on the incidence of public expenditures by gender and household expenditures in our sample of nine developing and transition countries (Bulgaria, Ghana, Jamaica, Madagascar, Mauritania, Pakistan, Peru, Uganda, and Viet Nam), applying the methods discussed in Section 2.[2] For each of these countries we have comparable data for two points in time over the last decade or, in a few cases, starting slightly earlier. We consider incidence in each period and also examine changes over time, seeking in particular to determine whether gender equity has been increasing, decreasing, or static in the countries considered. Our choice of countries is not random but rather is driven in part by data availability, in particular the need for comparable data sets from more than one period, and in part by our desire to analyze countries from many parts of the developing world.

As with most of the benefit incidence literature, we focus on publicly provided health and education services. Unlike most of the existing literature, however, we also attempt to measure the distribution of benefits (by gender and welfare) of infrastructure investments in water supply, as well as examining the distribution of public employment by gender and welfare level. To our surprise, we often fail to find evidence of gender gaps for public services, with the exception of water and public employment. Certainly, there are cases where gaps exist, usually in education rather than in health, but they are far from universal. Moreover, and somewhat at odds with expectations based on the literature, pro-male gaps in schooling, where they exist, do not usually narrow with increases in household expenditures.

In sections 5 and 6 we present new econometric analyses of the demand for public services disaggregated by gender, using detailed data from two of the countries in our sample, Madagascar and Uganda. We estimate the determinants of the use of education and curative health care services as well as the time allocation impacts of water supply

[2] The econometric exercises are more limited, to Uganda and Madagascar. Using those methods for all nine countries would be a major undertaking, and in any case not all of the countries have the necessary data for all aspects of our analysis.

investments. For education and health care, contrary to what one might anticipate from much of the existing research, rigorous statistical comparisons uncover relatively few differences by gender in the effects of factors such as distance to providers, cost, and service quality. The findings for water in Section 6 contain a few possibly surprising results as well. In rural areas of both Uganda and Madagascar, the most feasible wide-scale investments in water supply are to construct wells, but the econometric results suggest that this will result in very little time-savings for women over the main alternative, natural sources such as lakes or rivers. Such investments, whatever their overall benefits to health, will not have much effect on the large gender differences in the burden of fetching water in these contexts.

Section 7 concludes the study by drawing together these findings and discussing their implications both for our understanding of gender and public expenditures and for directions for further research by practitioners.

Finally, we address at the outset two possible frustrations with the study. First, in going for breadth of country coverage, we lose the relevant context for the results in any one country. Analysts familiar with any one of the countries in our sample will certainly be able to provide more insight into the why and how of our results for that country that we do here. Nevertheless, we hope that our application of a standardized set of methods to many countries will give useful insight into the broad question of the gender and welfare equity of public services, and we hope that the work will provoke practitioners to analyze these results and others like them in greater detail for the country in which they specialize.

Second, there are many important aspects of gender-relevant public policy that we do not address. Anti-discrimination or affirmative action legislation are obvious examples that may have a large impact on equity in the use of public services. Cultural and religious attitudes, which to some extent may be affected by public policy, also are likely to have important implications for the gender/income distribution of public services. While we do treat these subjects briefly, we spend relatively little time on them because they do not usually have important implications for public sector budgets, even if they do influence the outcomes that we study.

2 Methods

2.1 Benefit Incidence

Incidence analysis asks who benefits and/or who loses when the government pursues a given tax or expenditure policy. This question has interested economists at least since David Ricardo, who analyzed the incidence of the taxes imposed by the Corn Laws. More recently, attention has shifted from tax incidence to benefit incidence, especially in developing countries. The work of Meerman (1979) and Selowsky (1979) renewed interest in the question of benefit incidence, and a host of studies followed. The tool has become sufficiently common that it has moved out of the academic journals and into standard public policy documents such as public expenditure reviews.

The question of who pays the taxes and who benefits from public expenditures clearly matters. Policy makers and the general public care about how the budget affects different peoples' welfare. Knowing that a disproportionate share of the health budget ends up benefiting affluent urban residents, or that the bulk of education spending goes to boy's schooling, would surely figure in any country's political debates. The Meerman and Selowsky studies were designed to provide such knowledge, leading to information such as that in Table 2.1. The table gives the net results of Meerman's painstaking calculations of the taxes that each expenditure decile paid and the benefits that it received. Overall, the effect of the government's activities is progressive in that the poorer decile's post-fisc shares are higher than their pre-fisc shares.

Table 2.1 – Shares of income by decile, before and after taxes and public expenditures

Quantile	Pre-fisc	Taxes	Benefits/1	Benefits/2	Post-fisc
1	0.013	0.010	0.013	0.067	0.019
2	0.022	0.016	0.022	0.085	0.030
3	0.029	0.022	0.029	0.090	0.037
4	0.035	0.026	0.035	0.096	0.043
5	0.042	0.043	0.042	0.097	0.047
6	0.051	0.052	0.051	0.086	0.054
7	0.062	0.064	0.062	0.081	0.064
8	0.081	0.083	0.081	0.097	0.082
9	0.112	0.115	0.112	0.094	0.109
9.5	0.159	0.164	0.159	0.095	0.152
10	0.395	0.406	0.395	0.111	0.362

Source: Meerman, 1979, Table 8.8 (modified by authors)

Notes: 1/ Benefits that are not excludable, i.e. public goods, distributed in proportion to income shares.
2/ Benefits that are excludable, assigned to beneficiaries.

As a tool for policy analysis, however, the Meerman and Selowsky approach has important limits. Both authors endeavor to calculate the incidence of as much of the budget as possible, thus calculating the aggregate effect of many different expenditures. While this is a monumental undertaking, witnessed by the fact that each author needed to write a book to do it, it is clearly the right approach if we want to gauge the overall redistributive impact of the budget. But it does not provide useful information about specific expenditures or policy changes: e.g. what would be the impact of an increase in primary school subsidies? This more narrow type of question is easier to answer, and also more relevant to the practical concerns of policy makers faced with allocating the budget across many possible line items. For that reason, more recent benefit incidence analyses have focused on the incidence of fairly specific public expenditures – at a minimum, a sector such as education or health – but often a particular service such as outpatient consultations provided at health centers.

As an example, consider Table 2.2, which shows the shares of public education expenditure that each expenditure quintile[3] receives, by level of schooling.

Table 2.2 – Share of benefits from public education expenditure in Ghana, 1989

Quintile	Primary	Secondary	Post-Sec	All
1	21.2	16.8	7.7	17.1
2	22.1	18.0	3.8	17.0
3	22.2	21.8	19.2	21.4
4	20.3	23.4	19.2	20.8
5	14.3	19.9	50.0	23.7

Source: Demery, et.al., 1995 (modified by authors)

This table shows clearly that the poor gain a larger share of the benefits of primary schooling than they do of secondary and, in turn, more from secondary than post-secondary. Thus, if the government were to move subsidies from the beneficiaries of post-secondary education to the beneficiaries of primary education, say, then the poor would benefit. The table also shows that subsidies to public primary schooling go disproportionately to the poor on a per capita basis – the poorest three quintiles get slightly more than their population share of benefits – but this is clearly not true of secondary or post-secondary education. Even though the last column nods in the direction of Meerman and Selowsky by aggregating up the benefits from these three budget items, for the most part, the analysis in the Demery et.al. paper focuses on the disaggregated expenditures. The research that we do in this report follows in this tradition, making no attempt to aggregate up the incidence of the many public expenditures that we analyze.

This research also deals with some well-known limits to conventional benefit incidence analysis. Even when examining specific public expenditures, benefit incidence analysis is

[3] A quintile is one-fifth of the sample after it has been ordered from poorest to richest. A “quantile” is an unspecified fraction of the sample, also ordered.

not always a useful guide for policy analysis. Lipton and Ravallion (1995) first noted that the benefits of a policy change may not be distributed in the same way that existing benefits are. Since benefit incidence analysis describes the existing distribution of benefits, it may be misleading for policy analysis. For example, consider expanding the budget for primary schools. If we do this by reducing school fees or by providing new textbooks for all existing students, then benefit incidence analysis is a good guide to the distributional impact of the policy change because, to a first-order approximation, the new benefits will be distributed in the same way as the existing benefits (Younger, 2002). However, if we do this by building schools in remote areas where children previously did not attend school, then the beneficiaries are, by definition, not the same as existing beneficiaries, and the new benefits are almost surely not distributed in the same way. In such cases, it is incorrect to make inferences about the *marginal* incidence of public spending on the basis of information about current or 'average' incidence.[4]

Given that uptake of education, health, and most infrastructure services in developing countries is generally voluntary, marginal incidence is a matter not just of supply (the nature of the new public expenditures or policies) but of demand behavior as well. Hence we must look (primarily) to econometric studies of the demand for public services to get insight into the distributional impacts of potential policies.

In both our review of existing literature and our original analysis, we will consider both conventional (static) benefit incidence approaches and the use of demand analysis to understand marginal incidence. This distinction is maintained as we discuss methods and data requirements for benefit incidence analysis.

2.1.1 Methods and Data for Benefit Incidence Analysis

Benefit incidence analysis is concerned with the share of benefits received by different groups from a given public expenditure. As such, the only data necessary are (1) a variable that defines the groups, and (2) an estimate of the benefits that each group receives. The most common source of these data is a nationally representative household survey such as a Living Standards Measurement Survey (Grosh and Glewwe, 1998) or a household income and expenditure survey, although summaries of these surveys, as published by national statistical agencies, might suffice if they are disaggregated according to the grouping of interest. Demery (2003) is a good introduction to benefit incidence methods for practitioners.

2.1.1.1 Defining Groups

Usually, the groups are defined by welfare levels – poor vs. non-poor, or each quintile of the welfare distribution – so we require a variable that ranks people by welfare. For reasons given in Deaton (1997), who discusses this question in detail, the preferred choice in the vast majority of studies is household expenditures per capita or per adult

[4] The marginal-average distinction is addressed in various ways by Lanjouw and Ravallion, 1999; Glick and Sahn, 2001, van de Walle, 2003, and Younger, 1999, 2002, and 2003).

equivalent. But other groupings are possible, such as geographic location (political region, urban/rural, etc.), ethnicity, age cohort, or – especially relevant for this report – gender. In all cases, these variables are almost always readily available in the survey data. Finally, as we show below, a combination of variables may define groups. In our case, we will examine groups defined by welfare (e.g. quintiles of the per capita expenditure distribution) and gender.

While measuring each person's well-being by the per capita expenditures of his or her household is common practice, it carries a potential risk, because it assumes that all members of a household enjoy the same level of well-being. If females consistently receive less than an equal share of household resources, then using their household's expenditures per capita to estimate their level of well-being will rank them too high in the expenditure distribution, and vice-versa for males. If males are also more likely to receive the benefit of a public expenditure, then using expenditures per capita will underestimate the welfare ranking of likely recipients (males) and overestimate the ranking of likely non-recipients (females), thus making the distribution of benefits appear to be more progressive than it really is. Unfortunately, no readily available household surveys suitable for benefit incidence analysis are carried out in such a way as to allow calculation of separate welfare measures for each household member.

2.1.1.2 Estimating the Value of Public Subsidies

How much is a publicly subsidized social service worth to recipients? In some cases, the question is easy to answer. If someone receives a 100 shilling transfer payment, it is worth 100 shillings. If someone consumes rice that is subsidized at 100 shillings per kilo, then 100 times the number of kilos consumed is a good estimate of the value of the subsidy. But for most publicly subsidized goods and services, such simple calculations are not possible. Note first that for a *private* good for which demand is continuous, one can use the standard marginal conditions to calculate the value to the individual of the good consumed. But this is not generally applicable to the present case. First, it is not possible to know the value of public goods to individual consumers, precisely because they are non-excludable: people do not have to pay directly for defense, parks, etc, so analysis based on demand behavior (revealed preference) is impossible. Second, while many publicly provided services in developing countries, especially health and education services, are in principle excludable, they are usually heavily subsidized and often are free. In these cases, revealed preference only indicates that the service is worth more to the recipient than the user cost (which may be zero), so we still do not know the actual value of the subsidy to the recipient.

Third, even if this problem did not occur for excludable public services like health and education, demand for them is often discrete. No one chooses two primary educations or a second polio vaccination, no matter how cheap they are. This implies that the typical marginal conditions for continuous demand do not apply. Hence the marginal value of a discrete service may be quite different from the price that the users paid. We can only infer that it is greater than or equal to the price paid (since otherwise the purchase would not be made). Fourth and finally, many publicly provided services are rationed, which also removes the standard equality between price and marginal benefit of a service. Some potential users may value a rationed service quite highly but not be able to consume as much as they would like, because they are rationed.

For any or all of these reasons, benefit incidence analyses usually do not use the price paid by households to value the public services that each group receives. Rather, one of four methods is used: the government's cost of provision, compensating variations from estimated demand functions, a simple 0/1 indicator of public service use, and contingent valuation.

The most common approach in the recent literature is the one that values the service at the government's average cost of provision. That is, if the government spends 100 shillings to provide a health consultation, then we assume that the benefit to the recipient is 100 shillings. This supply-side approach has important theoretical and practical drawbacks.

There is no reason to assume that the subsidy paid by the government is the same or even close to the value of the service to the household, since the latter can only be indicated by demand behavior (or as discussed below, contingent valuation). If anything, it is more likely to correspond to the government's view of the value of the service.

In practical terms, the data used for determining unit costs are often of very poor quality as well as drawing on budgets at highly aggregated levels such as regions, provinces, or even the nation. The latter means that the costs attributed to the services received by any individual reflect a broad average rather than the specific cost for her/his service, introducing a significant aggregation bias. Finally, the difference between what is budgeted and what actually reaches recipients may be substantial (Ablo and Reinikka, 1998) due to administrative inefficiencies or corruption. Better data generation could improve this situation. Certainly it will be difficult for governments to make their expenditures more pro-poor if they do not have ready access to data on the pattern of expenditures, both budgeted and actual. One would hope, then, that developing country governments would strive to collect and make available such information on a timely basis. Lacking that, benefit incidence analyses that wish to use unit cost of provision to value services must collect this information themselves, as in Demery, et.al. (1996, 1996), a process that is expensive and time-consuming.

An alternative but technically quite demanding approach uses information from the demand for services to estimate their value to recipients. Gertler and his colleagues show that it is possible to use compensating variations[5] calculated from econometric estimates of demand functions to estimate the value of a public service to each household even when the demand is discrete as well as partially or largely subsidized to households (Gertler and Glewwe 1990; Gertler and van der Gaag 1990). This is feasible as long as there is some cost to households represented in the data. We discuss the econometrics of

[5] The compensating variation is the amount that one's income would have to change at the same time that a an exogenous variable changes in order to leave her/him at the same utility level that s/he enjoyed before the change. As such, it is the correct measure of the monetary value of the policy change to the recipient.

such models below. For now it should be noted that this is an application of demand models for valuing the current consumption of a service. Like the other approaches discussed here, it is an input into a standard benefits incidence analysis, that is, one that provides estimates of the distribution of the value to households of current public expenditures. As noted above, demand models can also be used for assessing the marginal incidence of specific policy changes. This too is taken up in detail below.

The simplest approach to valuation, which we rely on for much of the research in this report, begs all of these valuation questions and uses a binary indicator of whether or not one uses a service. One might want to term this "participation incidence" rather than benefit incidence, though it is equivalent to a benefit incidence analysis in which we assume that all who use a service or participate in a program receive the same benefit. This is obviously not correct, and most likely introduces a systematic pro-poor bias in the results because the poor probably receive lower quality public services than the rich whereas the binary approach assumes that the quality is the same. In addition, one cannot sum these binary indicators across services to get, for example, the total benefit of all health and education services to an individual, as one could using a monetary valuation.

Nevertheless, the binary method is easy to implement, while going beyond it is not straightforward. In practice, we have found that the binary approach often produces results that are similar to the standard methods, because data on the budgeted cost of provision are often not correlated with expenditures per capita. This may not seem intuitive – as we note, we might expect that less is spent on the poor – but it does occur quite often, perhaps reflecting the imprecise and highly aggregated nature of the cost data used. More importantly, when the two methods do differ, the simpler 0/1 measure of benefits tends to produce results that look more intuitively reasonable (Younger, 1999; Sahn and Younger, 2000). In Madagascar, for example, budgeted costs per patient in rural areas, which are relatively poor, are several times higher than those in urban areas, so that using the cost of provision rather than a 0/1 indicator yields a more pro-poor estimate of the incidence of public health facilities. Yet it seems implausible to anyone familiar with Madagascar that rural residents are really getting more valuable health care at public facilities than urban residents receive. Thus, because of problems of data quality, valuation approaches that are better in principle may end up being less reliable in practice.

Finally, a fourth approach is contingent valuation. This method relies on surveys that ask people, essentially, “How much is this service worth to you?” This approach has not been very popular in economics because it does not rely on revealed preference (Diamond and Hausman, 1993), that is, observed demand behavior. Especially for pure public goods, however, it is the only game in town (Arrow, 1993). Nevertheless, because almost no existing nationally representative surveys have used this method to date, there are no incidence analyses using contingent valuation in developing countries. Contingent valuation has instead been used in smaller scale surveys to assess the value to communities of certain health interventions such as provision of mosquito nets, without much attention to distributional issues.

2.1.1.3 Exactly What Public Subsidies?

As described above, the early benefit incidence studies tried to estimate the total value of all public spending to various groups (Meerman, 1979; Selowsky, 1979). Thus, their answer to this question was "as many of them as possible." However, much of public expenditure goes to providing public goods such as defense, security, and the rule of law. By their nature, public goods are not excludable, i.e. one person's benefit does preclude another's, so they do not usually have markets or prices. Determining the value of these goods and services to individuals is impossible, so a benefit incidence analysis, which describes the distribution of individual benefits, is also impossible. As a practical matter, benefit incidence analysis is only applicable to the public provision of private benefits.

Most of the recent literature is decidedly less ambitious than Meerman and Selowsky, focusing almost exclusively on education and health expenditures, which are the two largest types of social spending in most developing countries. They are also the two areas of public expenditure that most income multi-purpose household surveys ask about. Questions about use of other public services are less common and, obviously, without them, no benefit incidence analysis is possible.

In our own work, we have made the case for disaggregating the subsidies under study as much as possible – going as far in the opposite direction as possible from Meerman and Selowsky's aggregation of as many benefits as possible. Our argument is that much of the interest in benefit incidence comes from a desire to reallocate the budget in a pro-poor way. If this is our goal, then it is useful to have information on the distributional consequences of very specific expenditures that can be promoted or discouraged, rather than large aggregates. Thus, we favor the recent literature's attempt to look at disaggregated line items rather than entire budgets. However, the narrow focus on health and education is unfortunate. While it is true that benefit incidence analysis is limited to private benefits of public expenditure, there are certainly many line items outside of education and health that provide such benefits. Expanding the range of expenditures considered will shed more light on which parts of the budget are especially pro-poor, information that policy makers should find useful.

In the extreme, our disaggregated approach makes data demands that go beyond what household surveys routinely provide. For example, the majority of household surveys ask whether someone has visited a health practitioner over some time period, and most distinguish types of practitioners, including public and private. Far fewer ask detailed questions about the quality of the services provided. This is very important to understanding incidence, since quality is likely to vary, and in fact, to be lower in areas where the poor live. However, it would be difficult to obtain this information from households (e.g. What was the provider's training?). This is why some – but far from all – household surveys are complemented by community and local provider surveys to provide detailed data on the characteristics of service providers: for example, on qualifications of staff, as just noted; on availability of drugs; on the use of electricity, refrigeration, and running water, etc. The appropriate questions are also asked of schools. Such provider and community surveys have been less common than the household surveys themselves, but the situation is improving. Certainly such information is useful to policymakers who want to understand the incidence of detailed aspects of their public expenditure program, and the cost of collecting it is low relative to the household survey itself, so we would recommend that all general purpose household surveys include these modules as well.[6]

2.1.1.4 Presentation and Interpretation of Results

Because a benefit incidence analysis is a descriptive exercise, presenting its results is fairly straightforward. Existing literature relies on easily interpretable tables giving the benefits that different groups receive from a public expenditure or expenditures, usually in terms of shares. More recently, some studies have begun to provide graphical presentations of results, based on concentration curves. While somewhat less intuitive than the standard tables, they have an attractive interpretation in terms of welfare theory (Saposnik, 1981; Shorrocks, 1983; Foster and Shorrocks, 1988; Yitzhaki and Slemrod, 1991; Lambert, 1993). As it happens, both of these approaches have important shortcomings when we want to evaluate incidence across the welfare distribution *and* by gender, so we propose a modification that avoids these problems.

The most common, and simplest, presentation of results is to report the share of benefits that each group receives. The usual grouping is by quantiles of the expenditure distribution, but in our case, we will group people by both quantiles and gender. Demery, et.al. (1995) and Demery, Dayton, and Mehra (1996) use this approach. Table 2.3, taken from the research in Section 4 of this report, provides an illustration. The table’s cells show each per capita expenditure quintile’s share of the total number of male or female public secondary students in Peru in 1997.[7] A concentration curve simply cumulates and graphs these cells. The table shows, for example, that 18 percent of male and 14 percent of female secondary students are in the poorest quintile. In general, the middle quintiles have somewhat higher shares of pubic secondary students. One can surmise (and, indeed, confirm in the data) that the poorest quintile is under-represented because disproportionately large numbers of poorer students, especially females, do not go to school. On the other hand, richer students have a disproportionately low share of public school attendance because they attend private schools. Overall, the distribution of male students is slightly more equitable than that of female students because relatively fewer girls in the poorest quintile go to school.

[6] This information is also crucial to unbiased and policy-useful estimation of the demand for public services, as we discuss below.

[7] Note that the quantiles in tables like Table 2.3 should be defined for individuals, even though the data on expenditures are collected at the household level, because it is the welfare of individuals that we value.

Table 2.3 – Quintile shares for attendance at public secondary schools, Peru, 1997

Quintile	Male	Female
	0.18	0.14
1	(0.007)	(0.0065)
	0.22	0.23
2	(0.008)	(0.0084)
	0.22	0.22
3	(0.008)	(0.0083)
	0.23	0.26
4	(0.008)	(0.0089)
	0.16	0.15
5	(0.017)	(0.018)

Note: Standard errors in parentheses.
Source: ENNIV 1997 and authors' calculations

Most authors take 20 percent as a benchmark for an "equitable" share of benefits for a given quintile, because each quintile represents 20 percent of the population. Shares higher than 20 percent in the lower quintiles indicate "pro-poor" or "progressive" services and vice-versa.[8] From a gender analysis perspective, however, we have a problem. A strict Benefit Incidence interpretation of Table 2.3 is the following: the distribution of boys going to school is more equitable than that of girls, so a transfer of public resources from girls' schooling to boys' schooling will help to reduce inequality. While technically correct, and consistent with welfare theory (Shorrocks, 1983; Yitzhaki and Slemrod, 1991), this is clearly not what most people would take away from the table. The problem stems from the fact that the standard analysis is concerned with inequality in the income dimension only, while most people would be concerned with income *and* gender inequality. Thus, rather than the traditional interpretation, the usefulness of benefit incidence analysis here is to describe inequities in one or both dimensions so that policy makers can take note of situations that may require remedial action. To the extent that one is concerned primarily with gender biases, the analysis also suggests where in the income distribution these gaps are most severe, which may help in devising policy responses.

Table 2.3 differs from many benefit incidence analyses in that it includes the standard errors of the quantile share estimates. Statistical comparisons of the cell means in Table 2.3 is certainly possible, and we will make them in our own research, but many authors ignore the issue of statistical inference. This common practice can result in incorrect conclusions about the presence of differences in the population.

One problem with the approach taken in Table 2.3 is that it relies on *shares* of benefits, and thus standardizes by mean benefits. This is appropriate for the Yitzhaki-Slemrod type of welfare interpretation of the results, but it obscures information that may be of greater interest, particularly when studying gender differences in benefits. Consider the example in Table 2.4. Each quintile's share of benefits is equal for boys and girls, which would lead to

[8] Other definitions are, however, possible. The next most common benchmark is the share of expenditures or income per capita in each quantile.

identical quantile shares within each gender, which is how tables like Table 2.3 calculate them. Yet this obscures the fact that boys capture far more benefits than girls on average. A few studies, most notably Demery, et.al. (1995) and Demery, Dayton, and Mehra (1996), also calculate row shares, i.e. the share of benefits going to males and females within a quantile. This will highlight changes in the relative gender gap, but still obscures the absolute value of the difference in benefit shares going to males and females. For example, in Table 2.4, the absolute value of the gender gap (boys' benefits minus girls') increases over the expenditure distribution in this example even though the relative gap (boys' or girls' benefits divided by total benefits in a quantile) does not.

Table 2.4 – Example of mean scaling in concentration curves

	Girls		Boys	
Quintile	Benefits	Share	Benefits	Share
1	1	0.100	3	0.100
2	1	0.100	3	0.100
3	2	0.200	6	0.200
4	2	0.200	6	0.200
5	4	0.400	12	0.400
Total	10		30	

Source: authors' calculation

To avoid these problems, we will use tables in which each quintile/gender's share is calculated with reference to total benefits for both genders, yielding results like Table 2.5.

Table 2.5 – Example of shares calculated over both genders

	Girls		Boys	
Quintile	Benefits	Share	Benefits	Share
1	1	0.025	3	0.075
2	1	0.025	3	0.075
3	2	0.050	6	0.150
4	2	0.050	6	0.150
5	4	0.100	12	0.300
Total	10		30	

Source: authors' calculation

Using exactly the same data, this table gives a different impression of the gender gap in benefits, both absolutely and relative to the expenditure quintiles. Note first that the calculations in the table lend themselves to a straightforward comparison of actual benefit shares of a quantile/gender subgroup to what would be a 'fair' distribution. If the population of boys and girls is the same or almost the same in each quantile, which is reasonable to expect, then 10 percent would be a fair (equiproportionate) share for each quintile/gender cell as calculated here. The gender gap is clear from the table — boys have a larger share than girls at every quintile, and only girls in the fifth quintile have an "equitable" share of benefits as just defined. The gender gap also increases in absolute terms over the expenditure distribution, even though the ratio remains constant at three-to-one in favor of boys in every quintile. While the standard approach of Table 2.4 is useful for interpreting the incidence of expenditures by gender, and while it has a rigorous welfare economics interpretation, it is a less useful descriptive tool than using shares as defined in Table 2.5. Hence in our own benefit incidence analysis in section 4 we will use the latter approach.

2.1.1.5 Benefit Incidence Among Potential Beneficiaries – Coverage Rates

The methods discussed thus far describe how the benefits of public expenditures are distributed across the entire population. When we find that, say, 22 percent of spending on public primary schools goes to the poorest 20 percent of the population, we count everyone, adults and children alike, when we define that poorest quintile. An alternative approach examines the distribution of benefits among potential beneficiaries, also referred to as the target population for the service. For example, we might be interested to know what percent of all children less than five years old have been vaccinated for measles, or what percent of primary age children are attending primary school. What this gives is an indication of the *coverage* of the program.

We can disaggregate coverage information by a welfare variable and/or other categories like gender: 90 percent of children in the richest quintile are vaccinated, but only 40 percent in the poorest quintile; or 80 percent of girls are vaccinated, but only 60 percent of boys.

Even though we can look at coverage by groups such as expenditure quintiles in this way, the information provided in this method is not the same as the information from a benefit incidence analysis if the potential beneficiaries for a given expenditure are not distributed evenly across the groups of interest. For example, the benefits of an adult literacy program can be highly concentrated among the poor even if its coverage is low, and even if its coverage is lower for the poor than the non-poor, simply because the target population of illiterate adults is concentrated among the poor.

Schooling provides a more common example of this divergence. So in fact does any service for which the target population is children. Because poor households tend to have more children, the distribution of the target population is skewed toward the lower quantiles.

Table 2.6 – Coverage rates and benefit shares for public primary schooling in Viet Nam, 1993

Quintile	Coverage	Benefit shares
1	0.63	0.22
2	0.74	0.23
3	0.77	0.21
4	0.81	0.17
5	0.87	0.16

Source: VNLSMS 1993 and authors' calculation

Table 2.6 shows that the public enrollment rate among primary age children in Viet Nam in the first expenditure quintile is 63 percent, significantly lower than the shares of the other quintiles. Yet this 63 percent accounts for a slightly more than proportional share (22 percent) of all public secondary enrollment because the poorest quintile accounts for more than 20 percent of the target population (children of primary school age).

By virtue of its target population focus, coverage, unlike benefit incidence, can say how effectively a program is reaching its designated beneficiaries. Further, coverage calculations done by quintile clearly say something about distribution. Given this, does analysis of coverage really differ conceptually from benefit incidence (other than by the change in denominator from population to target population)?[9] Strictly speaking, there is a difference. Standard fiscal incidence analysis considers the provision of services as an income transfer that augments current welfare, measured by incomes or consumption. For this it is proper to look at the distribution of welfare in the entire population: education services received by a child in a poor quintile raises his or her welfare, hence improves the distribution of welfare overall. So describing the distribution of benefits across all people, as BI does, is the best way to identify programs that transfer resources to poorer people and so help to determine how budgetary allocations across different public expenditures affect the *ex post* welfare distribution.[10] In developing countries, where direct provision of services to the poor is usually the main way governments can mitigate income inequalities (since transfer payment systems are administratively difficult), this way of looking at education and health services has a good deal of plausibility.

Still, such services – especially education – are usually thought of as investments in an asset (human capital) that yields future returns through, among other benefits, greater labor market incomes. One might prefer to explicitly recognize the intergenerational (and intertemporal) nature of education investments and view the gains from education subsidies as accruing to the children themselves, in the future. This implies a concern with the *future* distribution of welfare among those who are school age children today;

[9] For more detailed discussion of the issues discussed here, see Glick and Razakamanantsoa (2001) and Bourguignon, da Silva, and Stern (2002).
[10] Technically, we also need to consider the distributional consequences of the source of the funds used to finance the benefits.

hence is it the distribution of schooling benefits among this subpopulation that is of interest from this perspective. This leads us back to looking at target population coverage (by quintile), though given that distribution concerns remain in the fore, some might prefer to consider this simply a different measure of benefit incidence, e.g., 'per child' benefit incidence as one set of authors calls it (Selden and Wasylenko 1995).[11] One would be led to the same per-child or more generally, per target population member focus if one framed the issue instead in terms of "needs."(Castro-Leal et. al. 1999; Van Doorslaer, Wagstaff, and Rutten 1993). From this perspective, the allocation of benefits across the income distribution should be compared to the distribution of the need for the service. In the example of schooling, needs are the number of children who must be educated; for curative health care, it would be the number of sick people in a given quintile.

Despite these differences in measures and perspectives of standard BI and coverage (or if one likes, standard BI and BI per target population member or the needs perspectives), these distinctions essentially become irrelevant if we are concerned only with gender differences across the income distribution. This is because there are usually more or less the same number of males and females in each income or consumption quantile. Therefore, while (for the case of education) the per capita and per child measures imply different distributions of benefits across income quantiles, the differences are equivalent for girls and boys; we are simply changing the denominator of two fractions in the same way. For the same reason, male-female (proportional) differences in the quantile/gender benefit shares defined above for Table 2.5 (with which we will work extensively in our data analysis in section 4) are equivalent to the gender differences in coverage within the quantile. This is shown formally in Appendix 2.1.

All this means that we can analyze gender differences with one or another measure that we may prefer for other reasons and make essentially the same inferences, though our conclusions with respect to distribution of overall (male and female) benefits across income groups may differ. Although our focus in this report is indeed on gender, we will still show coverage tables for several reasons. First, such tables are useful because they are standard and easily interpreted by non-specialists. Second, our benefit incidence results will focus strictly on public benefits, while for coverage the ties to fiscal incidence analysis are less strict. This makes the latter a good place to also discuss the use of private services by males and females in different income strata.

Finally, we noted in the discussion of Table 2.5 that a 'fair' distribution of benefits would be one in which quantile/gender subgroup received equiproportionate benefits. If we are considering 'needs' or target populations, the analogous measure would compare the share of benefits of a quantile/gender subgroup to its share of the target population. This of course would incorporate the possibility that the target population is not equally

[11] Since we are looking at rates of children's enrollment (a determinant of future welfare) by quintile of their current per capita household income, we are in a sense considering how public education spending affects the next generation's ranking in the income distribution relative to their current (their parents') ranking. While this is obviously quite crude, as many factors other than schooling will affect the relation of one's own to ones' parents' income, it captures an essential concept.

distributed across income levels. This can be done very easily using coverage tables by comparing (for the example of education) the enrollment rate of the specific quintile/gender group, for example, girls in the first quintile, to the overall enrollment rate (the mean enrollment for all girls and boys). If the ratio of the former to the latter is equal to one, the subgroup's share of the benefit equals its share of the target population; if it is less than (greater than) one, its share is less than (greater than) its share of the target population.[12]

2.2 The Demand for Public Services

As discussed above, an important limitation of benefit incidence analysis is that it is purely descriptive of the status quo at the time of the survey. It yields little insight into people's behavior, yet such behaviors are often crucial to our understanding of the distributional consequences of public policies. The empirical modeling of such behaviors – demand analysis – is the focus of this section. Because the methods rely on regression analysis, they are more familiar to most economists than those outlined in the last subsection, so they should require less detail and justification. Nevertheless, there are important features of the demand for public services that require somewhat non-standard methods. Our exposition focuses on those special features.

2.2.1 Methods and Data for Estimating the Demand for Public Services

A distinctive feature of the demand for many public services is that it is discrete: one enrolls in school or does not enroll, one seeks health care from provider type *j* or does not. In contrast, the vast majority of the demand literature is concerned with continuous goods (Deaton and Mullbauer, 1983). There is, however, a fairly substantial literature that uses discrete choice models to estimate demand functions for public services, especially in developing countries (McFadden 1978 and 1995; Gertler and Glewwe 1990; Gertler, Locay, and Sanderson, 1987; Dow 1995a,b; Younger 1999; Sahn, Younger, and Genicot 2003; Glick and Sahn 2001). There are many variants of such models, the choice of which to use being in large part a function of the type of data that are available. The simplest approach is to estimate the (0,1) decision to utilize a service using a binary probit or logit model.

However, households, even in poor rural environments, often have more than one provider from which to choose – e.g. public vs. private school (or local vs. distant school); public clinic, private clinic, or traditional healer, etc. Models that estimate the choice among discrete alternatives, called polychotomous or multinomial choice models, rely on the idea that there is a small set of options available and individuals choose the

[12] Letting capitals (G,B) letters represent the total girl and boy school age (target) population and small letters (g,b) represent total girl and boy enrollments, the benefit share of girls in quintile j over their share of the target population is $\frac{g_j/(g+b)}{G_j/(G+B)}$. This is the same as $\frac{g_j/G_j}{(g+b)/(G+B)}$, the ratio of quintile j girls' enrollment rate to the population mean enrollment rate.

option with the highest utility level. (Clearly this decision rule applies also to the case of just one provider, but the comparison reduces to one between utility from using the provider vs. using none.) Utility of the household conditional on choosing a provider is assumed to be function of the benefit of the service – a child's scholastic attainment from another year at school, the improvement in one's health after being treated – as well as of the level of other household consumption, which must be reduced by the costs of using that provider. Utility is not observable, nor generally is the benefit from using the service. Therefore the models assume that utility is a function of observable variables such as income, prices, characteristics of the option (quality), and characteristics of the individual or household, plus an unobservable error term:

$$V_j = U(y, p, Q_j, Z) + \varepsilon_j$$

where V_j is the indirect utility associated with option j; y is income; p is a vector of prices; Q_j is the quality of option j (its characteristics); Z are household or individual variables that are the same for each option; and ε_j is the unobservable component of utility. As noted, V_j itself is not observable, so this equation cannot be estimated, but it is possible to estimate the probability that V_j is better than any other option V_i for each i, making use of the information about which option an individual or household actually chooses. The precise model estimated depends on the assumptions that we make about the distribution of the ε_j's, but almost all such models in the literature are some variation of a (multinomial) logit or (multinomial) probit.[13]

2.2.1.1 Focusing on Gender

It is fairly standard to include a gender dummy in the Z vector, thus allowing for different intercepts for males and females. However, this does not provide all relevant information about gender and the demand for services. For example, a concern with potential gender differences in demand between poor and rich households calls for, at least, including both the gender dummy and a gender/expenditure interaction term to allow for some flexibility by welfare level in the estimation. In the case of schooling, a negative coefficient on the gender dummy combined with a positive coefficient on the interaction term would indicate that the probability of attending school is lower for girls than for boys – a gender gap – and that the gap decreases at higher expenditures. More importantly, consider the case where poorer boys are more likely to go to school than poorer girls but richer girls are more likely to attend than richer boys. Then a model with only the gender variable could have a zero coefficient on that dummy, because the positive male-female difference for the poor averages out the negative male-female difference for the rich. The simple model would lead us to conclude quite wrongly that gender is not a factor in the demand for the service. To date relatively few studies have looked at the interaction of gender and income in the demand for services.

[13] Dow (1995a,b) reviews these models and the assumptions about ε_j that distinguish them. Many software packages now run basic logits and probits. Stata, SAS, and Limdep run nested multinomial logits, and SAS and Limdep run multinomial probits. The nested logit and especially the multinomial probit can be difficult to estimated given the complexity of the models.

Gender interactions with policy variables are also potentially very important -- probably more important for policy makers -- though again, quite rarely seen in the literature. These variables include factors such as cost or distance to schools or clinics, and indicators of various aspects of service quality. For example, we might want know if the demand for girls' schooling is more or less responsive than boy's to specific improvements in school quality. If it is, and the context is one where girls are disadvantaged in education relative to boys, then investments in quality would be a policy lever for reducing the gender gap, in addition to its overall beneficial impacts on learning and enrollment. Or, if girls' enrollments are more sensitive to distance to schools than are boys' enrollment (a common finding), school construction programs in rural areas will benefit girls disproportionately. The existing evidence for gender differences in the effects of policy-related factors is reviewed in detail in Section 3.2, and we conduct a new gender-differentiated demand analysis in Section 5.

In the extreme, one could estimate separate models for males and females, which is equivalent to interacting all covariates with gender.[14] In fact, if sample sizes are adequate, it is recommended that separate models be estimated, as there are some potential disadvantages to not doing so. In a pooled model, selectively interacting certain regressors with gender and not others can lead to misleading results. Depending on the pattern of correlations in the data, the included interactions can pick up the effects of other interactions with gender that are not entered – an omitted variable bias. The more flexible approach avoids this risk.

Comparisons of female and male effects should always be statistical. For linear regression such tests are straightforward: for some policy variable x, say price, one simply tests for significance of the interaction term of gender and *x* (of course, the gender dummy and *x* are also included separately). As indicated, however, in many or even most cases, the demand for services is discrete and methods such as probit of logit must be used. In such cases the estimated coefficients are not the comparative static effects, that is, they do not show the effect of a unit change in the regressor on the outcome of interest, the probability of using the service of choosing a given provider.[15] Instead, these comparative static effects, also called marginal effects, must be calculated from the estimated parameters and the data. Statistical comparisons of male and female impacts of explanatory variables should be based on these marginal effects rather than the parameters themselves. As we demonstrate in the appendix to this section, inferences about gender differences may not be the same in the two cases. Constructing the appropriate standard errors to perform tests of equality of marginal effects is usually not difficult, but it does impose more of a burden on the researcher than simply comparing coefficients. Methods for the tests are presented in detail in the appendix to this section.

[14] The only difference is that, where (as in OLS regression) the variance of the regression is estimated, the pooled model with interactions imposes the same variance for both genders. Estimating separate models is therefore more flexible. As discussed below, this is not an issue for binary probit, binary logit and multinomial logit models since these models normalize by setting the error variances to a specific value for identification purposes.

[15] In the standard discrete choice framework the coefficients instead measure the effect of the variable on V_j, the utility from using provider j.

2.2.1.2 Data Requirements

The data requirements for demand analysis are significantly greater than those for benefit incidence analysis. The dependent variable – the choice of using the service (or which provider of the service) – and the welfare variable, y, are the same that we use for benefit incidence analysis. Candidates for Z variables include standard household and individual characteristics, including information on individual's parents, which most household surveys include.

It is important to be aware of data limitations that can yield misleading estimates of key parameters. While surveys generally collect reliable information on the variables just described, many surveys are significantly weaker when it comes to price data, p, and characteristics of the options available, Q_j. These data are typically collected at the community level, or (more rarely) directly from providers, rather than from households. Unfortunately, most household surveys are not complemented with community or provider surveys. For policy analysis, this is a critical omission, since the variables that policy makers can control to influence the demand for public services are found here.

Household survey data do often contain some key information about providers, namely fees paid and distance to providers. For these variables, several of the data issues discussed in Section 2.2.1 are relevant. Often, there is no explicit fee for public services, so we must focus on opportunity costs of using the service, in terms of time and/or distance to the service. But time and distance may have their own, independent influences over the demand for public services which confounds the coefficients' interpretation as a pure price effect. This is discussed in more detail in Section 3.2. In addition, when there are no (or few) provider characteristics available in the data, unobserved provider quality is often highly correlated with the observed price. So much so, in fact, that it is not uncommon to find that the latter's coefficient is positive, especially in education models, because it picks up part of the positive effect of quality. This is also a problem when the survey does collect some provider data, if the variables collected are incomplete or poorly measured.

These data problems naturally also affect the reliability of the effects of provider characteristics themselves. Measurement error would tend to lead to underestimates of their impacts while a lack of data on other provider characteristics would in contrast lead to overestimating the effects of the quality variables that are included in the model, since they capture in part the effects of excluded factors with which they are correlated. Local service quality variables may also be positively associated with unmeasured community level preferences for education and health (or more generally, with other community factors that improve these outcomes), which would imply an upward bias in the estimated effects; put another way, these covariates may not be exogenous to schooling and health care outcomes. One way this could occur is through 'selective migration', whereby households with strong preferences for education or health move to communities where schools or health facilities are of better quality. Or they may move simply to be closer to education and health services, in which case we will tend to overestimate the (negative) effect of the distance to providers on demand.

More or less the opposite of this pattern, governments may purposely locate facilities or upgrade service quality where the population is disadvantaged or for other reasons is less likely to utilize the service. Such ‘endogenous program placement’ (Rosenzweig and Wolpin 1986) would imply a *downward* bias in the estimates of the effects of quality on demand, since the estimates capture in part this association of poor outcomes and high quality. All of the above may bias the estimates for males and females in different ways, so we would not be able to be sure even about estimates of the relative impacts on males and females, a key focus for a gender relevant analysis.

These considerations explain the recent expansion of ‘policy experiments’ in which education or health programs are randomly assigned to some communities and not to others (several such projects are discussed in Section 3.2). Most analysts will not have the good fortune of being able to conduct, or use data from, randomized interventions, so they will have to confront the endogeneity and other problems inherent in non-experimental data. In some cases at least, one can assign a sign to the bias. For example, if one is familiar with the policy environment of the country, it may be possible to rule out the existence of endogenous program placement. In this case the bias on the effects of provider quality, if there is one, is probably positive.

From the perspective of gender analysis, a further limitation of most general purpose household and community/provider surveys is that they typically are not designed to capture gender relevant characteristics of services. Specialized studies have found that characteristics such as the share of female teachers, distance to school, and gender-segregated classes and latrines are more important for girls’ schooling decisions than for boys’. Other than the distance variable and possibly the share of female teachers variable, such questions rarely figure in surveys that are not specifically concerned with gender equity. Our understanding of which policies may be used to rectify gender imbalances in schooling and health would be greatly improved if community or provider questionnaires accompanying standard household surveys began to include such information.

2.2.1.3 Relation to Benefit Incidence Analysis

While there is much to be learned about variation in demand for public services by gender and welfare levels from regression results alone, it is possible to use the regression results to provide the “value of services” data for a benefit incidence analysis. Small and Rosen (1981) and McFadden (1995) show how to use demand estimates from discrete choice models to estimate the compensating variation for a policy change. The compensating variation is the amount that one’s income would have to change at the same time that an exogenous variable changes in order to leave her or him at the same utility level enjoyed initially. As such, it is the correct measure of the monetary value of the policy change to the recipient.

By calculating the compensating variation of a policy change for each person or household in a sample, we can then examine the distribution of those estimated benefits across groups such as gender and expenditure quantiles in a “typical” benefit incidence analysis. However, we are no longer limited to estimating the distributional impact of the service as a whole, which would be the standard benefit incidence measure. We can examine the distributional consequences of price changes, changes in individual characteristics of a service such student/teacher ratios, waiting times at clinics, qualifications of service providers, etc. Basically, we can simulate changes in any characteristic for which data are available and included in the demand regressions, expanding the possibilities of the benefit incidence analysis significantly. Further, these simulations are proper policy analyses in the sense that they capture the distributional consequences of the policy change, whereas the benefit incidence method captures only the distribution of the existing service.

The compensating variation approach estimates the change in utility (welfare), and its distribution, from a policy change. A simpler method is to look at the change in utilization of the service in response to a policy change by expenditure group or gender. Essentially, this involves comparing the predicted probabilities of use before and after the policy change, for each quantile or gender, to derive the change in the probabilities. The predictions can usually be generated in a straightforward manner from the demand estimates and the data; we need to calculate the probabilities for the current value of the policy variable in question and for a second value representing the value after the policy change. An example of this approach is Glick and Sahn's (2000) analysis of primary education in Madagascar.

Rather than giving us the distribution of changes in welfare from the policy change, this provides, for example, the distribution of new primary enrollments across the expenditure distribution. The two approaches may lead to different conclusions about the distributional impacts of a policy.[16] Compensating variation in principle would seem preferable since we are concerned with the distribution of welfare. However, it measures only private welfare to households (or in the case of schooling or health care of children, to the parents in these households). Because of well-known externalities to investments in schooling and health, private benefits to households of schooling a child or seeking health care are likely to be below the social benefits. This may lead us to prefer to look at changes in school enrollments or health care usage directly.

The use of econometric demand models to estimate the distributional effects of policies is in principle much to be preferred to simply assuming that these effects will be in proportion to existing benefits. However, demand estimates are at least an order of magnitude more difficult to obtain. The models are also often very sensitive to standard assumptions made about the functional form of the indirect utility function and the error terms. These issues are likely to be especially serious when attempting to calculate estimates of compensating variation, and somewhat less so when simply calculating the change in probabilities. Few authors have examined the robustness of their estimates and

[16] For example, say that a price increase is found to lead to disproportionate enrollment reductions for the poorest quintiles. This does not imply that the price increase leads to a greater proportional welfare loss for poorer households than rich households. In fact, the fact that the poor's demand is more price-elastic implies that they suffer smaller welfare reductions as they substitute more easily to other goods and services (see Dow 1995b).

policy conclusions to the restrictions imposed in their models (Dow 1995a, 1999 is an exception).

Finally, beyond the standard assumption that the right-hand variables are exogenous, a more subtle identification issue concerns rationing. We can estimate the discrete choice model only by assuming that observed option for each person is really chosen, i.e. that it provides the greatest utility for her/him. If, however, the public service in question is rationed (for example, there are inadequate places in the local school to accommodate all who would want to attend), this may not hold. Someone could have a very high indirect utility from attending a public school but still not be observed to be in public school because s/he is rationed out.

2.3 Measuring Benefits of Public Infrastructure Investments: Time Allocation Effects

There are often significant gender differences in the time allocated to certain activities that public services can affect significantly. The analysis of the distributional effects of such investments raises issues not encountered with the more standard incidence analysis for health and education sectors and thus requires slightly different methods and data. For this reason, we consider these investments separately in this section. However, the essential concepts remain the same.

Consider the case of publicly provided water supply, which we will be examining in detail in this report. Such provision may have large time benefits because they reduce the hours per day or week necessary to collect water from distant sources in the absence of the investment. We are interested in knowing how these benefits are distributed by income level and, especially, by gender. If we look only at who has access to safe water, we will almost certainly find differences by expenditure level, but we are unlikely to find differences by gender, overall or across the expenditure distribution. This is because entire households, not specific individuals within them, have access to safe water, and there is usually little variation in the gender composition of households across the expenditure distribution.[17] Clearly, household access to publicly provided water supply is not the appropriate indicator to capture gender specific impacts. Since gender differentiated impacts come instead in the form of time-savings, we need to look directly at variables related to time use.

How can these time benefits to water infrastructure be measured? For a purely descriptive benefit incidence analysis, we can make the assumption that the benefit is the reduction in the individual's time spent in water collection made possible by the service. This suggests that we can simply compare, say, hours per day in this activity by gender and across the income distribution. In fact, this provides an inverse measure of the benefit since more hours in water collection means less benefit. Therefore, it is appropriate to

[17] Note that this also implies that the health benefits of clean water supply are not likely to be very different within households, hence also, across genders.

define an indicator that could be called ‘time not required for water collection’, calculated as 24 minus the average hours per day collecting water; this converts the ‘bad’ to a ‘good’ (one could make an assumption about total time awake and use, say, 16 instead of 24 hours). This indicator will, of course, be distributed exactly the same way as the first measure, but it is more convenient since we can use it to construct benefit concentration curves and other measures comparable to those for education and health services.

There is, however, a significant limitation to this measure. It ignores household time reallocations that occur when water supply becomes more (or less) convenient to access. The time savings may be completely reallocated to other work activities, so that there is no reduction in an individual’s overall burden of work – no increase in her leisure – even with closer access to clean water. This does not mean that the household overall does not benefit, since the increase in other productive activities, whether for home production or income generation, yields utility (as do any health benefits from safer water, of course). However, there will be no *specific* benefit (reduction in overall work hours) to an individual if her time savings are simply reallocated to other work. More to the point from a gender perspective, if women are not able to fully control their own use of time, the time benefits from water infrastructure investments may in effect by “appropriated” by other members of the household. [18] Hence a potentially more informative measure of individual time benefits would be the change in total work hours (or conversely, leisure time), not merely hours in water collection, from public water infrastructure improvements.

On the other hand, the main impact on women may be substitution to income-generating activities under their direct control, which may confer significant individual benefits. If this is the case, an increase in a woman’s labor supply or labor force participation would be considered a benefit, even if her leisure time stayed the same or fell. The following table illustrates the range of possible scenarios and their implications for the welfare of the individual from policies that reduce the time required for water collection.

[18] The problem is that benefits from infrastructure investments that are realized as reductions in time in certain activities of specific individuals can potentially be appropriated by others in the household, if the latter have the power to dictate the time allocations of household members. This is a specific form of the intrahousehold “flypaper effect” problem, in which benefits that might be targeted to specific individuals (women in this case) may not “stick” to these beneficiaries (Jacoby 2002).

Table 2.7 – Potential time use impacts and benefits of water supply investments

Scenario	*Hours in:* Water collection	All domestic work	Market work	Leisure	Benefit
(1)	↓	–	–	–	None
(2)	↓	↓	–	↑	Positive
(3)	↓	↓	↑	↑	Positive
(4)	↓	↓	↑	↓	?

Notes:
1/ ‘ – ‘ means no change in hours in the activity
2/ Leisure hours is defined as: 24 hours – Domestic work hours – Market work hours
3/ ‘All domestic work’ includes water collection

Unfortunately, when conducting a standard benefit incidence analysis, we are essentially restricted to using only the (inverse of the) time spent directly in water collection as the benefit measure. Based on the preceding discussion, this could be considered the ‘first-round’ time allocation effect, i.e., prior to reallocations of time savings.[19] We would expect this to be closely associated with variations in local water infrastructure. But we cannot similarly look at differences over expenditure quintiles or gender in leisure time or total work time since these outcomes are determined by many other factors in addition to local water supply; that is, differences across groups would tell us little about the benefits actually attributable to the public water investments.

The situation is different for demand analysis, where we are concerned with the effects of *changes* in water infrastructure indicators on time use. Here we have the option (if the appropriate data are collected) of considering how these policies affect each of the outcomes of interest: the time in water collection, in all domestic activities or all work activities, and in income-generating activities. The reason is that regression analysis allows us to estimate the independent effect of the infrastructure variable on these outcomes, controlling for other factors.

[19] This distinction between first round and subsequent time effects is a useful way to think about the issue but, we should note, is not completely valid theoretically. The infrastructure improvement lowers household’s price of (the opportunity cost of collecting) clean water; based on this, the household jointly allocates the time of its members to different activities, one of which is water collection. The change in the latter will depend, among other factors, on the price elasticity of demand for clean water.

What form would these regression models take? The first consideration is to identify the relevant policy variable. In rural areas, a plausible and common policy would be the construction of a well in a village. In urban areas it might be the extension of the water supply system so as to provide public taps, or possibly even indoor taps, to neighborhoods not currently on the water supply network. If a community survey collects information on the presence of these and alternative water sources, the analysis is straightforward: one would regress individual time in water collection (or in total domestic work, or leisure) on the availability indicators and other controls. Provided the presence of say, a well, can be regarded as exogenous to household decisions regarding time use—or alternatively, the regressions contain adequate controls for these preferences—the regressions indicate the effect of the public investment on individual time in the given activitie(s). [20]

Making a water source available in the community is in effect a reduction in the distance to that source, and likely a very large one: distance may have been very significant before, say if the nearest source of that type was in another village many kilometers away. A more refined analysis of the effect of distance would use a continuous measure, i.e., meters (or kilometers) to the source for each household. If the household survey collects information on the distance from the domicile to alternative water sources, we could estimate the impact of each of these distances on time allocation. These reduced forms would provide direct information on the effect of investments in, say, public taps in urban areas, that would reduce the average distance to such taps by a given amount. Further, we could also use these data to model the choice of water source as a function of distance and other factors using, say, multinomial logit. This might be important if the policy objective was to get households to use a safe water source (e.g. a well rather than a river): how close must this source be to the household to raise the probability of use by some targeted amount?[21]

With regard to the functional form of models of time allocation, for a continuous dependent variable such as weekly hours of leisure, standard linear regression is appropriate. For variables for which there are many zero values, such as time in water collection or in the labor market (since many households members will likely report no time in this activity) tobit or the two-step Heckman selectivity model will be more appropriate.

Unfortunately, existing surveys usually fall short of providing the variables we would like to have for this analysis. Community surveys often ask respondents to identify which water source is used by the largest number of inhabitants, but much less often collect information on the presence or location of different types of sources. Household

[20] Endogenous placement of water infrastructure is difficult to deal with. As an example of this phenomenon, communities where people are more concerned to reduce time burdens of water collection—perhaps those in which women have a greater say in community decision-making—may take the initiative to construct a well or lobby the local government to build one.

[21] Though it should be noted that this public health objective may conflict with the goal of reducing time burdens on women. For example, a reduction in distance to a covered well may lead to a switch to this safe source from less safe river water even if the distance to the well remains higher than to the river, so that there is an increase in time to the household's chosen water source.

surveys very often collect information on the type of water used for drinking (interior tap, outdoor tap, well, river or lake) and the distance to that source, but rarely if ever are the respondents asked how far away the *alternative* sources are from the domicile. This makes it much harder for econometric analysis to yield reliable estimates of the effects on time use of specific public investments in water infrastructure (e.g., wells, public taps).

To see this, consider the pitfalls of some possible approaches using the standard data, some of which appear in the literature. We might, for example, consider simply regressing time in water collection on a series of dummies identifying the source used by the household. However, the coefficients on these dummies will not correspond to the effect of public investments (construction projects that make the source more accessible) on the time of those who actually use the source. Nor will they indicate – and this is probably more relevant – the average effect for the community of providing the source. To see the former, assume that in a rural area that there are just two options, natural sources (e.g., lake) and wells, so the model includes a dummy for household use of a well plus a series of controls. To measure the effect on the water collection time of those who make use of the well, the appropriate counterfactual is the hours that the households now using wells would have to spend if they used the lake source. In general, this will be larger than the average time observed for those who do use the lake, if people tend to choose a source that is closer to them.[22] Hence the comparison of means of well users and lake users will tend to underestimate the change experienced by those who use the new source.

With respect to estimating the *average* time reduction for the communities in which wells are placed, the bias can go in either direction. To see how the average gains could be underestimated, consider a simplified example of a cluster with 4 households. Two are 'near' the well (300 meters away) and 'far' from the lake (600 meters); thus they use the well and each report a distance of 300 meters. The other two households are 300 meters from the lake and 600 meters from the well; they use the lake and report a distance of 300 meters. The difference in reported mean distance (hence also water collection time) for lake and well is zero (300 minus 300). Yet without the well, the first two households would travel 600 meters to the lake. The actual total distance reduction resulting from well construction is 1800 meters (total distance of all households to water if there is no well) minus 1200 meters (total distance traveled if there is a well) = 600 meters, or 150 meters per household. Hence the observed mean difference in distance or time to water sources, calculated only from those who use the source (equal to zero in this example), underestimates the average benefit of the addition of the new source.

[22] To see this, consider the case where the well and lake are far apart, and maintain the assumption that only distance matters for the choice of water source. If the two sources are far apart, households that are located near to (and choose) the well are those who are far from the lake: for them the counterfactual (distance to lake) is higher than for those who are observed to use the lake. Consider next the other extreme whereby the two sources are located in the same place: in this case, households using the well will by and large have been about the same distance from the lake as those who continue to use the lake. Therefore the counterfactual distance must be equal to or greater than the observed mean distance to lake of those using the lake.

Counterexamples where the bias is in the other direction are easy to come up with.[23] Obviously these problems affect not just regression estimates but also simple comparisons of mean distances using information on observed choices.

A quite different potential source of bias is due to the endogeneity of household preferences. Households with strong preferences for safe water would be willing to walk longer to covered wells or taps than the average household. In contrast, households or individuals with high preferences for leisure or non-domestic work activities may choose sources that are closer or more expensive. Therefore the association of certain outcomes (for example, women's self employment activity, or girls' school attendance) and household use of a particular water source does not mean that a public policy that makes the source available to more households would lead to those outcomes. This is especially relevant where the household can choose between sources that are timesaving but potentially costly (e.g., indoor taps in urban areas) and those that involve time costs but are nominally free. One solution around this and the source of simultaneity described in the previous paragraph is to use two stage methods to predict the household's use of a specific source. This will be discussed further below.

If there is information on distance to the household's water source, another appealing—but also potentially problematic—strategy would be to directly estimate the effect of this distance on time in water collection or other activities. This is a reasonable approach if there is just one water source available to the household, or if it can be assumed that distance is the *only* factor entering household choices about water source. Generally, however, households face a choice among alternatives that will be influenced by distance as well as water quality and possibly, price. The estimated effect of 'distance' to the chosen source reflects these joint decisions rather than the effect of distance alone. For example, if a public investment reduced the distance to wells (a safer source), a household may switch from using river water to well water even if the well remains further away than the river source. Without accounting for choice among alternatives, the estimated effect of distance to the chosen source is not very meaningful for policy—it does not tell us how reducing distance to a specific type of water source will affect time allocations. The same problem occurs if instead we use the community average distance as the distance variable: as the mean of observed distances to chosen sources, this variable is an underestimate of true average distances to all sources (for the reasons discussed above), and still does not capture the effect of providing a specific water source nearer to households.

In these two approaches using standard data there are problems with respect to simultaneity and/or our ability to specify an independent variable that measures a well-defined policy intervention. We want indicators of water supply infrastructure that are both exogenous and correspond to specific policy levers. Availability indicators, such as presence of a well in a village, would be appropriate, as noted. Even though surveys do

[23] For example, if all 4 households were 300 meters from the lake and the well was built 100 meters from the first household but more than 300m from the others. The only case where there is no bias is where the well is built near the lake, and this is the case where there will not be a reduction in mean distance to the closest source.

not often record this information, we can infer availability from the household data for the cluster as follows: if one or more households in the cluster reports using the source, it is 'available' to the cluster; otherwise, it is not available. Although not necessarily free of ambiguity[24], regression analysis using availability dummy variables constructed in this way seem to provide the most useful guidance for policy makers in the absence of more detailed information.

The estimates would indicate, broadly speaking, the effect on time use of investments in specific water supply infrastructure such as wells or public taps. Importantly, these are reduced form estimates that measure the overall 'program effect' of the intervention, counting both the change in hours experienced by those who make use of the new water source and the zero change for those who do not use it. Hence it is not the same (it will be less in absolute value) than the effect on time use of those who actually use the source in question. This second measure also can be estimated consistently with the same data. To do this, we can use the availability indicators as instruments to predict individual (or household) use of specific sources, and include this predicted variable in the time use regression.

In general, however, the reduced form approach will be of more interest from a policy perspective. Providing a well or public tap does not mean that all will use it, given variation in distance and preferences within a locale, and the assessment of local impacts of the investment on time use should reflect this. The second approach in contrast indicates the effect of a household actually using the source, controlling for the endogeneity of this choice. This may be of interest where the intervention is one that is designed to induce greater utilization of an existing source, for example by reducing user fees at public taps. In other cases, the intervention may be expected to result in all households in the community using the service—for example, the authorities may be contemplating hooking up each domicile to the water system—and in this case too the two-stage estimates (showing the effect of having an internal tap on time use) are of interest. Obviously the preferred approach will be a function of the context and policy considered. We take these issues up again in Section 6 where we conduct an empirical analysis for Uganda and Madagascar.

Finally, it is worth reiterating that better data would enable better analysis: researchers and policy makers concerned about the time burden facing women would do well to collect detailed information on local water infrastructure (availability, distance, cost) in community surveys, and distance and to alternative (not just the chosen) sources of water in household surveys. Household surveys, of course, also should collect detailed individual level data on time use in water collection and other activities.

[24] In particular, the number of households interviewed in each cluster must be large enough to insure (or at least make highly probable) that if some households in the community use a particular source, at least one such household ends up being randomly selected for the sample. In section 6 we discuss these issues in detail in our application using data from Madagascar and Uganda.

Appendix 2.1: Equivalence of Gender Gaps Calculated on a Per Capita or Per Target Population Member Basis

Using the example of school enrollments, the (proportional) gender gap in coverage for quantile j would be the girls' enrollment rate divided by that of boys:

$$r_j = \frac{\left(\frac{g_j}{G_j}\right)}{\left(\frac{b_j}{B_j}\right)} \approx \frac{g_j}{b_j}$$

where

- g_j is the number of girls in quantile j who are enrolled;
- G_j is the total number of girls in the quantile;
- b_j is the number of boys in quantile j who are enrolled;
- B_j is the total number of boys in the quantile.

The approximation is valid if $B_j=G_j$, which should be approximately true.

Now consider the relative comparison of quantile/gender shares as we defined them in Table 2.5:

$$\rho_j = \frac{\left(\frac{g_j}{g+b}\right)}{\left(\frac{b_j}{g+b}\right)} = \frac{g_j}{b_j}$$

where g and b are the total number of girls and boys enrolled in all quantiles, respectively. This and the first ratio, clearly, are approximately the same. Differences across quantiles will also be approximately the same for the two ratios.

Appendix 2.2: Comparing Impacts of Independent Variables on Male and Female Demands for Services[*]

As noted in the text, when probit or logit methods are used to estimated the discrete demand for services, the coefficient estimates do not show the impact of the covariates on the probabilities of using a service or choosing a particular provider. Instead these marginal effects must be calculated from the estimated parameters and the data. Because the change in probabilities are functions of the data and the full set of parameters, it is particularly important when making gender comparisons to compare male and female marginal effects rather than simply testing for significance in the interaction term.

To illustrate this, consider a probit model where the dependent variable *y* is a 0,1 indicator of use of the service. The estimated probabilities of using the service for individual *i* take the form:

$$\Pr(y_i=1) = \Phi(\beta' x_i)$$

where Φ denotes the standard normal cumulative distribution function, β is a vector of estimated parameters, and x_i is the vector of regressors for *i*. The marginal effect of a change in the *j*th regressor is the derivative of the probability with respect to x_j

$$\partial(\Phi(\beta' x_i))/\partial x_{ij} = \phi(\beta' x_i)\beta_j$$

where ϕ denotes the probability density function of the standard normal. The derivative is clearly dependent not just on the value of the coefficient on x_j (β_i), but on all the data and parameters. To allow for gender specific impacts, we can estimate separate probits for males and females, or equivalently estimate a single probit model on the pooled male and female sample in which each regressor, including the intercept, is interacted with gender dummies.[25] Consider a very simple example of the former, a model with just intercepts and a single policy regressor *x.* We have for females and males:

$$\alpha' x = \alpha_1 + \alpha_2 x$$
$$\beta' x = \beta_1 + \beta_2 x$$

Since the marginal effects depend on the data, to calculate them we have to choose the values for the regressors x. The large majority of studies reporting marginal effects evaluate them at the sample means of the data. As discussed below, this is not the only

[*] We thank Dominique van de Walle for particularly helpful comments on this appendix.

[25] Pooling with interactions would not yield the same model if the variances of the male and female equations were different. However, for identification purposes the binary probit is routinely normalized to have error variances equal to 1; analogously the binary logit model normalizes the variance to $\pi/3$ (see Maddala 1983). Given these normalizations, pooling with interactions and separate estimations yield the same results. This is also true for multinomial logit, but it is not true for nested logit or multinomial probit models, because these models estimate correlations among choice-specific error terms which will depend on the sample.

way this can be done and for several reasons it is advisable to test for differences in male-female marginal effects at different points in the distribution of the xs. However, for this exposition we assume the evaluations are made at the sample means of x for females and males (x_f and x_m respectively). Note first that this will yield the following predicted probabilities:

$$Pr_f(y=1) = \Phi(\alpha_1 + \alpha_2 x_f)$$
$$Pr_m(y=1) = \Phi(\beta_1 + \beta_2 x_m)$$

We are interested in whether the change in the probability with respect to x differs by gender. The marginal effects in ratio form are:

$$\frac{\partial \Pr_f(Y=1)/\partial x}{\partial \Pr_m(Y=1)/\partial x} = \frac{\phi(\alpha_1 + \alpha_2 x_f)\alpha_2}{\phi(\beta_1 + \beta_2 x_m)\beta_2}$$

A ratio greater than (less than) unity means greater (lesser) response of female demand than male demand. This expression makes clear why a simple comparison of the female and male parameters for the variable of interest, rather than of marginal effects, can be misleading. The relative marginal effects depend not just on the ratio of α_2 and β_2, the probit coefficients on x for females and males, but also on the ratio of the probability densities $\phi(\alpha_1 + \alpha_2 x_f)$ and $\phi(\beta_1 + \beta_2 x_m)$. Hence the marginal effects can differ even if the parameters α_2 and β_2 do not, because of differences in α_1 and β_1 or in x_f and x_m (and of course, in any male and female parameters or regressors in more elaborate right hand side specifications). For the same reason, the male marginal effect (say) can be larger than the female marginal effect even though α_2 is greater than β_2.[26]

Comparing marginal effects rather than parameter estimates is even more important when using multinomial logit or multinomial probit to estimate the choice among multiple alternatives. In these models, the probability of choosing alternative j depends on the data and the parameters in the utility functions for *all* of the choices. This makes the connection between parameters and changes in probabilities less direct than in binary choice models. If the coefficients of the utility functions are allowed to vary over alternatives, it is even possible for the coefficient on a variable to have one sign in the utility function for *j* while the change in probability of *j* has the other sign.

Many statistical software packages now routinely compute marginal effects for binary probit and logit models and (less commonly) for multinomial logit, nested multinomial logit, and multinomial probit models. For statistical comparisons of marginal effects for females and males one needs the estimated covariance matrices of the marginal effects. These can be derived using the delta method (see Deaton 1997). Let b represent the

[26] However, if the mean female and male probabilities are very close, the ratio of female to male coefficients on *x* will be a reliable indicator of the relative marginal effects at the data means: $Pr_f = Pr_m$ implies equality of the female and male index functions $\alpha_1 + \alpha_2 x$ and $\beta_1 + \beta_2 x$, hence also of $\phi(\alpha_1 + \alpha_2 x_f)$ and $\phi(\beta_1 + \beta_2 x_m)$.

(complete) Kx1 vector of parameters estimated from the model and say that we have used these parameters to construct a vector of *m* marginal effects, denoted ME(b). In a binary probit model, for example, these could be the derivatives of the probability with respect to the *m* regressors of interest. Let V_b denote the asymptotic covariance matrix of the *b*; this matrix is typically saved by the software after the estimation. Lastly, let G be an mxK matrix of partial derivatives of the marginal effects with respect to b: each row *j* contains the derivatives of the *j*th marginal effect with respect to each of the *b*s, i.e., $\partial(ME_j(b))/\partial b_k$, k = 1…K. Then the covariance matrix of the marginal effects is estimated by the following m-by-m matrix:

$$VME = GV_bG'$$

Now say we want to test whether the marginal effect of the variable x_j is different for males and males, that is, we want to test the hypotheses $ME_j^f - ME_j^m = 0$. We form the following statistic:

$$\frac{(ME_j^f - ME_j^m)^2}{Var(ME_j^f - ME_j^m)}$$

This is distributed as χ^2 with 1 degree of freedom, so we can test for equality of the female and male marginal effects by referring to the statistical tables for the χ^2 distribution. The denominator of this expression, the variance of the difference in the marginal effects, is calculated as

$$VME_{jj}^f + VME_{jj}^m$$

That is, as the sum of the *j*th diagonal elements of the female and male marginal effects covariance matrices VME^f and VME^m, each computed as just described.[27] Equivalently, when testing (as in this example) the equality of a single pair of marginal effects across gender, one can take the square root of the statistic above to yield the following (asymptotically) standard normal variable:

[27] In fact the general expression for the covariance of differences is more complicated: $Var(ME_j^f - ME_j^m) = VME^f{}_j + VME^m{}_j - 2cov(ME_j^f, ME_j^m)$. However, the last term, the covariance of the female and male marginal effects, is zero. This is because the female and male *b* vectors, from which the marginal effects are derived, have zero covariance, which in turn is because the *x*s for males and females are by construction uncorrelated. To see the latter, recall that estimating separate models for males and females is equivalent to pooling the samples and interacting each variable with gender. The covariance across gender of these interactions is always zero: when female*x_j >0, male* x_j =0, and the reverse.

$$\frac{ME_j^f - ME_j^m}{\sqrt{Var(ME_j^f - ME_j^m)}}$$

which can be treated as a standard t-statistic for testing whether the difference equals zero.

For econometric packages (such as STATA and Limdep) that provide standard errors to go with the marginal effects ME_j^f and ME_j^m, one can construct this statistic with a few additional calculations and no matrix manipulations. The $VME^f{}_{jj}$ and $VME^m{}_{jj}$ terms are simply the standard errors (squared) from the output tables for the marginal effects of the regressor x_j for females and males, respectively.

Things are a bit more complicated when testing for joint equality across gender of multiple marginal effects. For example, we might want to test whether the effect of several school quality variables on enrollment are jointly the same for girls and boys. For this the standard errors provided in output tables for marginal effects are not enough, since they correspond only to the diagonal elements of the covariance matrix of the marginal effects and it is necessary to account for their correlation, i.e. the off-diagonal elements, as well. This means that we need to work with the male and female VME matrices.[28] This will also be the case for evaluating the marginal effect of a covariate entered in polynomials. For example, if we include income and its square in the model, the change in probability with respect to income is a function of two parameters which have a non-zero covariance.

Say therefore that we want to test whether the impacts of *m* covariates are jointly equal for males and females. The χ^2 statistic for the test is the more general form of that given above:

$$MEDIFF*\text{inv}(\text{VAR}_{MEDIFF})*MEDIFF' \sim \chi^2(n)$$

MEDIFF is set up as a 1xm vector of differences in marginal effects ME_j^f - ME_j^m with respect to the covariates of interest. VAR_{MEDIFF} is the covariance matrix of these differences, or the sum of the appropriate m-by-m submatrices of VME^f and VME^m. The degrees of freedom, *m*, is the number of equality restrictions, that is, the number of marginal effects whose joint equality is being tested.

Several additional points should be made:

(i). The discussion above has been in terms of derivatives of probabilities. For discrete variables such as education level or presence of electricity in a health facility, we are

[28] We are unaware of any standard econometrics package that both calculates marginal effects and saves the full covariance matrix of the marginal effects. Therefore one has to construct the covariance matrix oneself as shown above.

interested in the difference in the predicted probability when the variable takes different values – in the case of dummy variables, 0 and 1. Derivatives in this case are not meaningful, though in practice they usually provide good approximations of the discrete change. The delta method for deriving standard errors is perfectly applicable for functions that are differences in probabilities, even though as seen the method involves differentiation and thus requires a continuous function. This is because the difference $P(Y=1|x_j=1) - P(Y=1|x_j=0)$ for a discrete regressor x_j is still continuous in *b,* which is what the delta method requires.

(ii). We noted earlier that it may be advisable to evaluate marginal effects and their differences not just at the sample means for males and females, but also at different values of the data. This is especially important if the index function of the model is specified to be nonlinear in key covariates, e.g., if income is entered in quadratic form or its effect is allowed to vary according to some other covariate through interactions. A quadratic specification for income means that the effect of income will depend on the level of income, hence so may the difference in female and male marginal effects. Clearly in this case one should test for gender differences in the marginal effects for different values of the income variable. Alternatively, one could divide the sample into quantiles of the income distribution and evaluate the marginal effects of income at the quantile-specific mean values of all the covariates. Since the poor differ from non-poor not just in terms of income but other factors as well (education, household size, distance to services) this approach can provide more directly relevant policy conclusions. It can show, for example, what the gender difference is in the enrollment impacts of a change in income for a 'typical' boy and girl in the bottom two quintiles of the income distribution.

Further, even if the index function of the probit or logit model itself is linear, the marginal effects are non-linear transformations of the index, as stressed above. This means that the marginal effects will depend on the level of x, as is already clear—but the additional implication is that differences in male and female marginal effects may also vary with x. This is another reason to test for differences in male-female effects at different values of the covariates, particularly those covariates we care the most about, e.g., income and price.[29] Again, this is just a matter of adjusting the values in the covariate vectors used in the calculations.

(iii). A different consideration is that evaluation of marginal effects at the sample means of the data is not the same as the calculation of the *average* marginal effect for the sample. The latter is obtained by calculating the marginal effect for each observation in the sample and taking the mean; this more accurately represents the average response of the population. In contrast, evaluation using the vector of data means (or alternatively, data medians) can be interpreted as the response of a 'typical' individual; analogously, as indicated above, computing marginal effects at the data means for the lower income quantiles gives the response of a 'typical' poor person but not the average response among the poor. The two measures diverge because—again—the derivatives are nonlinear functions of the values of the regressors. The extent of the divergence will be a

[29] In our application in section 5 we found that the patterns of gender differences in marginal effects did not notably vary when this was done, but this robustness should not be assumed.

function of the estimated parameters and the distributions of the regressors. In principle it is straightforward, though slightly more complicated, to calculate average marginal effects as well as their standard errors.[30] Presently, software packages that generate marginal effects generally do so only by evaluating them at the sample means of the data or at some other user-specified vector of data values, so to derive the average response measure requires additional matrix calculations on the part of the researcher.[31]

(iv). As noted, for binary probits and logits (and multinomial logit) one can interact all *x*s with gender and estimate the model on the pooled sample rather than estimating separate models for males and females. However, care must be taken in calculating the marginal effects in this case. If this is done at the overall 'sample mean' (the default in most econometric software packages) the results will be misleading. This is because the sample mean of a gender interaction term such as FEMALE*price will be approximately equal to 0.5*price as it is includes the zero values of this term for all the males in the pooled sample. This applies as well to less general models in which some but not all covariates in a pooled estimation are interacted with gender. As long as the software package permits the user to set the values of the covariates when computing marginal effects, this problem can be avoided by making sure the female-only data means (or other desired points on the x distribution) are used when calculating marginal effects and their variances for females, and the reverse for males. Estimating and calculating marginal effects on the separate samples of males and females avoids this problem entirely.

(v). With respect to interpretation, it is important to be clear that the comparison of marginal effects shows only whether demand responds differently by gender to a change in the policy variable being considered. This is not the same as testing whether 'preferences' for (say) boys' vs. girls' education differ, and in fact we cannot infer anything about the nature of utility functions from this exercise. This will become apparent in the literature review in section 3, which makes an effort to interpret empirical findings of differences in male and female demand responsiveness in conceptual terms. With respect to schooling, for example, these differences could easily be due to gender differences in marginal costs or returns -- or even in the variances of the utility functions – rather than to differences in preferences (i.e., in the utility function parameters).[32] Still, this does not render the comparisons any less useful from a policy perspective. On the contrary, they indicate whether a policy will have the same or different effects by gender

[30] With regard to the calculation of the standard errors, each row *j* of the matrix G is now a row of partial derivatives (with respect to the *b*s) of the sample mean of the individual marginal effects for covariate j; this mean of course is just an additive function of the individual marginal effects.

[31] Researchers deciding to use the sample average of the marginal effects should be aware that this measure can be sensitive to outlier values of the individual marginal effects.

[32] With respect to differences in utility variances, the probit and logit models do not separately identify both the parameters of the index function and its error variance. Rather, the betas are normalized on (divided by) the variance (see fn. 25); put another way, the parameter values must adjust to allow the variances to have the specified value, e.g., 1 for binary probit. Therefore a lower estimate of beta for females than males may simply be due to a larger variance in the utility of girl's schooling relative to boys'. Further, when we calculate and compare marginal effects based on the estimates, gender differences in these statistics will also reflect male-female differences in the mean values of the covariates and the values of all parameters, as noted.

on outcomes such as school enrollment and health care utilization, which is what policymakers presumably want to learn.

It may also be of interest to evaluate and compare male and female marginal effects for the same values of x rather than the gender specific values. Since differences in the xs explain part of the overall gender difference in responses, these calculations are less useful for understanding the potential gender-differentiated impacts of policies. But by controlling for gender differences in the explanatory variables, we come closer to a comparison of 'pure' behavioral differences in male and female demands. Even here, however, the differences may reflect just different error variances in utility from boys' and girls' schooling rather than differences in the structural parameters of the utility functions.

(vi). A final point. The foregoing has outlined the methodology for making statistical comparisons of female and male impacts of explanatory variables in non-linear models of the demand for services. It is fairly common, and certainly easier than the approach just described, to infer that a gender difference exists if a covariate has a significant impact on outcomes for one gender but not the other. For example, school cost may have a significant negative effect on girls' enrollment but not on boys'. It is important to recognize that these two results together do not support the statement "girls' enrollment is more sensitive to cost than boys' enrollment". Given that we are dealing with probability statements, this can only be inferred through direct statistical comparison of the impacts on girls and boys. It is entirely possible – and quite common in practice – for there to be no significant difference in these impacts despite having one impact significantly different from zero and the other not.[33] In our review of the demand literature in the next section, we are somewhat liberal with respect to including results of the kind just described, both because these types of 'casual' comparisons are common and because otherwise the literature comparing gender impacts would be even thinner than it is. Again, however, comparisons of female-male impacts need to be based on tests like those outlined in this section.

[33] To make this point clearer, note that finding no significant difference in female and marginal effects of x would be analogous in a linear regression framework to finding an insignificant coefficient on the term interacting gender and x.

3 Review of Existing Research

3.1 Benefit Incidence

There are many studies and reports that document differential benefits of public services for men and women, boys and girls, in developed and developing countries alike. There are somewhat fewer, but still quite a lot, of studies and reports that document differential benefits of public services across the expenditure distribution,[34] especially in developing countries. Reviewing either or both of these literatures is beyond the scope of this report.[35] Rather, our focus is the intersection of these two sets: analyses that consider differential benefits of public services by gender and across the expenditure distribution. That is, we are interested in describing how the gender gap varies across the expenditure distribution.

Because benefit incidence examines the share of different groups in the benefits from public expenditures, disaggregating by gender or other discrete categories is a straightforward extension of the standard method. We simply define a group by welfare quantile and gender (or quantile and ethnicity, quantile and area, etc.). Such disaggregation is useful insofar as it gives a clearer picture of inequities, defined by gender or welfare. For example, for a country with a known gender gap in public school enrollments, we might find that this gap is large for the poor but negligible for the rich. This information would lead policy makers interested in gender differences to focus on poorer students. Alternatively, policy makers interested in improving the equity of public services might find that the distribution of subsidies to girls is more equitable than that to boys, so that shifting subsidies to girls would improve the overall equity consequences of the budget.

Despite the ease with which the standard benefit incidence methods can be extended to include gender, the literature is sparse, remarkably so in light of the attention that benefit incidence by gender and by welfare have received individually. A search of published and publicly available research yields many assertions that studies of how gender differences in access to public services differ across the expenditure distribution *should be* carried out, especially in the gender-based budgeting movement (Cagatay, et.al., 2000, Budlender and Sharp, 1998, Elson, 1991 and 1998). But we have found only five studies that actually carry out a systematic analysis: the seminal works by Selden and Wasylenko (1995) in Peru and by Demery and his colleagues (Demery, et.al, 1995, and Demery, Dayton, and Mehra, 1996) in Ghana and Côte d'Ivoire; a study by Sahn and Younger (2000) for eight African countries; and a large international comparison study by Filmer

[34] For practical reasons, developed countries tend to use income to measure welfare, while developing countries use expenditures. (Expenditures are actually the more attractive variable in theory, since they are more closely related to permanent income.) Since our focus is developing countries, we will refer to the expenditure distribution when we want to rank people or households by welfare.
[35] For those interested in developing countries, King and Mason (2001) is an excellent synthesis of the literature on gender differences.

(1999) that uses 57 DHS samples from 41 countries.[36] Further, a review of World Bank Public Expenditure Reviews that were completed during the past two years finds only two, for Ghana and Malawi, respectively, that examine the correlation between gender and expenditure incidence.[37] Of these, only two, Demery, Dayton, and Mehra (1996) and the Ghana PER, looks at how the gender/expenditure incidence of a public service (education) has changed over an extended period of time.[38] So, while we review these studies here, the most important observation is that there appears to be a gap in the literature that calls for the sort of analysis that we undertake in Section 4 of this report.

All of the studies that we review focus on the education and health sectors. In part, this reflects the dominance of these two sectors in developing countries' social sector budgets, especially in poorer countries. The focus of existing survey instruments has also been on health and education. A few benefit incidence studies look at the incidence of expenditures on infrastructure (van de Walle, 1998 and 2003) or other social sector expenditures (Younger, 2002), but none of these also consider gender differences, or gender differences by income level. There are also a few studies of how public investments affect time use differentially by gender (Brown and Haddad 1995, Ilahi and Grimard 2000; Ilahi 2001), but as far as we know, no study examines such gender differences across the expenditure distribution.

3.1.1 Education

Demery, Dayton, and Mehra (1996) examine the incidence of public expenditures in Côte d'Ivoire. They find that girls receive 42 percent of public primary school subsidies, with girls in the first quintile receive only 33 percent of that quintile's subsidies, while girls in the top quintile received 54 percent. A similar pattern is found in Demographic and Health survey data for Côte d'Ivoire analyzed in the study by Filmer (1999), discussed below. Rather surprisingly, the same correlation is not found in public secondary school subsidies, where girls' share of subsidies is fairly constant across the expenditure distribution.

Demery et al. (1995) do a similar analysis for subsidies to public schooling in Ghana. They find that the share of girls in primary school subsidies is 47 percent, and 41 percent for secondary subsidies. These shares are fairly constant across the expenditure distribution, except for secondary schools, for which girls' share in the lowest quintile is only 30 percent. This too is consistent with DHS schooling data analyzed by Filmer (1999). Although these two country studies do not permit general conclusions, one insight is that we cannot simply assume that gender-income interactions operate the same way in all countries, even for neighbors. In Ghana, there is little such interaction for primary schooling, but some evidence for it at the secondary level. In Côte d'Ivoire, the opposite is true.

[36] A few more studies examine this question using demand analysis. We review those studies in section 3.2.
[37] We are grateful to Julie Taparia for conducting this review.
[38] Demery, et al., also look at incidence over time, but only for the three-year period between 1989 and 1992.

Filmer (1999) uses the Demographic and Health Surveys (DHS) to examine differences in school attendance rates for boys and girls and rich and poor. The DHS data do not include household expenditures, but they do include variables that allow Filmer to create an index of wealth – assets owned by the households – using principle components methods. Filmer then divides households into poor, non-poor, and rich, based on the value of this index. While this approach is obviously approximate, the great advantage of the DHS data is that they are available for many countries at many points in time, and the surveys are standard across countries and time.

Filmer finds that enrollments of boys 6-14 exceed that of girls in about 25 of the 41 developing and transitioning countries in his survey, though in some cases the boy-girl differences are small (and in several countries where boys are not favored, girls actually have a slight advantage).[39] This illustrates that a gender gap in schooling is a common phenomenon, but by no means one that occurs everywhere. Regional differences stand out: the female disadvantage is largest in Western and Central Africa, North Africa, and South Asia. With respect to how this gender gap interacts with wealth, in almost all the DHS countries where there is an *average* female disadvantage, the disadvantage appears to be larger among the poor than among the middle class or rich (but see the discussion in Section 3.2.1 below). In a number of countries where the overall gap is large, the interaction of gender and wealth is also large, leading to very significant female disadvantage among the poorest (Niger, Egypt, Morocco, India, and Pakistan).

Beyond these studies, the literature includes only occasional discussions of how gender shares in benefits vary across the expenditure distribution. Selden and Wasylenko (1995) give the share of all education subsidies by age group for Peru in 1985. They find that subsidies are evenly distributed across both gender and expenditure decile for children 7 to 12 years old, but for children 13 to 17 years old, the distribution favors boys and, surprisingly, their share is highest in the top deciles.

A recent Public Expenditure Review for Malawi (2001) gives primary school enrollment rates for boys and girls across the expenditure distribution.[40] They find that male enrollment rates are somewhat higher than female rates, but the difference is similar across the expenditure distribution and across time (between 1990 and 1997). This occurs despite a significant increase in enrollments between these two years, especially among children from poorer households.

Sahn and Younger (2000) give brief mention to gender/expenditure differences in their study of health and education benefits in eight African countries. Unlike all of the studies cited to this point, which present their results in terms of shares of benefits by expenditure quantile and gender, Sahn and Younger use cumulative shares of benefits across the expenditure distribution – concentration curves – because they have an

[39] This is taken from table 3 in Filmer, counting the latest DHS surveys for countries where there are more than one.
[40] Note that enrollment rates are not the same as benefit incidence. The denominator for the sum of beneficiaries is the "eligible" population – e.g. school-age children – rather than the entire population, as it would be for a benefit incidence calculation.

intuitive grounding in the theory of welfare economics (Shorrocks, 1983; Yitzhaki and Slemrod, 1991). In addition, these authors use statistical tests for differences in cumulative distributions rather than simply comparing central tendencies. Using the rather demanding statistical criteria that these tests require, Sahn and Younger find only one public health or education service in one country – primary education in Uganda in 1992 – in which the concentration curves differ significantly by gender. This does not mean that they find gender equality, but rather, that the degree of gender inequality is relatively constant across the expenditure distribution.

3.1.2 Health

Demery, Dayton, and Mehra (1996) find that overall, males and females receive about the same subsidy from public health clinics in Côte d'Ivoire, while females receive about 60 percent of the subsidy to public hospitals.[41] With the exception of the top expenditure quintile, the gender differences are fairly consistent across the expenditure distribution. For public clinics, men get between 52 and 59 percent of the subsidy received by the first four quintiles, but this falls to 40 percent in the top quintile. For public hospitals, women get between 62 and 68 percent of the subsidy for the first four quintiles, but this drops to 53 percent for the richest. So, while there is not a strong correlation of incidence in the dimensions of gender and welfare, there is clearly something distinct about the richest quintile.

In Ghana in 1989, Demery et.al. (1995) fine that subsidies to outpatient care received at public hospitals are split roughly evenly between males and females, with little variation across the expenditure distribution. For inpatient care, however, there are substantial differences, with only 22 percent of the subsidies received by the poorest quintile going to females, but 50 to 60 percent in the other quintiles. For health centers and clinics, females received 53 percent of subsidies in the poorest quintile, but 67 percent in the top quintile. This pattern is the reverse of the one observed in Côte d'Ivoire, where women's share of health subsidies declines as welfare increases. In 1992, the overall pattern of subsidies is similar to that in 1989, although females' share of subsidies to health centers and clinics is now fairly constant across the expenditure distribution, while their share in subsidies to hospital outpatient care now shows a mildly negative relation to quintile level.

There is no analysis of health care visits in the DHS data comparable to Filmer's (1999) paper on education. However, it is worth mentioning an analysis by wealth and gender of under 5 *mortality* using the DHS data (cited in World Bank, 2001 p. 63). In two thirds of the countries examined there is a declining ratio of female to male under 5 mortality rates as household wealth rises. Unlike school enrollments, in the majority of countries showing this pattern, mortality rates are actually lower for girls than boys at all wealth levels. This reflects (presuming that parents are not actually favoring girls) that female newborns are more robust than boys. Therefore in these cases it is the female advantage

[41] These data are for 1995. The paper does not include an intertemporal comparison of health services incidence.

that increases with wealth. These patterns in mortality should only be regarded as suggestive of what may be occurring with regard to gender and the use of public health services for children under 5, since there are of course other determinants of mortality than use of care.

3.1.3 Summary

It does not seem possible to draw any general conclusions from the existing literature about how the incidence of public expenditures on health and education varies by gender and expenditure levels. We have few studies, and they are not always in agreement. While it is true that the most comprehensive study, Filmer's (1999) analysis of the DHS data, does show that countries that have large gender gaps also tend to have larger gaps for the poor than the rich, that finding is based on differences in point estimates, not tests for differences. Filmer's own regression analysis finds far fewer cases in which a significant gender gap is accompanied by a significant decrease in that gap across the welfare distribution (discussed further in Section 3.2.1 below). Thus, rather than draw firm conclusions from the existing literature, we will defer our discussion to our own empirical work in Section 4.

3.2 Evidence of Differential Gender Impacts of Public Expenditure Choices in Education, Health, and Water Sectors

There are many dimensions to public policy in the social sectors and in infrastructure, going well beyond simply the determination of the level of expenditures in a given sector. These include pricing and subsidy policies, improvements in access through construction of new facilities, investments in provider quality, and schemes to increase the private sector's role in service provision. Sometimes by design and sometimes not, these policy choices will differentially affect utilization by men and women or by girls and boys. Where gender imbalances in access to services exist, knowledge of gender-differentiated policy impacts can provide guidance on which policy levers can be used to reduce the imbalances, or at least, on how to avoid exacerbating them.

In this section we review the evidence for impacts by gender of specific aspects of public policy in the education, health, and water sectors. Much of this evidence comes from studies that estimate the demand for services (or service-related outcomes) using large-scale household surveys, often combining these surveys with community level data on providers. A second source is program evaluations. In a few recent cases for education these are formal evaluations of policy experiments in which program interventions are randomly assigned to some communities and not to others, and differences in outcomes are compared statistically across communities. In many other cases, particularly for water projects, evaluation is less formal or is qualitative, consisting for example of monitoring the use of new facilities by villagers or interviewing women and men about changes that have occurred as a result of the project.

To gain some perspective, it is useful to relate the research reviewed in this section to conventional benefit incidence analysis. Standard benefit incidence is a descriptive tool that shows how current allocations of public expenditures benefit different groups in the population. We are also, of course, interested in how reallocations of public expenditures, additions to expenditures, or other changes in public policy, can improve incidence along the lines we are interested in, that is, make it more pro-poor and gender equitable. As we described in more detail in Section 2, conventional benefit incidence analysis provides some basic insights into how budget reallocations can improve expenditure incidence.

However, there are significant limits to what can be inferred about the distributional consequences of changes in public expenditures or policies –the *marginal* incidence – on the basis of information about average or current incidence. Policy changes or new public spending can take many forms, with varying effects on the utilization of services by poor and non-poor or by females and males. There is no reason to assume that the distribution of these marginal benefits will be the same as that of current benefits.[42] Given that uptake of education, health, and most infrastructure services in developing countries is generally voluntary, this is a matter not just of supply (the nature of the new public expenditures or policies) but of demand behavior as well. For example, poor households may be more responsive than non-poor to improvements in school quality; girls' enrollments may be more responsive than boys' to reductions in the distance to schools.

The research reviewed in this section directly addresses this issue by (in most cases) estimating the demand for services, disaggregated by gender, as a function of service cost, access, and other aspects of service delivery that reflect policy choices. Parameter estimates from demand models allow one to calculate, for example, the changes in girls' and boys' primary enrollment probabilities resulting from a halving of distance to school, or from an increase in the education level of teachers.[43] These analyses thus allow us to say something about marginal incidence – how different groups in the population would benefit from specific forms of public investment or changes in policy. From a gender perspective, as noted, it offers guidance as to how to direct new spending, or reallocate existing resources, to eliminate gender imbalances in the utilization of different services.

This review is structured as follows. In section 3.2.1 we consider education and health care demand. After looking at evidence on gender-income interactions in demands for these services, we review findings on gender differences in the effects of household resources, prices of and distance to services, and quality and other non-price aspects of

[42] Somewhat more technically, average incidence is valid as a guide to the distribution of new benefits only in the case of small changes in benefits that are proportional to existing benefit levels for the groups in question. There is no reason to assume these conditions to be satisfied for most expansions of public services.

[43] There is a caveat that applies to all the demand studies reviewed below. Excess demand for public services may lead to rationing of access (places in secondary school, appointments at hospitals, etc.). Then the estimated associations of provider characteristics and household demand are not a reliable guide to actual household preferences for these characteristics, since the behavior of some households will be constrained by rationing. This obviously will depend heavily on the specific context and service and will be especially problematic where the presence or level of the characteristic being considered is related to the degree of rationing.

services. For each case, we consider first demand studies using household (or other) survey data, then evidence based on program evaluations if these are available. We also assess the much more limited evidence on whether gender differences in the impacts of price and non-price provider characteristics differ over the income distribution. Although our main focus throughout is on empirical findings, where relevant we will also refer to theoretical frameworks that can aid in the interpretation of observed patterns.

Section 3.2.2 looks at gender impacts of investments in water infrastructure. Reflecting the relative dearth of empirical work on the effects of water investments, this section is much shorter than the previous one. Finally, a concluding section draws together the evidence from education, health, and water studies.

3.2.1 Education and Health Care

Among studies of the education and health sectors, the vast majority of research investigating gender differences in response to price and quality has been concerned with education. This is no doubt a reflection of the fact that where gender disparities exist, they are typically much wider for schooling than for health care. In these analyses, the outcome variable – education or health services 'demand' – is defined in various ways. Most commonly, and most pertinently for this analysis, indicators of current school enrollment or recent use of a health facility are used. In some cases the type of provider, e.g., public or private, is distinguished, and the analysis estimates the choice of provider. Other education outcomes are grade attainment and dropout. We will also refer at times to studies that consider achievement indicators, that is, performance on tests. This is not itself a measure of demand, but factors that affect girls' and boys' academic performance differently can also be expected to affect decisions to enroll girls and boys or keep them in school. In any case, learning outcomes are themselves benefits from education services – in fact they represent the benefit more directly than enrollments.

3.2.1.1 Gender-Income Demand Interactions

Before addressing the effects of sector policies, we take a slight detour that is nonetheless pertinent to the objectives of this study. A number of demand studies explore whether household income affects the use of education and health services differently for girls and boys (or women and men). Although in the short term the level of household resources is not generally a policy variable – successful direct transfer schemes are rare in developing countries – these analyses shed light on how income and gender interact to determine the distribution of the benefits from public expenditures. As this is the focus of our descriptive multi-country benefit incidence analysis, we are also interested in what econometric studies have had to say on the subject.

Demand studies typically show income or other measures of household resources to be positively and significantly related to the use of education and health services. This is not unexpected. Of more interest is the number of studies that find that investments in girls' education and health are more responsive to changes in income than investments in boys. In education, higher income elasticities of enrollment or grade attainment for girls have been found in very diverse settings: India (Sipahimilani 1999, Basu 1997), Malaysia (de Tray 1988), Peru (Ilahi 1999), Mexico (Parker and Pederzini 2001), Turkey (Tansel 1998), Tanzania (Mason and Kankher 1996), and Guinea (Glick and Sahn 2000). Schultz (1985) comes up with a similar pattern using national level time series data for some 90 countries over two decades, with gender specific enrollment ratios as outcome measures. The boy-girl differences vary considerably across the studies just enumerated but are often substantial: it is common to find coefficients on household income that are twice as large for girls as for boys.

The larger income elasticities for girls in these analyses imply that the gender gap narrows with income: boys become less favored. Recall that this was the story told by the descriptive data presented by Filmer (1999) for some, though not all, countries. However, other education demand studies do not find gender differences in income effects. In Kinshasa, Congo, Shapiro and Tambashe (2001) report that greater economic well being (measured by an index of consumer durable ownership) is associated with higher enrollments of both girls and boys with no clear pattern in gender differences across levels of well being. In some cases researchers have found gender differences in the opposite direction (larger or more significant effects for boys). This is seen in studies of rural areas in Pakistan (Alderman et al. 1997) and the Philippines (Bouis et al. 1998), both of which report a significant impact of household resources on boys' schooling outcomes but not girls'. Tansel (1997), looking at primary, middle, and post-middle schooling determinants in Côte d'Ivoire and Ghana, finds that, by and large, per adult household expenditures affects only girls' schooling in the former but only boys' schooling in the latter.

Therefore while a good deal of empirical research points to a stronger income effect for girls' education, it is by no means a universal pattern. Is this because the existence of gender differentials in the impact of household resources depends on the size (or presence) of the mean gender schooling gap (which varies greatly across these samples)? In the DHS data sets examined descriptively by Filmer it appeared that in countries where pro-male enrollment gaps existed, they tended to be smaller among the wealthier portion of the population than among the poor. In the multivariate studies just discussed, however, it is hard to discern any such pattern. Thus, for example, increases in household resources benefit girls' schooling more than boys' both in Guinea, where there is a large pro-male enrollment gap, and Peru, where there is not. Increases in resources have effects on boys' but not girls' schooling both in rural Pakistan, where there is a mean male advantage, and the Philippines, where there is a slight female advantage.

Further evidence on this comes, again, from Filmer's study. In addition to his descriptive analysis, Filmer estimates probit models of enrollment on the DHS datasets for children age 6-14 that include interactions of his wealth index with gender. As noted earlier, the DHSs are limited in terms of the number of relevant covariates they contain, in particular requiring that an index of assets be used in place of household income or expenditures to represent household welfare. On the other hand, they offer the strong advantage of forming a large and more or less 'representative' sample of 41 developing countries with highly comparable data from each of them.

Filmer finds significant gender-wealth interactions in slightly less than half (18) of the countries – but these cases are exactly equally divided between those showing that higher wealth benefits girls' schooling more than boys and those showing the opposite pattern.[44] However, closer examination shows that the countries where increases in wealth more strongly favored female enrollments also tended to have sizable average (pro-male) enrollment gaps, while most (7 of 9) of the countries where increasing resources brought stronger benefits to *boys'* enrollments had either no average gender gap or very small gaps in favor of one gender or the other. Therefore there is something of a pattern in these regressions using internationally comparable data that is consistent with the descriptive findings.

However, to say that where one finds increases in wealth or income benefiting girls' schooling more than boys one is also likely to see a large pro-male average gender difference is not the same as saying that wherever such pro-male gaps exist, increases in resources favor girls. Indeed, many of the DHS countries with large average gender gaps exhibit no significant interactions of wealth and gender in Filmer's regressions. Therefore both this multi-country econometric analysis and the total of the country examples discussed previously do not support a general claim that increases in household resources will tend to favor girls' education investments over boys'. There is at least some evidence, however, that this tends to occur more in contexts where the average or initial pro-male gender gap is large.

For health care – where relevant studies are far more limited – Alderman and Gertler's (1997) study for Pakistan finds income elasticities of girls' care that are 36-48 percent higher in absolute value than for boys' care. Interestingly, this is basically the opposite of the pattern for education seen in Alderman et al.'s (1997) study using what appears to be the same Pakistan data, cited above. Studies such as this one that estimate separate income effects for girls' and boys' (or women's and men's) health care are very rare. However, other researchers have considered the interaction of gender and income in the demand for health inputs such as calories or for nutritional outcomes such as weight or height. For Ghana, Garg and Morduch (1996) find that the effect of per capita income on the probability of being underweight is 50% larger for girls than boys, though there is no gender difference in the effects of income on being stunted (low height for age) or wasted (low weight for height). Several studies find that girls' or women's health status or caloric intake is more affected by seasonal variations in agricultural incomes (Behrman 1988) and by negative or positive weather shocks that affect agricultural incomes (Rose 1999; Hoddintot and Kinsey 2000).

Thus this rather more limited research in health indicates that as income rises, females may benefit relative to males in terms of health care usage or health outcomes. This is consistent with the pattern of a declining ratio of female to male under 5 mortality rates as household wealth increases found in about two-thirds of the countries in the analysis of DHS surveys noted in Section 3.1.2. Clearly, it would be useful to have more research on the health side about the impacts of family resources.

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[44]See Filmer (1999), Table 8.

Theoretical interpretations

While the evidence is not consistent, larger impacts of household resources on girls' education and health or health care are often found. Why would this occur? It is worth briefly considering theoretical approaches to this question because they will also be relevant for the interpretation of gender differentials with respect to other factors such as price or quality. One explanation is based on the common notion that parents view schooling and health care for children as investments that generate future benefits through higher labor market earnings. Parents invest in the schooling and health of girls and boys until the marginal costs are equal to the (discounted) expected marginal benefits in each case.

Poorer households, however, lack access to credit markets to secure funds for these investments and so are constrained by their own limited resources. This explains why poorer households invest less in children's education or health overall than wealthy ones, but not why such investments might favor boys at low levels of income. Parental investments will favor boys if the returns to investing in their human capital are higher, which could arise from labor market discrimination against women in hiring or pay[45] or because remittances from adult sons are greater than from daughters, or both. However, this is still not enough to explain why the bias toward boys would be greatest at low levels of income. This requires specifically that boy-girl differences in the marginal (not total) benefits relative to costs of human capital investments be larger for poor households.

Garg and Morduch (1996) show that this condition will be met under a combination of several plausible assumptions. One is that there is a standard concave earnings function such that the marginal impacts on earnings of human capital (health in their model, but equally applicable to schooling) is positive but declining in the level of human capital. Another is that the returns in the labor market for women are some fraction α of those of men; equivalent results would obtain if labor market returns were the same and α instead represented the ratio of daughter to son remittances out of their incomes. This specification implies that the marginal returns to investing in sons are always greater than for daughters, but as human capital increases the marginal returns fall more quickly for males so that the gender gap in marginal returns narrows.[46] Therefore where resource constraints bind so that children overall receive only small investments in human capital, the difference in marginal returns favoring males is relatively large: poor households

[45] With respect to education, the increments to earnings in the labor market from additional schooling—which should determine the allocation of family investment resources at the margin—are usually found to be as high if not higher for women as men, despite lower mean earnings for women (Schultz 2001). However, if parents expect that their daughters will not enter the labor force or else will work only intermittently, returns to daughters' education may remain below those of sons. Countering this, in turn, is the fact that schooling itself increases the likelihood of female labor force participation, hence lifetime earnings.

[46] I.e., the concave return function for males is represented by $R_m = aH_m - bH_m^2$ where H is the level of human capital and a and b are parameters. For females it is $\alpha(aH_f - bH_f^2)$. The marginal returns for males and females are, respectively, $\partial R_m/\partial H_m = a - 2bH_m$ and $\partial R_f/\partial H_f = \alpha a - 2\alpha bH_f$. The former declines more rapidly with increases in H.

invest significantly more in boys. At higher incomes more investment resources are available, the overall human capital of children is greater, and the boy-girl difference in marginal returns is smaller. Hence the allocation across genders begins to even out. A result similar to Garg and Morduch's could be obtained even if marginal returns did not differ for males and females, if there were differences in the marginal schooling cost functions for girls and boys. These would arise from assumptions (somewhat arbitrary) about the different technologies of production in the activities in which girls and boys engage, for example, household work for girls and farm work for boys.

Alternative explanations arise from a consumption perspective, i.e., that are based on parental preferences. Parents may view girls' education and health as more of a 'luxury good' than boys'. This too implies a higher income elasticity for the former. Or, 'inequality aversion' may be a normal good: at low incomes parents invest more in the human capital of boys (the marginal returns to boys are assumed higher in this explanation), but at higher incomes they increasingly allocate resources to girls as well because their desire for fairness in allocations increases (Garg and Morduch 1996). All of these explanations underscore the point that household demand behavior is an important determinant of service utilization and thus of benefit incidence, including, of course, incidence by gender.

3.2.1.2 Effects of Price/Distance

Evidence from demand studies

Looking now at the evidence of gender differences in the impacts of provider factors, we begin with the cost of services. 'Cost' is represented rather broadly in the literature: it is measured by price in some studies but in others is proxied by the distance to (or presence of) a provider. Many, even most, demand studies use distance rather than monetary measures such as fees to represent costs. Since in many developing countries public education and health services are nominally free, distance is frequently the only cost variable available in surveys. Distance is associated both with direct costs for transportation and with opportunity costs: longer distances to providers means more time traveling to and from the school or health facility, hence more foregone income or output from home, farm, or other productive labor. In some cases opportunity cost is expressed directly in monetary terms (and added to direct costs if these are also available). To do this, the time spent traveling to a provider (plus, for education, the estimated hours in school and for health the time at clinics if available) is multiplied by the relevant local wage or the individual's predicted hourly value of time based on a wage regression.[47]

There is a difficulty in interpreting most of these studies that applies both to those using actual price measures and those using distance variables: the reported price elasticities are

[47] While this procedure may be attractive conceptually, the use of wages to represent the opportunity cost of time is problematic in environments where most people do not participate in the labor market, as many researchers have noted (see Glick et al. 2000 for discussion). In such cases, predicted wages are likely to underestimate an individual's true value of time given the expectation that the decision not to enter the wage labor market is due to marginal productivity in self-employment or home activities being higher than the offered wage.

usually uncompensated, that is, they include the income effects accompanying a price change. For studies that use distance to proxy price, this is unavoidable. Distance does not have a money metric equivalent (unless the time spent traveling is multiplied by a measure of the value of time). Thus there is no way to net out the income effect of the 'price' change from the price coefficient. In contrast, for studies using monetary indicators of price, this is straightforward to do as long as the estimations also include an income term, but it has generally not been done. Hence where gender differences are found, we are left with some doubt about whether we are only observing differential price effects by gender, or whether we are instead (or in addition) really observing differential income effects, which as we have seen are often present.

Many of the studies that estimate demand models disaggregated by gender do find differences: girls' schooling or health demand is more sensitive to cost-–however defined – than boys'. Distance to school or the presence of a local school has stronger impacts on female enrollments than male enrollments in settings as varied as India (Sipahimalani 1999), Ghana (Lavy 1996), Malaysia (de Tray 1988), the Philippines (King and Lillard 1987), and Pakistan (Hazarika 2001).[48] In Kenya, higher school fees increase dropout probabilities for girls while having no effect on boys (Lloyd et al. 1998). Similarly, for Peru, using a cost measure that includes both opportunity costs and direct costs, Gertler and Glewwe (1992) find that price elasticities of enrollment probabilities for different school alternatives (local and distant schools) are consistently larger for girls. Finally, Schultz's (1985) study provides evidence from country level data. Using public education expenditures per teacher as a measure of the price of schooling, he finds greater price responsiveness of girls' than boys' education measured by changes in gender specific enrollment ratios.

This is an impressive array of country (and in one instance, cross-country) examples, but as in the case of income/gender interactions, the results of Filmer's (1999) comprehensive study caution against making general claims about gender differences in impacts. He uses information on the presence of local primary or secondary schools in the local community, available in 19 of the countries in the DHS sample, as indicators of access to schools. Controlling for other factors, including other local infrastructure characteristics, access usually strongly encourages school enrollment of rural children 6 to 14. However, in only four cases is there a significant difference by gender in this impact, three showing a stronger impact for girls and one showing a stronger impact for boys.

For those cases where girls' schooling is found to respond more strongly than boys' to changes in distance or local school availability, how appropriate is it to interpret this result as indicating that girls' schooling is 'more price-sensitive'? Though this

[48] This refers to Hazarka's finding that girls' primary enrollment probabilities, but not boys', are negatively affected by distance. He also reports the distance to middle school has a negative (though marginally significant) impact on boys' primary enrollment but not girls'. However, the effect of distance to middle schools may not be relevant for girls or may simply be difficult to capture in a regression, since in rural Pakistan few girls go on to middle school.

interpretation is often made, it may not be valid. Responses to reductions in travel time or distance to schools might differ by gender even if the impacts of a reduction in direct monetary costs, e.g., through a subsidy or a fee reduction, would not. For cultural reasons or because of safety concerns, parents may be reluctant to allow girls to walk to school on their own, in which case sending daughters to school may entail spending money on transportation, or else enduring psychological costs, that are not incurred for sons. Having a school in closer proximity thus can reduce costs for girls while having little or no effect on cost for boys. Further, if her labor is needed in the home, the value to the household of a girl's time may be higher than that of a boy's, in which case a reduction in the time required for traveling to school implies a greater decline in the cost of girls' attendance.[49]

For these reasons, reductions in distance to schools can mean larger *effective* reductions in the cost to households of schooling girls compared with boys, and thus can yield bigger gains for girls' education than boys' – even if a change in school fees or subsidies would have no gendered impacts. Some direct evidence on this issue is discussed below. However, this in no way lessens the relevance for policy of studies relying on distance measures that find such gender differences. They make clear that in such settings, rural school construction programs that reduce the average distance between home and school will have disproportionate benefits for girls' education. Further, they suggest that the process of urbanization, which among other things makes services such as education more accessible to households, should also have larger benefits for girl's schooling.[50]

Turning to the demand for health services, as indicated, relatively little research on gender differences has been carried out. However, a study of Pakistan by Alderman and Gertler (1997) using local provider price information finds that, consistent with the education analyses, price elasticities of health care utilization are substantially larger for girls. Further, the gender difference is greatest among the poor: price elasticities range from 58 percent higher for girls in the lowest income quintile to 14 percent higher in the richest quintile. Consistent with these findings (though not a study of health care demand) is another study from South Asia, Behrman and Deolalikar's (1990) analysis of nutrient consumption in India. They find that elasticities of nutrient intake with respect to food prices are generally larger for girls and women than for men and boys. In contrast to these two studies, an analysis of the demand for adult health care in Kenya (Mwabu et al. 1993) does not find statistical differences in the effect of distance on the probabilities of seeking care by men and women.

Evidence from program evaluations

A different source of information on how investments in children's human capital are affected by cost or availability, and whether this differs by gender, are evaluations of

[49] Since boy's labor is often important in other (e.g., family farm) work, the situation described in the text can be described more accurately as one where the marginal cost of time of girls in domestic work is higher than that of boys in these activities.
[50] Gender schooling gaps, especially at post-primary levels, do tend to be smaller in urban areas, though a variety of reasons in addition to greater accessibility are probably at play, such as higher average incomes and parental education and exposure to modern attitudes and female role models in the mass media.

interventions set up specifically to lower the costs to households of specific public services. Several such studies, all involving education,[51] are especially valuable because they were based on randomized policy experiments, or else "natural experiments," in the sense that the subsidy was in effect assigned randomly. An example of the latter is the study by Angrist et al. (2001) of Colombia's national voucher system for private secondary schooling, in which a limited supply of vouchers were assigned to qualified public primary students based on a lottery system (hence randomly). Voucher recipients performed at least modestly better in terms of school attainment and test scores. The positive effects, however, were larger for girls – even though neither boys nor girls were more likely to receive the subsidy.

A similar differential gender effect resulted from a very different demand side intervention, the PROGRESA program in Mexico. Rural communities were randomly assigned to receive this intervention, which was designed to raise primary school enrollment among poor children by providing education and food grants to mothers conditional on their children attending school and coming in for regular medical checkups (Skoufias 2001). As with the Colombia voucher program, PROGRESA served both boys and girls, though the subsidy was marginally higher for girls' enrollment. Also as in the Colombian case, while both boys and girls benefited, girls' enrollments increased more than boys' (Schultz 2000).

The evaluations of these subsidy programs appear to confirm that girls' schooling is more price elastic than boys – not merely more sensitive to distance. On the other hand, this was not found for the Food for Education subsidy program in Bangladesh, which offered households a monthly food ration conditional on a child's school attendance. In this case, as Ravallion and Wodon (2000) report, the program had statistically equivalent positive effects on primary enrollments for girls and boys. Still, the evidence overall, including from demand studies using fees or other monetary measures of cost, does suggest a tendency for the demand for girls' schooling to be more price elastic than the demand for boys' schooling.[52]

What explains this higher sensitivity to price? We should note, first, that each of the program evaluation studies is designed to estimate a full program effect. Hence the evaluations of these subsidy programs capture both the price (substitution) effect and the income effect of the price reduction brought about by the subsidy. The income effects may be stronger for girls, and as was noted with reference to the demand studies, this could explain part of the gender difference in observed price impacts.

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[51] We could not locate formal evaluations of analogous programs to reduce health care costs in which the evaluation disaggregated impacts by gender.

[52] There is also less rigorous, but often compelling, evidence from the experiences of some countries in which primary school fees were eliminated or sharply reduced. In Uganda, Tanzania, and Malawi in recent years, such policies resulted in sudden surges in enrollments, with girls' enrollments increasing the most (See Hertz and Sperling (2004) and references therein). Note however, that where fee elimination or other policies actually come close to getting all children enrolled--as in Uganda in the late 90s--it is inevitable that female enrollment will rise more if they were initially lower than male enrollments.

Assume, however, that the observed differences reflect true gender differences in price elasticities. The reasons for why this occurs may seem less evident than in the case of distance, where the difference could be ascribed to, among other factors, a higher value of time for girls. However, the economic framework discussed in the context of income/gender interactions offers some potential answers. Again resort is made to assumptions about the shape of either the marginal return functions or the marginal cost functions for schooling. Take the case where, as before, the marginal returns to boy's schooling exceed those to girls', but are falling more rapidly with increases in schooling. The household equates marginal costs of schooling to the expected marginal benefits for each case. With a reduction in price, the household seeks to restore this equality by allocating more resources to the schooling of both genders. However, since the marginal return is falling more quickly for males, this equality is reached with a smaller increase in schooling for boys than for girls. Hence the change in schooling for girls from the same reduction in price is larger than for boys. Alternatively, there may be no difference in male and female marginal return functions, but the marginal costs of schooling may be rising more rapidly for boys. Because of this difference in slopes of the cost curves, the increase in schooling which brings marginal costs into equality with marginal returns is smaller for boys than for girls.

Whichever conceptual framework explains the higher price responses of girls' schooling in the empirical studies, the results of these studies imply that interventions that reduce the monetary costs to households of enrolling their children, even if they do not single out girls for special treatment, may disproportionately raise female enrollments. Of course, the converse is also true: cost recovery schemes that raise fees will hurt girls' enrollments more. These implications carry over to the health sector, to the extent that the demand for nutrition and health services is more price elastic for females.

We have been considering interventions that alter the direct costs of education by the same amount for girls and boys. Other interventions, in contrast, have specifically targeted the cost of girls' schooling, i.e., they were designed to lower the cost to households of educating girls relative to boys. Where this approach has been implemented, it has been very effective at improving gender equity in schooling. An early example is the Bangladesh school stipend program, begun in 1982 to subsidize household expenditures on girls' secondary education. In the first 5 years of the program, girls' secondary enrollment rates in program areas rose by more than twice the national average, from 27 to 44 percent, (Bellew and King 1993).

Two other programs, both in Balochistan Province, Pakistan, were created to improve girls' access to local schools. One of these, the Quetta Urban Fellowship program, encouraged NGOs to build new primary school facilities in poor neighborhoods by paying a subsidy to the school – not to parents – for each girl enrolled. Enrollment growth of girls in the neighborhoods randomly selected to participate in the pilot project was 33 percentage points higher than in non-intervention neighborhoods (Kim et. al. 1999). Enrollment increased slightly for boys as well. Kim et. al. suggest that the sharp increase in girls' enrollments was in part an outcome of reduced distances to schools that would accept them.

The second pilot program, in rural areas of Balochistan, supported village organizations in setting up and operating separate primary schools for girls staffed by female teachers. Enrollment of girls rose 22 percent in program areas relative to other areas, while also rising 13 percent for boys (Kim et al. 1998). In this case, and in the urban program as well, the success of the programs presumably had a great deal to do with characteristics of the new schools that made them culturally appropriate for girls, not just with changes in price or access. These characteristics are discussed further below.

Are gender differences in response to price different for poor and non-poor?

This question is potentially important for policy. If girls are more sensitive than boys to an increase in provider costs *and* this difference is larger among poor households, cost-recovery policies will reduce utilization by girls more than by boys and by poor girls most of all. Conversely, subsidies that lower costs will benefit girls in poor households the most. If there is a gender gap in the use of a service and the gap is largest among the poor (a frequent though not universal pattern, as noted), such a subsidy will thus help to close the gap where it is the widest. However, very few studies have investigated this issue, even among those that estimate gender-disaggregated models (we address it in our own demand analysis in Section 5). [53]

One of the few that have is Alderman and Gertler's study of the demand for children's health care in Pakistan. As noted above, they find that the gender difference in response to fees is greatest in the bottom quintiles. Hence the demand for girls' health care would be reduced more than for boys' by a price increase, and the reductions would be proportionately largest for girls in the poorest quintile. Further, for any price level, Alderman and Gertler find that girls are less likely to see a doctor (the most common and highest quality source of treatment) when ill than are boys, and this difference is greatest among lower income households. Therefore a price increase, by reducing doctor's visits the most among poor girls, would serve to widen the gender gap in such care where it is greatest; conversely, a reduction in fees would reduce the gap where it is greatest.

Among the education studies cited above, only Gertler and Glewwe (1992) report gender-disaggregated price elasticities by income level. In contrast to Alderman and Gertler, their estimates indicate that although price elasticities overall are higher for girls, the decline in price elasticities as income rises is similar for boys and girls. However, because they do not estimate separate schooling models for boys and girls, Gertler and Glewwe's models allow for only limited flexibility in responses by gender.

3.2.1.3 Aspects of Service Delivery

The heading of this subsection refers broadly to all non-price (and non-distance) characteristics of service providers. It encompasses standard measures of quality –

[53] Note that since it is usually found that the poor are more sensitive than the non-poor to changes in prices (Strauss and Thomas 1995), what we are asking is whether price elasticities fall at the same rate for males and females as income rises.

number of qualified teachers in schools or doctors in health facilities, availability of blackboards or textbooks in schools and drugs and refrigeration in clinics, and so on. It also refers to factors that affect demand or education and health outcomes but that may not be considered to be measures of service 'quality', such as having more female teachers in a school. Almost all research on differential responses by gender to non-price aspects of education or health services has been concerned with education. In reviewing this evidence, we follow Lloyd et al.'s (1998) distinction between characteristics of the school environment that are the same for boys and girls but nonetheless may have gender-differentiated impacts on outcomes, on the one hand, and aspects of the school environment that are different for boys and girls, on the other. The latter may be the intentional outcome of policies to improve gender balance, as in a policy of supplying female teachers, or it can be largely unintentional, as in a prevailing negative attitude among teachers toward girls' education.

The evidence on quality impacts is more limited than on price impacts, reflecting the relative rarity of school or community surveys with good indicators of school quality (in contrast, price and distance variables can often be constructed directly from household surveys). Further, caution is almost always in order when interpreting estimates of the impacts of 'quality' from non-experimental data because of the difficulties in obtaining accurate measures of provider quality, noted in section 2.2.1.2. These indicators are very prone to measurement error, which would tend to lead to underestimates of their impacts, for both boys and girls. Local service quality may also be positively associated with unmeasured community level preferences for education and health, which would imply an upward bias in the estimated effects. Or, more or less the opposite of this, governments may purposely locate facilities or upgrade service quality where the population is disadvantaged or for other reasons is less likely to utilize the service; this would imply a downward bias in the estimates of the effects of quality on demand. It is not clear how any of these factors would affect *differences* in estimates of the impacts of quality for boys and girls.

That said, with respect to aspects of service delivery that are the same for girls and boys, there is some evidence that school quality affects the demand for girls' schooling more strongly than boys'. Both Khandker (1996) for Bangladesh and Lloyd et al. (1998) for Kenya find that increases in indicators of teacher quality raise girls' enrollments or reduce their dropout probabilities but have no effect on boys' schooling. In rural India, Dreze and Kingdon (2001) report that various measures of school quality have larger or more significant impacts on girls' primary enrollments than on boys'; the most impressive difference is in the impact of providing mid-day meals in schools, which raises the female enrollment probability by 15 percentage points.[54] King et al. (1999) find for Pakistan that merit-based grade promotions have greater impacts on girl's school continuation than boys' (though rather than an indicator of differential school 'quality' effects, this could reflect a selection process whereby relative to boys, only high achieving girl students get promoted for merit). In rural Pakistan, Hazarika (2001) finds that while having a local school with a water supply has similar effects on boys' and

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[54] As these authors note, the free meal is a form of education subsidy. The stronger effect for girls therefore may therefore be an indication of greater price responsiveness of the demand for girls' schooling.

girls' primary enrollment probabilities, the proportion of local schools with blackboards is positively associated only with girls' enrollments.

These findings come mostly from environments were girls on average suffer a significant disadvantage relative to boys in access to education. It is not clear whether this means that stronger female school quality impacts occur only in such countries or whether researchers have simply not bothered to pose the question for countries where there is no overall gender gap in schooling. Another question is why, in the cases studied, the demand for girl's schooling would respond more than boys' to changes in service quality that are apparently targeted equally to girls and boys. It is possible that such improvements somehow affect girls' ability to learn more than boys', inducing parents to enroll girls or keep them in school longer. This can only be inferred indirectly from the evidence since these analyses use enrollment rather than learning outcomes, and it is not clear why this would occur. It may be that that rather than representing a causal effect, better quality schools also feature better learning environments for girls (e.g., better qualified teachers tend to be more 'enlightened' and make efforts to encourage girls) that are not recorded in the data. Then at least part of the gender differential in benefits to quality actually reflects aspects of school that differ for boys and girls.

An alternative explanation for gender differentials in quality impacts, and one that does not require quality improvements to affect girls' and boys' learning differently, comes out of the investment model of parental decision-making. An improvement in school quality shifts up the marginal returns functions for boys and girls. If, as in the Garg-Morduch scenario outlined above, marginal returns fall faster for boys (the function is more negatively sloped), then the increase in schooling required to restore equality of marginal returns and marginal costs is larger for girls. The same outcome could also be generated by differences in the slopes of the male and female marginal cost functions. Therefore it is not necessarily the case that quality improvements that disproportionately increase girls' enrollments do so by having a greater impact on girls' ability to learn. This distinction, of course, may not be crucial for policy when the goal is precisely to raise female enrollments.

With respect to the impacts of aspects of schools that are *different* for boys and girls, there is little doubt that in many countries the school learning environment does differ in ways that strongly favor boys (World Bank 2001). Demand studies confirm that this has negative outcomes for girls' education. Lloyd et al. (1998) find that girls' dropout probabilities are significantly influenced by teacher attitudes about whether math is important for girls, by differences in the (self-perceived) abilities of girls and boys to seek advice from a school staff member, and by differences (again, self-perceived) in the treatment of boy and girl students. Also in Kenya, Appleton (1995) finds that girls' exam performance, unlike that of boys', is negatively affected by unfavorable teacher evaluations of their abilities.

Negative teacher attitudes toward girls and differential treatment of students based on gender are not, presumably, the result of explicit policies to target students on the basis of gender, though in essence they target boys for favorable treatment. Nevertheless, policy potentially can change these school or teacher factors, for example through more appropriate teacher training or gender-specific monitoring of student performance.

In some cultures, parents may be unwilling to enroll their daughters unless they can be taught by a female teacher, or may require that they attend girls-only schools. In Bangladesh, the presence of female teachers is found to positively affect girls' enrollments (Khandker 1996). This study found as well that having separate toilet facilities for boys and girls increases girls' enrollment and school attainment. Similarly, in cross-country regressions for Africa, Mingat and Suchaut (1998) find that having more female teachers is associated with higher enrollments and lower dropout rates for girls.

The estimates of the foregoing two studies are consistent with culturally determined parental preferences for female teachers for girls, though they may also have to do with girls responding better to female teachers or with female teachers being more sympathetic to female students. These latter two factors would induce parents to enroll girls by increasing the returns to doing so, that is, by improving girls' ability to learn. Consistent with this possibility are the results of a five country African study by Michaelowa (2001), who reports that girls' learning gains in the 5th grade are larger when they have a female teacher, while boys' are larger when the teacher is male.

Where there are cultural barriers to having girls taught by male teachers or to sending them to coeducational schools, investments in training female teachers or building separate schools for girls may strongly improve girls' opportunities for education. The pilot program in rural Balochistan mentioned above is an example of this strategy, in which villages were assisted in opening primary schools for girls, staffed by female teachers. As indicated, there were very significant gains in female enrollments in these villages compared to non-program villages. It should be noted that the treatment-control setup of this study, while providing strong confirmation of the value of the program overall, does not make it possible to disentangle the effects of its different elements, e.g., female teachers, girls-only schools, strong parental involvement, and the fact that distance to the nearest school was effectively reduced.[55]

For the health sector, only the Kenyan study by Mwabu et al (1993) explores whether provider quality affects demand for services differentially by gender. Interactions of gender and several measures of quality were not significant in their estimates of the demand for health care services by adults. However, most of their quality measures had no effect on *either* men or women, or had effects in the opposite direction than expected, so there is some question about the reliability of the provider data used for this study. Clearly, more research is needed on gender-differentiated responses to policies in the health sector, with respect both to price and non-price aspects of services.

[55] In part due to the success of these earlier projects, several similar pilot programs to raise female enrollments through targeted price subsidies, or subsidies in combination with efforts to improve the school environment for girls, have been implemented in countries such as Guatemala, Bolivia, and Tanzania. To date, formal assessments of these interventions have not been made available.

We note, finally, that none of the studies cited here interact income with provider quality in their models to see if poor households are more or less responsive than non-poor to changes in the quality of services. There has evidently been much less concern about this issue than about how price elasticities vary across income groups. It follows that the more refined distinction of looking at whether gender differences in response to quality vary by income has not been addressed either. Further, it would be of interest to see if aspects of service delivery that are purposefully designed to increase girls' participation – for example, providing female teachers in primary schools – have stronger effects on enrollments of girls from poor families than from non-poor ones. In sum, the education and health demand literature is not informative about the interactions of gender and income in determining the response to changes in cost or (even more so) the characteristics of services. Yet this information could be of value for policies that seek to close the gaps in access to services that exist between poor and non-poor, and between girls and boys or women and men.

3.2.1.4 Policies that Address the Opportunity cost of Girl's Time

The idea that girls' access to education is constrained by their domestic obligations is supported by both ethnographic studies (Nieves 1981; Safilios-Rothchild 1980; Engle et. al. 1985) and econometric demand analyses (Glick and Sahn 2000; Deolalikar 1998; Levison and Moe 1998; Pitt and Rosenzweig (1990). The latter show girls' schooling to be more negatively affected than boys' (if boys' is affected at all) by the presence of younger siblings, or in the case of Pitt and Rosenzweig, by the illness of a an infant brother or sister. This suggests that female schooling can be increased by interventions that directly address the opportunity cost of girls' time, in particular those that reduce the burden of childcare. The benefits to girls' schooling of providing subsidized childcare services may therefore be substantial. Unfortunately, while there are anecdotal accounts of community based childcare services freeing up girl's time for school attendance (see Hertz and Sperling 2004), there has been little rigorous analysis of the issue. Still, one well-conducted study for Kenya (Lokshin et. al. 2000) finds that lower local childcare center costs increase both maternal employment and girls' schooling (but not boys' schooling).

Girls may also benefit from flexibility in school schedules that help them balance domestic responsibilities and school. Flexibility could be provided by holding afternoon sessions for girls, or opening small satellite schools to be nearer to where girls (and boys) live and work. Often it is informal or community schools which offer these options. Although here too there is a lack of formal evaluations, descriptions of a number of such interventions suggest that they can significantly raise the school attendance of girls (Hertz and Sperling 2004 and Hertz et. al. 1991 discuss several examples). In some contexts (e.g., Pakistan's Balochistan Province, see World Bank 1996) offering later sessions for girls can work by dealing with a common cultural barrier to educating girls: double sessions make it possible for girls to attend school separately from boys.

3.2.1.5 Public Information Campaigns to Encourage Girls' Schooling

Where traditional beliefs make parents reluctant to send girls to school, it would seem that there are potentially large benefits to information campaigns that extol the benefits of educating daughters. Further, for poorly educated parents in particular, such campaigns may be well grounded in economic theory. They can supply information that these parents lack on many of the benefits to female schooling—for example, the effects on child nutrition. Because parents lack complete knowledge of the benefits, schooling is undersupplied from a social point of view.

It is quite difficult to assess the effectiveness of information campaigns. For one thing, they tend to be implemented in conjunction with other education policies (nation-wide or local), making it hard to attribute enrollment gains specifically to the information campaign. For example, in the case of Uganda's universal primary enrollment strategy of the late 1990s, promotion of primary school enrollments through the media was an accompaniment to more dramatic policy changes, notable the elimination of school fees. Community level policy experiments to assess the efficacy of mobilization programs are a possibility. Here these programs would be carried out in some communities and not others (or other policies plus mobilization would occur in some communities but only the other policies in others). This experiment, however, would evaluate only locally implemented information campaigns, so it would not be informative about other potentially vital ways the government can spread information, i.e., through national level mass media such as radio or television. In any event, claims have been made for the effectiveness of community based efforts to sensitize parents to the need to school their daughters. For example, Miller-Grandvaux et. al. (2002) note several African cases where such community education campaigns, in the context of community school development, were associated with large gains in female enrollment.

3.2.2 Water

Worldwide, gender disparities in the time burden of water collection activities are large. Evaluations of water supply projects therefore have increasingly had a gender emphasis. Of course, the primary – or at least, the traditional – goal of public investments to provide clean water supply is to improve the health of all household members. The health benefits of clean water, particularly for children, are well established in the epidemiological literature, and most econometric studies using household survey data show that access (variously defined) to clean water is associated with lower child malnutrition, morbidity, and mortality (Strauss and Thomas 1995).

However, very few researchers have used household survey data to assess gender specific time allocation impacts of public investments in water infrastructure, despite the availability of detailed time use data in a number of such surveys.[56] As discussed in

[56] We are considering here only how time-use effects vary by gender. It is conceivable that the health impacts of clean water accessibility also vary by gender, in particular, for girls vs. boys. This issue has not to our knowledge received attention in the empirical literature.

Section 2.3, it is not clear how best to define individual-level benefits from improved access to water supply. The first and most obvious measure is the reduction in the time an individual spends in water collection. But these time savings may be simply reallocated to other work activities, with the result that the individual does not receive any special benefit. We might then prefer to see the effects on her (and others') total hours of work, or conversely, leisure time. On the other hand, the main impact on women may be substitution not to leisure but to income-generating activities under their direct control, which may confer significant individual benefits. If this is the case, an increase in a woman's labor supply or labor force participation would be considered a benefit, even if her leisure time stayed the same or fell.

In a study using household data from Pakistan, Ilahi and Grimard (2000) measure local water infrastructure by the mean distance to water source among households in a community. They find that closer access reduces the time a woman allocates to water collection. Of more interest, they find that her time in income-generating activities increases while her overall (market plus home) burden of work falls, that is, her leisure time rises. Since both women's leisure *and* their time in market-oriented activities increase, it can be concluded that they do capture some of the benefits from the reduction in the time needed for water collection.

In contrast, using more limited data from Peru, Ilahi (2001) finds that having an in-house water source has no effect on total housework hours or total work hours overall (domestic and market-oriented) of either men or women, though it is associated with a reallocation of male time from wage work to self-employment.[57] Stated another way, his results indicate that time reallocations (between different uses of women's time, or between women's and men's time in different activities) insure that overall domestic and other work time of women does not increase when access to water is more difficult. This implies, contrary to what one might anticipate, that *lack* of a piped household water connection does not impose a (net) burden on women's time in this sample.

Project evaluations in the water sector are far more numerous. These have tended to be less quantitative than the education examples given above, instead relying on qualitative assessments and participatory evaluation methods. There is no study that we are aware of that is analogous to the experimental studies on education cited above, in which certain villages are randomly assigned an intervention and outcomes are compared with those in control areas. Nevertheless, gender has become a major focus of water infrastructure project assessments, which consider outcomes such as changes in women's time collecting water, female representation on water user committees and in project planning, and the percentage of local women using the new facilities. Most of the rural projects analyzed in the recent literature have been community-based initiatives, reflecting the increasing emphasis placed in the last decade on local 'demand-responsive' approaches

[57] However, as the author notes and as we discussed earlier (Section 2.3), such estimates are potentially contaminated by endogeneity, since having an in-house water source is likely to be simultaneously determined with household work/leisure decisions. Families with high labor supply and incomes are more likely to live in neighborhoods with piped water service.

to water supply investments.[58] A recent summary of 88 community-managed water supply projects in 15 countries (Gross et al. 2001) emphasizes the importance for sustainability and coverage of having women participate actively in both planning and management. This is logical, given that women typically will be the main users of the new facilities as well as having the most knowledge about the practicality of using alternative technologies.

Unfortunately, while this major evaluation study addressed differences between men and women in labor contributions for maintenance of the water facilities, it did not collect information on impacts on the overall time allocations of women and men. As stressed above, the latter is important for evaluating the gender-specific impacts of the projects: it would have been very helpful to learn about changes in total work burdens of women and men, and perhaps, whether women expanded their income-earning activities as a result of the projects. Similarly, in an earlier summary of World Bank supported water projects with a gender emphasis (Fong et al. 1996), for less than half of the projects for which monitoring and evaluation indicators were listed did these indicators include changes in the time use of women, either in water collection or more generally. Measured gender outcomes instead included factors such as the share of local women using the facility or female representation on management committees. The latter is an important indicator of female empowerment but is not very helpful for understanding benefit incidence, strictly defined.

We should also note that the conclusions from these evaluations are typically based on simple associations of outcomes and aspects of project design, and hence do not clearly establish causality from the latter to the former (something that Gross et. al. acknowledge in their study). If communities themselves choose whether to adopt gender-sensitive planning approaches, the community characteristics that determine adoption may also independently influence outcomes. If so, the importance of specific planning approaches may be overstated in the water project evaluation literature. Studies of water supply projects using data from control or comparison communities (i.e., non-intervention communities) would therefore be very useful.

3.2.3 Summary: Gender Impacts of Public Expenditure Choices in Education, Health and Water

Although there are some gaps in the literature – particularly in the research on health care and water infrastructure – several significant patterns emerge with regard to the impacts by gender of public investments in these sectors. Many studies find that girls' school enrollments are constrained more than boys' by distance to schools. This appears to occur both in countries where cultural factors would be expected to constitute strong barriers to girls' traveling from home to school and in countries where we might expect these barriers to be less operative. Public investments that increase the local availability of schools therefore are likely to disproportionately raise girls' enrollments.

[58] World Bank projects are summarized in Fong et al. (1996, App. 6); see also Gross et al. (2001).

There is also some evidence that girls' schooling – and possibly their use of health services – is more sensitive to changes in fees and other direct costs. Where this is the case, programs that subsidize households' schooling costs or that reduce the costs of using health facilities will also have larger benefits for girls than boys. There is (more limited) evidence as well that the demand for girl's schooling is more responsive than that of boys to improvements in school quality, pointing to another route through which policy may redress gender imbalances even while not specifically targeting girls.

However, where gender imbalances are very large and cultural barriers to female education remain significant, it may be more expedient to directly target girls' schooling. Several evaluations of gender targeting – through subsidies to girls' secondary education as in Bangladesh, or the construction of separate primary girls schools staffed by female teachers as in rural Pakistan – suggest that these approaches can be highly successful in reducing gender enrollment gaps. Other possible gender-based education policies include the training of more female teachers, the redesign of teacher training to improve attitudes toward female students, and the provision of separate school bathroom facilities for girls and boys. These changes would benefit girls' ability to get an education either by reversing aspects of the school environment that effectively favor boys' learning, or by making schools more acceptable environments for daughters in the eyes of traditional parents. Other gender-targeted policies that hold promise for increasing girls' access to education are flexible or double shift school sessions and the provision of childcare services, both of which address girls' typically very significant domestic work obligations.

We also considered in this section the interaction of gender and household income in the demand for education and health services. Many studies, especially in education, indicate that increases in household resources disproportionately benefit girls. A number of others do not, however, and a large multi-country study using comparable data (the DHS surveys) did not find this pattern. One might be able to say – tentatively – that pro-female resource effects, where they occur, tend to be in countries where girls suffer a large disadvantage on average. One cannot say, however, that in all cases where these gaps in schooling exist, increases in incomes will necessarily narrow the gaps. This conclusion based on the econometric literature is similar to that based on our review of descriptive studies discussed previously.

A more general point that emerged from this review is that one should be wary of making broad generalizations about differential female and male responses to policy and other factors. This was clearly seen for the role of income. It is true for the effects of distance as well: in contrast to the individual case studies examining the impacts of distance to schools, the same systematic study of DHS surveys just noted found little gender difference in the enrollment effects of school access. No similar study exists for other provider factors such as the quality of education or health care – no equivalent comparable multi-country data are available to conduct such a study – but clearly we need to be cautious in our statements about gender differences here as well. Ultimately, while the existing literature suggests some broad patterns, conclusions – and policies – for a given context need to be based on country-specific analysis.

Finally, in the water sector, the evidence from household surveys for the effects of improved access to clean water on women's and men's use of time is still very limited. Assessments of specific, typically community-based, water infrastructure projects show strong associations of project success (defined in terms of coverage and sustainability) with the incorporation of women in planning and management of the water resources. These projects evaluations, however, did not measure time allocation outcomes and are likely to some extent to be contaminated by endogeneity of program placement.

4 Benefit Incidence Analysis Results

4.1 Dominance Results

This section presents results of standard benefit incidence analyses for nine countries at two points in time each. We consider the incidence of public services in education, health (by type of facility), infrastructure, and vaccinations for childhood illnesses. In a few cases, we also consider the incidence of public employment, implicitly assuming that there is a transfer element to such employment. We focus on the two novel aspects of this report, namely, benefit incidence analysis by welfare level *and* gender, and changes in the distribution of benefits over time. The analysis produces an enormous quantity of descriptive information, and we will not present all of it here. Instead, we attempt to summarize the results as concisely as possible. Details are available in Annexes 1 and 2 to this report.

Table 4.1 – Country/year coverage of benefit incidence analysis

Country	Year	Survey
Bulgaria	1995	Bulgarian Integrated Household Survey
	2001	Bulgarian Integrated Household Survey
Ghana	1987	Ghana Living Standards Survey
	1992	Ghana Living Standards Survey
Jamaica	1989	Jamaica Survey of Living Conditions
	1999	Jamaica Survey of Living Conditions
Madagascar	1993	Enquete Permanente aupres des Menages
	1999	Enquete Prioritaire Aupres des Menages
Mauritania	1987	Enquete Permanente sur les Condiciones de Vie des Menages
	1995	Enquete Permanente sur les Condiciones de Vie des Menages
Pakistan	1991	Pakistan Integrated Household Survey
	1999	Pakistan Integrated Household Survey
Peru	1985	Encuesta Nacional de Hogares sobre Medición de Niveles de Vida
	1997	Encuesta Nacional de Hogares sobre Medición de Niveles de Vida
Uganda	1992	Uganda Integrated Household Survey
	1999	Uganda National Household Survey
Viet Nam	1993	Viet Nam Living Standards Survey
	1998	Viet Nam Living Standards Survey

As noted in our discussion on methodology in Section 2, the “benefit” that we use is a simple 0/1 indicator of service use. Thus, we identify who attends public school or who gets medical attention at a public facility, but we make no attempt to value those benefits in monetary terms. As such, the analysis identifies the distribution of beneficiaries across the per capita expenditure distribution and gender, not implicit or explicit monetary benefits.

4.2 Shares of Benefits by Quintile and Gender

We report tables of the share of benefits by per capita expenditure quintile and by gender, where the share is defined as the number of females (males) in quintile j who benefit from a particular public service divided by the total of both male and female beneficiaries. Because the quintiles are defined for the entire sample, the reference point for a "fair" share is 0.10,[59] since each group is approximately one-tenth of the relevant population.[60] We also report the gender gap as the share of female beneficiaries minus the share of male beneficiaries, by quintile and for the entire sample, and the average gender gap.

The complete set of results – 72 tables in all – can be found in Annex 1 to this report. As an example, consider Table 4.2 which presents each quintile/gender's share of students for Mauritania. The second and third columns of the first two blocks give the share of the specified quintile and gender in overall public primary school attendance. The fourth column gives the t-statistic for the difference between these two columns (males minus females), with results that are significant at the ten percent level shaded lightly, and those at the five percent level shaded darker. In this particular example, boys account for a larger share of public school attendance than girls in Mauritania in all quintiles and in both 1987 and 1995, but these differences are statistically significant for only the first and third quintiles in 1987 and the first and fourth quintiles in 1995.

[59] In the standard analysis, this reference would be 20 percent, because we calculate each quintile's share of (fe)male benefits using (fe)male beneficiaries in the denominator

[60] The actual reference might vary a bit if the number of males and females in each quantile differs, but it is unlikely that such variation is important.

Table 4.2 – Share of each quintile/gender in public school attendance, Mauritania

1987			
Quantile	Males	Females	t
1	0.061	0.028	**2.833**
2	0.061	0.050	0.902
3	0.118	0.079	**2.062**
4	0.141	0.107	**1.695**
5	0.183	0.173	0.299
Total	0.564	0.436	

1995			
Quantile	Males	Females	t
1	0.080	0.050	**4.373**
2	0.124	0.111	1.518
3	0.110	0.118	-0.962
4	0.136	0.112	**2.376**
5	0.093	0.066	1.536
Total	0.542	0.458	

Change between surveys

Quantile	Males	Females	M-F difference
1	1.485	**2.851**	-0.31
2	**5.132**	**5.377**	0.09
3	-0.551	**2.746**	**-2.28**
4	-0.279	0.327	-0.42
5	**-4.299**	**-4.776**	0.46

The second and third columns in the last block gives t-statistics for the change in each quintile/gender's share in attendance over time (later sample minus earlier sample). The share of benefits going to the poorer quintiles increased between 1987 and 1995, and this increase is statistically significant at the second quintile for boys, and the poorest three quintiles for girls. In the richest quintile, shares for both boys and girls fell significantly.

The fourth column of the last block gives the t-statistic for the change in the gender gap, defined as males' minus females' attendance share in the second sample minus males' minus females' attendance share in the first sample. The gender gap closed[61] in the first, third, and fourth quintiles, and widened in the second and fifth. But the only statistically significant change is in the third quintile, where girls' share increased relative to boys'.

The preceding discussion should make clear why we cannot review all 72 tables here. Instead, we provide a summary in which we add up the number of statistically significant t-statistics in the fourth column, by quintile and gender, for each public service considered.

[61] It is more accurate to say that it changed in favor of females. In cases like education, where the gap usually favors boys, "closed" is synonymous with "changed in favor of females."

4.2.1 Public Primary School

Table 4.3 shows that in 36 percent of the 45 quintile comparisons (9 countries x 5 quintiles) show significant gender gaps in favor of boys in the first survey year, rising slightly to 40 percent in the second survey year.[62] More strikingly, there are no cases in which a gender gap favors girls' attendance at public primary schools. Thus, while gender gaps in public primary schooling are far from universal, they clearly benefit boys when they exist. Most of the cases with significant gender gaps (12 of 16 in the first survey year and 11 of 18 in the second) are concentrated in three countries, Ghana, Pakistan, and Uganda. In addition, Viet Nam has a significant gender gap in three of five quintiles in its second survey (1998).

In contrast to the levels, most of the changes in gender gaps favor girls, although there are relatively few changes overall. These reductions in the gender gap are concentrated in Uganda (3) and Pakistan (2), two of the countries with the most significant gender gaps in levels. In Uganda, this is clearly related to the surge in enrollments associated with the universal primary education policy, while in Pakistan, it is associated with a large decline in boys' public (and overall) school enrollments. The two cases where there is significant change in favor of boys are in Jamaica, where there was a strong shift of girls to private schools (see the coverage results below). Thus, even though some gaps exist, they seem to be closing, albeit less than completely and less than universally.

Except for the fact that the richest quintile never shows a significant correlation of gender differences in shares, there is little correlation between the absolute gender gap and expenditures per capita except in Pakistan, where it is strongly negative in 1991, and slightly negative in 1999. Combined with the information on relative gaps from the concentration curves, this result shows that the gender gaps that exist in primary education are not robustly correlated with expenditures per capita. Significant reductions in the gender gap, however, are somewhat more likely to have occurred in the poorer quintiles.

Table 4.3 – Summary of statistically significant gender gaps in shares of beneficiaries for public primary school, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
1	5	0	4	5	0	4	1	3	5
2	3	0	6	3	0	6	0	2	7
3	5	0	4	4	0	5	0	2	7
4	3	0	6	6	0	3	1	0	8
5	0	0	9	0	0	9	0	0	9
Shares	0.356	0.000	0.644	0.400	0.000	0.600	0.044	0.156	0.800

Source: Annex 1

Note: The year for the first and second survey is not consistent across countries, nor is the lag between them.

[62] The t-statistics behind these tables are all calculated at the five percent significance level.

4.2.2 Public Secondary School

The gender gap for public secondary schooling tends to be somewhat larger than for primary schools, although Bulgaria, Jamaica, and Peru are exceptions where the gap is very small, or actually favors girls (quintiles 1 and 3 in Bulgaria in 2001). Still, the number of statistically significant differences is not much larger, 42 percent in both the first and second survey years. As with primary schools, there are relatively few significant changes between surveys, but all of those favor girls.

There is no clear pattern of differences in the secondary school gender gap across expenditure quintiles for the countries that do have a gender gap. Over time, the gender gap declines in almost all the countries that have one, except for Madagascar, yet few of these changes are statistically significant. (In Bulgaria, a gap actually opens up in favor of women.) In general, then, the story for public secondary schooling is similar to primary: the gender gaps that exist almost always favor males, but they are far from universal. Over time, these gaps are declining, but not by as much as one might hope.

Table 4.4 – Summary of statistically significant gender gaps in shares of beneficiaries for public secondary school, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
1	4	0	5	3	1	5	0	2	7
2	4	0	5	4	0	5	0	0	9
3	3	0	6	6	1	2	0	2	7
4	5	0	4	4	0	5	0	1	8
5	3	0	6	2	0	7	0	0	9
Shares	0.422	0.000	0.578	0.422	0.044	0.533	0.000	0.111	0.889

Source: Annex 1
Note: The year for the first and second survey is not consistent across countries, nor is the lag between them.

4.2.3 Public Post-Secondary School

These results can be somewhat erratic because most samples have relatively few post-secondary students. Even using a 10 percent critical value, there are fewer statistically significant gender gaps than there are for public primary or secondary schooling, even though the absolute differences can be quite large. Most of the gender gaps that are statistically significant favor men, as with primary and secondary schooling. Although there are proportionately a few more in favor of women (five), three of these are in Jamaica. Statistically significant changes in the gender gap usually favor females, as before. Unlike lower levels of public school, however, there are several cases in which there is a correlation between the gender gap and expenditures per capita. In most cases (Ghana in 1992, Mauritania, Pakistan, Peru in 1985), this is positive, which reflects that fact that very few poor people, male or female, attend university, while proportionately more males than females from the upper quintiles continue their studies beyond secondary school.

Table 4.5 – Summary of statistically significant gender gaps in shares of beneficiaries for public post-secondary school, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
1	3	2	4	0	1	8	0	2	7
2	2	0	7	3	0	6	1	2	6
3	2	1	6	3	1	5	0	1	8
4	3	0	6	2	0	7	1	1	7
5	5	0	4	4	0	5	0	2	7
Shares	0.333	0.067	0.600	0.267	0.044	0.689	0.044	0.178	0.778

Source: Annex 1

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.

2/ Statistical tests in this table are at the 10 percent level due to the small number of post-secondary students in most samples

4.2.4 Public Medical Visits

The gender gap for public medical visits favors women in every country and for virtually every quintile of the expenditure distribution within these samples. Yet as with the education results, only about one-third of these differences are statistically significant. There are so few statistically significant changes in the gender gap over time for public health visits that it seems fair to characterize the situation as static. There are also only a few cases in which there appears to be a correlation between the gender gap and expenditures per capita. Ghana in 1992 and Bulgaria in 2001 have negative correlations, that is, the female advantage is larger in the richer quintiles than in the poorer ones, while Jamaica in 1989 has a positive correlation.

Table 4.6 – Summary of statistically significant gender gaps in shares of beneficiaries for public medical visits, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
1	0	3	6	0	3	5	0	0	8
2	0	7	2	0	2	6	0	1	7
3	0	3	6	0	3	5	0	0	8
4	0	3	6	0	4	4	1	1	7
5	0	0	9	0	0	8	0	0	8
Shares	0.000	0.356	0.644	0.000	0.300	0.700	0.024	0.049	0.927

Source: Annex 1

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.

2/ Pakistan is included only in the first survey year data. No health visits were recorded in the 1999 survey.

One immediate response to these results is that they may reflect greater "need" for medical attention on the part of women, especially for reproductive, pre-natal, and post-natal care. It is difficult to judge the extent to which this is the case. On one hand, very few of the surveys that we use explicitly exclude these classes of care from the health questionnaire. On the other, all of them condition questions about health care with a question as to whether one has been sick or injured in a certain time period. To the extent that women do not consider reproductive, pre-natal, or post-natal care an "illness," they may answer "no" to the conditioning question and thus never answer questions about the type of institution they visited.

To try to avoid the problem of differential needs, we repeat the analysis for medical visits by children under 12 years old and for adults over 45 years old. In both cases, the vast majority of comparisons show no gender gap, and no change over time. Further, there are only a couple cases in which there appears to be any correlation between the gender gap in shares of public medical visits for these age groups and per capita expenditures. Thus, unlike education, it appears that shares of public medical visits are roughly equal for males and females at each quintile in these nine countries.

Table 4.7 – Summary of statistically significant gender gaps in shares of beneficiaries for public medical visits by children under 12 years old, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
1	1	0	8	0	0	8	0	0	8
2	0	1	8	0	0	8	2	0	6
3	0	0	9	2	0	6	1	0	7
4	1	0	8	1	0	6	0	0	7
5	0	0	9	0	0	8	0	0	8
Shares	0.044	0.022	0.933	0.077	0.000	0.923	0.077	0.000	0.923

Source: Annex 1

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.

2/ Pakistan is included only in the first survey year data. No health visits were recorded in the 1999 survey.

Table 4.8 – Summary of statistically significant gender gaps in shares of beneficiaries for public medical visits by adults over 45 years old, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
1	0	1	8	0	1	7	0	0	8
2	0	3	6	1	1	6	0	1	7
3	0	1	8	0	1	7	0	0	8
4	0	2	7	0	1	7	0	0	8
5	0	0	9	0	0	8	0	0	8
Shares	0.000	0.156	0.844	0.025	0.100	0.875	0.000	0.025	0.975

Source: Annex 1

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.
2/ Pakistan is included only in the first survey year data. No health visits were recorded in the 1999 survey.

4.2.5 Public Vaccinations

Benefits from public vaccinations for childhood disease are almost always very similar for boys and girls, across the expenditure distribution, and this pattern does not vary over time. There are no cases in which there is a noticeable gradient across the expenditure quintiles. Thus, as with public health care visits, public vaccinations are shared equally between boys and girls at each expenditure quintile.

Table 4.9 – Summary of statistically significant gender gaps in shares of beneficiaries for public vaccinations, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
1	1	1	5	2	0	5	0	0	7
2	0	0	7	0	0	7	0	0	7
3	3	0	4	0	1	6	0	0	7
4	0	1	6	0	0	7	0	0	7
5	0	0	7	0	0	7	0	0	7
Shares	0.114	0.057	0.829	0.057	0.029	0.914	0.000	0.000	1.000

Source: Annex 1

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.
2/ The precise variable is having had any one vaccination, although this is highly correlated with having had all vaccinations.
3/ These results do not include Bulgaria or Peru, for which no vaccination data were collected.

4.2.6 Public Employment

With the exception of Bulgaria, there is a gender gap in public employment in every sample that we study. Further, this gap is larger than any other that we observe, and is much more often statistically significant. The gap does tend to close over time, but by very little, and usually not by a statistically significant amount. The two exceptions are

Uganda and Viet Nam, where the gap actually increased (but not significantly). In Viet Nam, this was concurrent with a fairly substantial decline in overall public employment. Unlike most of the other benefits that we study, there is almost always a clear increase in the gap as we move up the expenditure distribution. As with higher education, the explanation for this is that few poor people have public jobs, regardless of gender.

Table 4.10 – Summary of statistically significant gender gaps in shares of public employment, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
1	4	0	3	5	0	1	2	0	4
2	5	0	2	4	0	2	0	0	6
3	5	0	2	5	0	1	0	0	6
4	5	0	2	5	0	1	0	1	5
5	5	0	2	4	0	2	0	1	5
Shares	0.686	0.000	0.314	0.767	0.000	0.233	0.067	0.067	0.867

Source: Annex 1

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.
2/ These results do not include Jamaica, Madagascar, and Pakistan (1999 only), for which no employment data were collected.

Having a public sector job is, it should be obvious, not quite equivalent to receiving a benefit such as health care or education.[63] Further, lower rates of female public employment (at any quintile) reflect in part lower female participation in the formal sector. With the formal sector, women typically are more likely to be found in government employment, suggesting that gender discrimination in hiring is less operative there. Still, if we are willing to consider public jobs as a form of public expenditure 'benefit', this is an example of a clear male advantage.

4.2.7 Water

Table 4.11 shows the quintile shares by gender for time spent collecting water. Recall that we wish to interpret this as a "bad" that could be alleviated with public infrastructure spending. There is a large gender gap in both countries, especially Madagascar, where it is by far the largest gender gap among the items that we have examined, across the expenditure distribution. In Uganda, the gap is similar overall to the gap in public sector employment, although larger in the poorest quintile and smaller in the richest. Somewhat surprisingly, the share of time spent collecting water is larger in the richer quintiles than the poorer in Madagascar, for both men and women. In Uganda, the share falls with expenditures for women, but even here the decline is not very large.

[63] That is, one's salary is a payment for work rendered, not a benefit. Still, there is often—even typically—a premium to public sector employment over private employment either in terms of pay or non-pecuniary benefits. Such rents represent income transfers (or the provision of free services) to public employees and would indeed be on par with other types of public sector benefits.

Table 4.11 – Quintile shares of time spent collecting water, Madagascar (1993) and Uganda (1992)

	Madagascar				Uganda			
Quantile	Male	Female	Difference	t	Male	Female	Difference	t
1	0.035	0.150	-0.115	-18.84	0.060	0.158	-0.098	-18.71
2	0.029	0.144	-0.115	-20.18	0.062	0.148	-0.086	-16.45
3	0.036	0.173	-0.137	-20.49	0.067	0.140	-0.073	-15.02
4	0.041	0.178	-0.138	-19.75	0.069	0.128	-0.059	-12.50
5	0.052	0.161	-0.110	-8.83	0.063	0.106	-0.043	-4.29
Total	0.193	0.807	**-0.614**		0.320	0.680	**-0.359**	
Test for equality of absolute gender gap across all quantiles								
	Chi-square	alpha			Chi-square	alpha		
	12.7	0.013			39.8	0.000		

4.3 Coverage Rates by Gender and Expenditure Quintile

The benefit incidence results above are based on how users of public services are distributed among the entire population. This is useful as a guide to understanding how well expenditures on public services redistribute resources to the less well off, but it does not tell us the extent to which public services reach the target population. To make this assessment, this section looks at the coverage of various public services, i.e. the number of beneficiaries divided by the target population, disaggregated by expenditure quintile and gender. As noted in Section 2, coverage tables can also convey information about distribution, in particular, how the relation of benefits to target population (or ‘needs’) varies over the income distribution. Also, unlike the previous section, we consider coverage at both public facilities and at all (public and private) facilities, to check whether a given quintile/gender has low public coverage but high overall coverage because it uses private services. For services in which the target population is not the same as the entire population, e.g. education, we also examine both net and gross enrollment rates. Net rates are defined as the number of beneficiaries from the target population divided by the target population. For example, for primary education, the rate is the number beneficiaries of primary school age divided by the number children of primary school age. Gross rates are defined as all beneficiaries divided by the target population. Our aim is to look for correlations of coverage rates with welfare and/or gender. As with the benefit incidence analysis, the breadth of the study generates a large amount of output, which can be found in its entirety in Annex 2. Here, we attempt only to summarize those results, drawing out plausible generalizations.

4.3.1 Primary Education

There are relatively few differences in net primary enrollment rates between boys and girls in our nine countries. Only about one-fifth of the per-quintile difference between boys' to girls' net enrollment rates are positive and significant in public schools, and one-quarter in all schools. Further, half of these differences are found in just one country, Pakistan. Unlike the quantile shares, there are a few cases in which girls' net enrollment rates are actually significantly higher than boys'. There are also relatively few significant changes in the male-female gap over time, but all of these favor girls, and they are disproportionately found in the poorest quintile.

Table 4.12 – Summary of statistically significant gender gaps in net enrollment rates at primary schools, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
				Public Schools					
1	4	0	5	2	1	6	0	4	5
2	3	1	5	1	1	7	0	1	8
3	1	1	7	1	1	7	0	0	9
4	1	0	8	2	0	7	0	0	9
5	1	0	8	2	0	7	0	1	8
Shares	0.222	0.044	0.733	0.178	0.067	0.756	0.000	0.133	0.867
				All Schools					
1	4	0	5	2	1	6	0	4	5
2	3	0	6	2	2	5	0	1	8
3	1	1	7	1	1	7	0	0	9
4	2	1	6	2	0	7	0	0	9
5	3	0	6	4	0	5	0	1	8
Shares	0.289	0.044	0.667	0.244	0.089	0.667	0.000	0.133	0.867

Source: Annex 2
Note: The year for the first and second survey is not consistent across countries, nor is the lag between them.

Gross primary enrollment rates are more likely to differ by gender: about half of the per-quintile comparisons show statistically significant differences in favor of boys. But, as with net enrollments, there are a few cases of differences favoring girls in both public and all schools. The most notable differences between gross and net enrollments are in Africa, where it seems that many more boys who are "too old" for primary school are enrolled than are girls, a pattern that we see throughout the expenditure distribution. Over time, gross enrollments usually increase, though this is not universally true, nor is it necessarily a good thing if it reflects delayed entry to school or a high propensity to take too long to finish. Viet Nam is a notable exception, where gross primary enrollments fall across the expenditure distribution, while net primary and secondary enrollments increase significantly, a pattern that reflects an improvement in the likelihood that children (of both sexes) begin and advance normally through their schooling. There are relatively more cases where the change in the gender gap is statistically than is the case for net enrollments or for quantile shares. In about one-quarter of cases, girls' gross enrollment rates improve relative to boys', and in a few cases, the opposite is true.

Table 4.13 – Summary of statistically significant gender gaps in gross enrollment rates at primary schools, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
					Public Schools				
1	6	1	2	6	1	2	2	4	3
2	4	0	5	4	1	4	0	3	6
3	5	1	3	4	3	2	0	4	5
4	4	0	5	3	1	5	1	1	7
5	6	1	2	3	0	6	0	1	8
Shares	0.556	0.067	0.378	0.444	0.133	0.422	0.067	0.289	0.644
					All Schools				
1	6	1	2	5	1	3	2	5	2
2	4	0	5	4	2	3	0	2	7
3	5	2	2	4	2	3	1	2	6
4	5	1	3	4	1	4	2	2	5
5	6	1	2	3	0	6	0	1	8
Shares	0.578	0.111	0.311	0.444	0.133	0.422	0.111	0.267	0.622

Source: Annex 2

Note: The year for the first and second survey is not consistent across countries, nor is the lag between them.

4.3.2 Secondary Education

Rather surprisingly, the gender gap is not much more pronounced in secondary than primary enrollments, at least as measured by our summary of statistically significant differences. (The actual differences do seem to be larger, but so are the standard errors.) Boys' net enrollments are significantly larger than girls in less than half of the per-quintile comparisons for both public and all schools, but girls' enrollments are significantly larger for only one and two cases, respectively. For gross enrollments, about half of the comparisons favor boys, whereas girls' enrollments are significantly larger for only two and three cases. Further, while girls' enrollments often increase by more than boys' over time, the difference is statistically significant in fewer comparisons (all favoring girls) than was the case for primary enrollments, for both net and gross rates. There is no clear correlation of either absolute differences or relative changes over time with the expenditure distribution.

The cases of significant gender gaps in favor of boys are highly concentrated by country. Ghana, Pakistan, and Uganda have significant gender gaps in all five quintiles in both surveys for both net and gross secondary enrollments, accounting for the vast majority of the significant differences observed in Table 4.14. In the other countries, the differences are much less dramatic.

Table 4.14 – Summary of statistically significant gender gaps in net enrollment rates at secondary schools, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
					Public Schools				
1	4	0	5	2	0	7	0	1	8
2	3	0	6	3	0	6	0	0	9
3	4	1	4	3	0	6	0	0	9
4	3	0	6	2	0	7	0	1	8
5	6	0	3	2	0	7	0	2	7
Shares	0.444	0.022	0.533	0.267	0.000	0.733	0.000	0.089	0.911
					All Schools				
1	4	0	5	3	0	6	0	1	8
2	4	0	5	4	0	5	0	0	9
3	4	1	4	3	0	6	0	0	9
4	3	0	6	1	0	8	0	1	8
5	6	0	3	2	1	6	0	2	7
Shares	0.467	0.022	0.511	0.289	0.022	0.689	0.000	0.089	0.911

Source: Annex 2

Note: The year for the first and second survey is not consistent across countries, nor is the lag between them.

Table 4.15 – Summary of statistically significant gender gaps in gross enrollment rates at secondary schools, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
				Public Schools					
1	4	0	5	3	0	6	0	1	8
2	3	0	6	4	0	5	0	0	9
3	5	1	3	5	1	3	0	2	7
4	4	0	5	3	0	6	0	2	7
5	6	0	3	4	0	5	0	1	8
Shares	0.489	0.022	0.489	0.422	0.022	0.556	0.000	0.133	0.867
				All Schools					
1	4	0	5	3	0	6	0	2	7
2	3	0	6	4	0	5	0	0	9
3	4	2	3	5	1	3	0	2	7
4	4	0	5	3	0	6	0	1	8
5	6	0	3	6	0	3	0	3	6
Shares	0.467	0.044	0.489	0.467	0.022	0.511	0.000	0.178	0.822

Source: Annex 2

Note: The year for the first and second survey is not consistent across countries, nor is the lag between them.

4.3.3 Post-Secondary Education

We should preface our comments on enrollment rates in post-secondary education by noting that the sample sizes are invariably quite small. Indeed, there are several quintiles in which there are no post-secondary students in these samples. Mostly for this reason, there are few statistically significant differences by gender for either net or gross post-secondary enrollments in either public or all schools, even though in most cases where enrollments are positive, they are larger for men than for women, sometimes substantially so. One contrast with lower levels of education, however, is in the changes in gender gaps over time. While there are relative few cases of significant changes, for net enrollments, they are almost as often in favor of males as of females. Another contrast is that the number of significant gender gaps in favor of males does increase somewhat with expenditure quintiles. This is because very few men or women from the poorest quintiles are enrolled in post-secondary school in these countries, while at the higher quintiles, men are more likely to study than women.

Table 4.16 – Summary of statistically significant gender gaps in net enrollment rates at post-secondary schools, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
				Public Schools					
1	1	1	7	0	0	9	1	1	7
2	4	1	4	2	0	7	2	1	6
3	2	1	6	1	0	8	0	1	8
4	3	0	6	2	0	7	0	0	9
5	3	0	6	2	0	7	0	1	8
Shares	0.289	0.067	0.644	0.156	0.000	0.844	0.067	0.089	0.844
				All Schools					
1	1	1	7	0	0	9	1	1	7
2	3	1	5	2	0	7	2	1	6
3	2	1	6	1	0	8	0	1	8
4	2	0	7	2	0	7	0	0	9
5	3	0	6	3	0	6	0	0	9
Shares	0.244	0.067	0.689	0.178	0.000	0.822	0.067	0.067	0.867

Source: Annex 2

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.

2/ Statistical tests in this table are at the 10 percent level due to the small number of post-secondary students in most samples.

Table 4.17 – Summary of statistically significant gender gaps in gross enrollment rates at post-secondary schools, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
				Public Schools					
1	2	0	7	1	0	8	0	2	7
2	4	0	5	3	0	6	1	1	7
3	3	0	6	5	0	4	1	0	8
4	3	0	6	3	0	6	0	1	8
5	6	0	3	4	0	5	0	2	7
Shares	0.400	0.000	0.600	0.356	0.000	0.644	0.044	0.133	0.822
				All Schools					
1	2	0	7	1	0	8	0	1	8
2	2	0	7	2	0	7	1	1	7
3	3	0	6	4	1	4	1	1	7
4	2	1	6	1	1	7	0	1	8
5	4	0	5	5	1	3	0	1	8
Shares	0.289	0.022	0.689	0.289	0.067	0.644	0.044	0.111	0.844

Source: Annex 2

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.

2/ Statistical tests in this table are at the 10 percent level due to the small number of post-secondary students in most samples.

4.3.4 Medical Consultations

As in the quantile shares analysis, examination of the coverage rates for medical consultations show that about 40 percent of significant gender gaps favor females, while there are no cases in which the gap favors males. While there is no clear relationship between this gender gap and expenditures in the first round of surveys, the second round shows a gradient that increases with the welfare quintile, suggesting that the gap is larger (favors women more) as per capita expenditures rise. For all medical services, but not public services, the changes in the gender gap over time actually favor the gender (females) that had the initial advantage. This is in contrast the quantile shares analysis and the education coverage results.

Table 4.18 – Summary of statistically significant gender gaps in usage rates for public medical visits, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
				Public Services					
1	0	2	7	0	1	7	0	0	8
2	0	4	5	0	1	7	0	1	7
3	0	1	8	0	3	5	1	0	7
4	0	4	5	0	4	4	0	1	6
5	0	3	6	0	6	2	0	0	8
Shares	0.000	0.311	0.689	0.000	0.375	0.625	0.026	0.051	0.923
				All Services					
1	0	2	7	0	2	6	1	1	6
2	0	5	4	0	3	5	0	1	7
3	0	0	9	0	3	5	0	2	6
4	0	5	4	0	5	3	0	1	7
5	0	5	4	0	6	3	0	1	7
Shares	0.000	0.378	0.622	0.000	0.475	0.550	0.026	0.154	0.846

Source: Annex 2
Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.
2/ Pakistan is included only for the first survey year.

Once again, there is the suspicion that these results may reflect differences in needs rather than true gender biases, so we repeat the analysis for children under 12 and adults over 45. For children, there are almost no significant differences in either gaps or their change over time. For adults over 45, there are only a few more cases in which the gap favors women, and a tendency for these cases to be in the upper expenditure quintiles. There are only one case of a significant change in the gap. Overall, then, our conclusion from the quantile shares analysis holds up here: medical visits, public and private, are roughly equal for males and females at each quintile in these nine countries once we exclude the age range where reproductive health may generate differential needs for medical attention by gender.

Table 4.19 – Summary of statistically significant gender gaps in usage rates for public medical visits for children under 12 years old, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
				Public Services					
1	0	0	9	0	0	8	0	0	8
2	0	1	8	0	0	8	0	0	8
3	0	0	9	1	0	7	0	0	8
4	0	0	9	0	0	7	0	0	8
5	1	0	8	0	0	8	0	0	8
Shares	0.022	0.022	0.956	0.026	0.000	0.974	0.000	0.000	1.000
				All Services					
1	0	0	9	0	0	8	0	0	8
2	0	1	8	0	0	8	0	0	8
3	0	0	9	0	1	7	0	1	7
4	0	0	9	0	0	8	0	0	8
5	1	0	8	0	0	8	0	0	8
Shares	0.022	0.022	0.956	0.000	0.026	1.000	0.000	0.025	0.975

Source: Annex 2

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.
2/ Pakistan is included only for the first survey year.

Table 4.20 – Summary of statistically significant gender gaps in usage rates for public medical visits for adults over 45 years old, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
				Public Services					
1	0	0	9	0	0	8	0	0	8
2	0	2	7	0	1	7	0	1	7
3	0	1	8	0	1	7	0	0	8
4	0	2	7	0	1	7	0	0	8
5	0	0	9	0	0	8	0	0	8
Shares	0.000	0.111	0.889	0.000	0.075	0.925	0.000	0.025	0.975
				All Services					
1	0	1	8	0	1	7	0	1	7
2	0	2	7	0	2	6	0	1	7
3	0	1	8	0	2	6	0	0	8
4	0	5	4	1	1	6	0	0	8
5	0	2	7	0	3	7	0	0	8
Shares	0.000	0.244	0.756	0.025	0.225	0.800	0.000	0.050	0.950

Source: Annex 2

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.
2/ Pakistan is included only for the first survey year.

4.3.5 Vaccinations

There are almost no significant gender gaps at any expenditure level in our samples, and there are no significant changes in the gender gap over time in these samples.

Table 4.21 – Summary of statistically significant gender gaps in vaccination rates for children, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
					Public Services				
1	2	0	5	0	0	7	0	0	7
2	2	0	5	0	0	7	0	0	7
3	0	0	7	0	0	7	0	0	7
4	0	0	5	0	1	6	0	0	7
5	0	0	7	0	0	7	0	0	7
Shares	0.121	0.000	0.879	0.000	0.029	0.971	0.000	0.000	1.000
					All Services				
1	2	0	5	0	0	7	0	0	7
2	2	0	5	0	0	7	0	0	7
3	0	1	6	1	0	6	0	0	7
4	0	0	7	0	1	6	0	0	7
5	0	0	6	0	0	7	0	0	7
Shares	0.121	0.030	0.879	0.029	0.029	0.943	0.000	0.000	1.000

Source: Annex 2

Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.
2/ No data are available for Bulgaria and Peru.

4.3.6 Public Employment

For public employment, we calculate the coverage rate as the number of public employees divided by the population aged 19 and over, for each quintile/gender. In Bulgaria, there are no significant differences in public employment by gender. In every other country, the difference is significant in favor of males at virtually every quintile. In most cases, the gender gap is strongly correlated with the expenditure distribution, though there are exceptions (Bulgaria, Peru in 1997, and Viet Nam in 1993). There are clearly no cases of the reverse being true. Over time, the gender gap closes significantly only for the two richest quintiles in Peru, while the gap widens significantly for the poorest quintile in Peru, Uganda, and Bulgaria (though here, it changes from a small gap in favor of women to a small gap in favor of men). Apart from medical attention for people of all ages, this is the only case in our study in which there are more cases of an existing gender gap worsening over time than improving.

Table 4.22 – Summary of statistically significant gender gaps in public employment, by quantile and survey

	1st survey			2nd survey			Change		
Quantile	Male	Female	None	Male	Female	None	Male	Female	None
				Public Services					
1	5	0	2	5	0	1	2	0	4
2	5	0	2	4	0	2	0	0	6
3	6	0	1	5	0	1	1	0	5
4	5	0	2	5	0	1	1	1	4
5	6	0	1	4	0	2	1	2	3
Shares	0.771	0.000	0.229	0.767	0.000	0.233	0.167	0.100	0.733

Source: Annex 2
Notes: 1/ The year for the first and second survey is not consistent across countries, nor is the lag between them.
2/ No data are available for Jamaica, Madagascar, and Pakistan (1999 only).

4.4 Summary

From the perspective of this report, the most important generalization is that no matter which method we use, we find no consistent correlation between gender gaps in public health and education services and welfare as measured by per capita expenditures. While there certainly are cases in which the gender gap differs from one quintile to the next, they are relatively few, and the correlation is not consistently negative. Even for time collecting water in Madagascar and Uganda, where the gender gaps are very large, the gap does not decline much for the richer quintiles in Uganda and, if anything, it seems to increase a bit in Madagascar. The one exception to this generalization is public employment, where gender gaps are large and, in many cases, increase strongly with expenditures. Post-secondary education is another possible exception, although the fact that there are so few post-secondary students in these samples makes statistical comparison difficult.

The gender gaps per se that we observe are consistent with the literature reviewed in Section 3. Secondary education has many significant gender differences favoring males in all expenditure quintiles. Post-secondary education also has many gaps, though the rarity of post-secondary students in these samples yields large standard errors, so that it is difficult to reject the null of equality even when the point estimates indicate a large gender gap. Primary education is, with a few notable exceptions, more closely balanced. While there are still many quintiles where boys hold a statistically significant advantage, the reverse is also true in a few cases.

Nevertheless, one has the sense from the existing literature that gender gaps in schooling are universal, a conclusion that our results do not support. Rather, somewhere between one-fourth and one-half of the quintile-specific comparisons show a statistically significant gap in favor of boys, depending on the level and type of service. Further, we find that the significant differences are highly concentrated in three countries – Ghana, Uganda, and Pakistan. Other countries have relatively few significant differences.

Over time, changes in the gender gap for schooling tend to favor girls. At first glance, the fact that there are relatively few cases where this change is statistically significant might lead us to believe that progress is not as rapid as one might hope. But taking into account the many cases where the gap is already small, so that changes are not desirable, the results on changes look a little better. In many cases, the significant reductions in gender gaps occur in the same countries and quintiles where the gaps were large to begin with. But the fact that significant gaps remain in the second survey implies that this process remains incomplete.

Health care consultations usually display gender gaps in favor of females, in all quintiles of the expenditure distribution. However, if we limit our attention to ages in which reproductive health is not a factor, there are very few significant gender gaps, nor are there significant changes over time. Similarly, vaccination rates are almost always similar for boys and girls. Thus, unlike education, gender gaps in health care are of limited importance in these countries.

By far the largest and most consistent gender gaps that we found are in two areas that benefit incidence studies do not typically examine: public employment and time spent collecting water. With the notable exception of Bulgaria, men have significantly higher public employment rates than women in all countries and almost all quintiles, and there is no sign that this is improving over time. If we are willing to consider public jobs as a form of public expenditure 'benefit', this is an example of a clear male advantage—even if it is also true that the public sector is probably less discriminatory in hiring than private employers.

Our data for time spent collecting water are limited to two African countries, both poor. But these results are also dramatic, and point to a means by which governments can at least potentially promote gender equity while pursuing a standard public infrastructure investment in potable water. This issue is explored in the econometric analysis of section 6.

5 Gender Differentiated Demand Analysis: Education and Health Services in Madagascar and Uganda

5.1 Introduction

Given the strong emphasis placed both on investing in human capital and on ensuring or attaining gender equity, it is important to understand not just how the use of services such as education and health care responds to different policies, but also whether these responses differ by gender. In contexts where women or girls are disadvantaged in access to services, this knowledge can provide a way for policy to reduce gender gaps through a form of implicit targeting: it can focus on changing those characteristics of providers that have relatively large effects on female demand. Even in contexts where there is no gender gap in utilization of services, it is important to know whether prospective policies will affect women and men, or girls and boys, differently. For example, if girls' enrollments are more sensitive to price than boys', attempts at cost-recovery though imposition of higher user fees will reduce attendance by girls more than boys.

Although the empirical literature on education and health care demand is very large and growing, studies that investigate whether there are differences by gender in the impacts of policy (and other) variables remain surprisingly rare. Our review of the relevant literature in Section 3.2 assessed the existing evidence for differing impacts of education and health care provider characteristics, service cost, and household resources. Several patterns emerged from this review. There were many examples in education, and a few in health, in which responses to changes in distance or price were larger for females, and there is some evidence for schooling that girls' demand is more sensitive than boys' to service quality. Changes in the level of household resources also often had larger effects on demand for girls' schooling and (though the evidence is comparatively thin) healthcare or nutrition. However, numerous examples also exist of no gender differences or of greater male demand response. Thus there is a need for country-specific analysis, using appropriate statistical methods to make gender comparisons, and this is the motivation behind the analysis in the present section.

We estimate the demand for education and health services in two African countries, Madagascar and Uganda. The main focus of the analysis is the testing for gender differences in response to changes in provider quality, availability (distance), and cost. However, following on the benefit incidence analysis in Section 4, we are interested not only in supply characteristics but also in the interaction of gender and the level of household resources in determining the demand for services. We take the most flexible approach to addressing these questions, by estimating separate models for males and females and testing for statistical differences by gender in the effects of specific covariates on the probabilities of using the service. We consider the following services: primary education; secondary education; and curative health care.

5.2 Data and Methodology

We use two of the surveys included in the benefit incidence analysis: the 1993 Madagascar EPM and the 1992 Uganda IHS. These countries and survey years were chosen because the household surveys were complemented with community surveys that include information on the characteristics of local education and health facilities. The types of questions asked are routine for such surveys. For example, for primary school, the numbers of students and teachers, fees and textbook costs, building condition, etc; for health services, presence of doctors, nurses, medicines, and refrigeration. In Madagascar the community survey was essentially restricted to rural areas – about 90% of the sample is rural. In Uganda there was no such restriction, but the population is overwhelmingly rural so an almost equal share of the sample (about 87%) is rural in any case.

Despite these similarities, the data and estimation procedures differ in a number of respects for the two samples. Some country specific details are addressed next.

5.2.1 Madagascar

The manner in which the Madagascar provider data were collected makes these data especially suitable for demand analysis. For education, information on provider characteristics is collected for up to three providers used by local residents; for health care, up to four providers are listed. The type of facility (e.g., for health care, clinic, hospital, doctor) and sector or management (public, private non-religious, private religious) is recorded in each case. These categorizations correspond to those in the household survey modules on utilization of services. Therefore we are able to estimate discrete choice models of the choice among alternatives as a function of the attributes of each alternative (as well as of individual and household characteristics). These choices are estimated using either multinomial logit or nested multinomial logit.[64]
The econometric framework for estimating provider choice is described in detail in Appendix 5.1 at the end of this section. A somewhat atypical feature of our application of these models is that they are constructed to account for the fact that not all individuals have access to every type of provider. For example, while almost all rural communities have access to a public primary school, only about 1/4 have a local private primary schools, that is, have such a school listed in the community questionnaire as being used by local residents. For health care, the large majority of individuals in our sample live in communities where there is access to some form of basic care, but the average availability of hospital and private formal providers such as doctors or private clinics is smaller: 61 and 45 percent, respectively.

[64]The nested model is a generalization of the simpler non-nested multinomial logit model that allows the error terms for related choices to be correlated (see the appendix to this section for details). In cases where the estimated correlation fell outside of acceptable bounds (that is, was greater than 1), we present instead the non-nested multinomial logit results.

In health care, unlike primary schooling, there is a fairly wide range of alternatives – up to 10 possible provider types, ranging from informal healers to major hospitals. Given this large number of choices, it is necessary for the estimation to group the alternatives into broader categories. We use the following logical groupings: hospital (primary or secondary hospitals); basic or primary care facilities (*Dispensaire, post sanitaire, post d'infirmerie, centres de soin de sante primaire*), private formal care (doctors, private clinics, and pharmacies), and a base category consisting of self-care and informal private care (e.g., traditional healers). The first two categories, hospitals and basic care facilities, are generally public in Madagascar.[65] We should note that individuals in rural areas who visit hospitals almost always are doing so for basic care, that is, for outpatient services; only a tiny percentage of such individuals report staying overnight in a hospital.

For secondary schools the community surveys do not collect detailed data; only the distance to the nearest lower and upper secondary schools is recorded. Still, given the evidence discussed earlier that girls' enrollment tends to be more sensitive to changes in distance to schools, and the fact that gender gaps are usually larger in secondary than primary school, it is worthwhile to assess the effect of distance in the current context. We do this using a simple binary probit model to estimate current secondary school attendance of children age 14-18. We choose this restricted age range because even though the official age for starting lower secondary school is 12, many children have not completed primary school by this age due to the prevalence of late entry as well as grade repetition during the primary cycle. Choosing a higher age for the bottom of the range avoids considering current primary enrollees, some of whom may go on to secondary school, in the same (base) category as older children not in secondary school, whose future attendance can more reliably be ruled out.

5.2.2 Uganda

The community survey component of the 1992 IHS gathers information on the nearest primary schools and health clinic used by residents of the community; if the nearest is not the most commonly used, information on the most common provider is also collected as long as the provider is in the district (a much larger geographical unit than the community itself). Unfortunately, while the household data indicate the sector (e.g., public, private) of school or health care provider chosen, sector information is not recorded for the providers listed in the community data. Because of this, as well as the fact that typically just one provider is listed, it is not possible to estimate polychotomous choice models using provider specific characteristics as for Madagascar. Instead, we make the dependent variable a binary indicator of seeking care at any facility (for health care) and attending any primary school (for education). These are estimated using ordinary probits, as functions of the characteristics of the nearest provider.[66] This approach, while dictated

[65] See Glick et al. 2000 for details.

[66] When two providers are listed, it is not obvious a priori whether the characteristics of the nearest provider or an average of the two are a better representation of the opportunity set of the household. For health care, the results were essentially the same. For primary school, the 'nearest' data provided a slightly better fit in the sense of greater precision in the provider characteristics estimates, so we used the nearest provider data in both cases.

by the nature of the facility data, will be less than ideal to the extent that individuals have more options of different types than just the one or at most two listed in the community questionnaire.

A second issue to note with respect to the Uganda data is that the community data were collected at the level of the RC (‘revolutionary council’) whereas households are only identified by enumeration area, which can encompass several RCs. About 25% of the enumeration areas in the survey have more than one (usually two) RCs. For these cases there is no way to match households to the data for their specific communities/RCs. The alternatives were to take averages over communities of the provider variables where there was more than one community per enumeration area, or to restrict the analysis to those cases where there is just one community per EA. The former approach risks imprecision in representing the constraints facing households since we are averaging over the community in question and the others in the EA. The latter may entail selectivity bias since EAs with just one RC may differ in unmeasured ways from those with more than one, which presumably are more densely populated and possibly, less remote. We judged the first risk to be a more serious limitation, and restricted the analysis to the cases where we could clearly match households to the characteristics of their local providers.

As in Madagascar, the available data on secondary schools in the IHS community survey is limited to the distance to the nearest secondary school (not distinguishing lower or upper levels). As we do for Madagascar, we model the choice of attending secondary school using probit and for similar reasons restrict the sample to children age 14-18.

Finally, a general comment: as in the vast majority of similar studies, we treat school and health provider characteristics as exogenous to household choices. As discussed in some detail in Section 2, this assumption can fairly easily be challenged. In the absence of ways to deal with potential endogeneity, one must at the very least be cautious in interpretation: the estimated effects of community and provider factors (and possibly, gender differences in them) may be biased. As noted earlier, the direction of the bias is generally not clear *a priori*.

5.2.3 Treatment of Costs

The cost to households of using education or health services is comprised of direct monetary costs (fees, medicines or school supplies, transportation expenses) and indirect or opportunity costs. The latter represents household income or production from work in the labor market, a family farm or enterprise, or in domestic activities, that is foregone when the individual makes use of the service. As described in section 3.2, researchers sometimes aggregate these costs to arrive at a single monetary measure. This procedure involves multiplying the estimated time foregone in productive work by a monetary measure of the individual’s opportunity cost of time; the latter is represented by a regression-based predicted wage or the average wage in local labor markets. Such a procedure was carried out in an earlier (non gender-disaggregated) analysis of the Madagascar data (Glick et al. 2000).

While conceptually appealing, this method has a number of shortcomings. Among them is the doubtful reliability of market wages as a measure of opportunity cost of time for most individuals in rural areas, where labor markets may be thin and most individuals do not participate in the labor market but rather are self-employed in family farms or engaged in domestic work. For children, who are even more rarely involved in wage work, it is often impossible to derive even such an imperfect measure of the value of their time. For a gender-focused analysis, the procedure is even more problematic. The opportunity cost of using a service is closely related to the distance to the provider, and our earlier review of the literature suggests that distance may have different impacts on demand by gender even if the response to monetary costs are the same. For this analysis, therefore, it will be more illuminating to enter distance separately from the direct monetary costs of a service. Further, since these two factors correspond to distinct policy levers (change user fees, build more clinics to reduce travel times to services), disaggregation potentially provides more policy relevant information.

As noted, information on distance to education and health providers is available from the community surveys for both of our samples.[67] With respect to direct costs, for the Madagascar primary schooling nested logits, the costs of public and private school alternatives are the community median per pupil education expenditures (tuition, textbooks and other expenses) for each school type, calculated from the household survey.[68] For Uganda primary schooling, the cost variable used is the sum of fees and textbook expenses for the provider(s) recorded in the community survey. For the secondary school probits we are only able to include the distance measure. As indicated, cost data for were not collected for secondary schools in either community survey, and the alternative of using community medians was not feasible because secondary attendance is too sparse to make such cost estimates reliable. While one could instead impute costs based on means or medians over a larger geographical area, as a representation of costs to households in a given community this would probably entail a great degree of measurement error.

For health care, in both the Uganda and Madagascar samples, the cost variable used is the consultation fee recorded for the facility, taken from the community survey. It should be noted, however, that in most cases no (official) fee is charged, a situation typical of developing countries' health services. Thus in Madagascar only about 10% of public hospitals and 17% of basic care providers in the community survey are reported as charging consultation fees. In Uganda the share of clinics charging a fee is higher, about 42%.

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[67]Note that for schooling, the distance to school captures only part of the opportunity cost of schooling, and not the most important part, which instead arises from the hours per day spent in school. To measure this cost requires a reliable measure of the opportunity cost of time of children, which as noted is difficult to obtain.

[68] See Glick et al. for details on variables used in the Madagascar analysis (for both education and health) including a discussion of potential shortcomings of the measures used. See also Glick and Sahn (2000) for a discussion of general specification issues in these models.

Many empirical studies have found that responsiveness to the price of services is greater among the poor than the well off. To allow for this we interacted our cost measures with household expenditures in all models. In only once case, primary schooling in Madagascar, were these interactions found to be statistically significant, so other than in that case, the final specifications do not include the interactions. In addition, it is possible that responsiveness to specific non-price provider characteristics also varies across the income distribution. This is potentially important since it could help in choosing education or health care improvements that would have the strongest impacts on service utilization by the poor (or by females in poor households). However, experiments with a range of interaction terms did not support this hypothesis for these data.

5.2.4 Comparing Female and Male Demand for Services

Finally, we make a few comments on our method for making gender comparisons of the impacts of policy (and other) variables of interest. While we will also discuss the logit and probit models estimates, for our gender comparisons we test the null of equality of marginal effects, that is, the impacts on the probability of using a service or on the choice of provider. For the cases where we have estimated the choice among multiple providers, we also compare impacts on the overall demand for the service, that is, the probability of enrolling in any primary school or getting treated by any formal health care provider. For simple binary probits for using a service the computation of marginal effects and their standard errors is straightforward and follows the method discussed in Section 2 closely. The reported marginal effects represent sample average responses, that is, we calculate the sample means of the individual marginal effects (see the discussion in Appendix 2.2).

For the provider choice models for primary schooling and health care in Madagascar, the fact that certain providers are not available to all households in the sample raises a few complications. Obviously, when a provider type is not available, changes in either household or provider characteristics will have a zero effect on the probability of choosing that provider. Therefore the estimate of the overall (sample mean) marginal effect will be a weighted average of the non-zero marginal effects for those with access to the provider and zeros for those with no access to the provider. The marginal effects we calculate and compare across genders thus incorporate existing availability constraints facing different households when simulating a policy change or change in household circumstances. While this approach is probably the most relevant for policy analysis, the results do not reflect purely behavioral determinants, since the changes in choice probabilities are affected by whether providers are locally available. To assess household responses without this effect of availability, we also made the calculations for the subsamples for which all the education or health care providers are available.[69] As one

[69] This does not actually entail re-estimating the multinomial logit models on different subsamples. The parameter estimates on which the calculations of the changes in probability are based are the same for different subsamples; that is, the underlying functions for utility conditional on choosing an option are the same for all those for whom the option is available. When the option is not available, the observation simply does not contribute to the identification of the parameters associated with that choice.

would expect, these simulations tend to show substantially more substitution between alternatives in response to changes in provider or household characteristics. However, our inferences regarding differences in male and female demand responsiveness to these changes were essentially unchanged. Given that this is the focus of our analysis, in what follows we do not report the marginal effects and statistical tests on the subsamples defined by availability.

5.3 Empirical Results

5.3.1 Rates of School Enrollment and Treatment for Illness/Injury

Some basic enrollment data for our two samples are shown in Tables 5.1 and 5.2. Primary enrollment, not to mention secondary enrollment, is strikingly low in these poor, largely rural samples. In both cases, only about 50% of children 6-12 are attending primary school. Primary enrollments of girls are slightly lower than enrollments of boys in Uganda (47% vs. 51%) but not in Madagascar. At the secondary level the gender gap widens (in proportional terms) in Uganda: 13% of girls compared with 17% of boys age 14-18 attend secondary school. For Madagascar there is no evidence of a gender gap, but overall secondary enrollment is even lower than in Uganda. Note that these patterns in secondary enrollments remain when we consider just children who are primary school graduates.

Table 5.1 – Primary enrollment rates, children 6-12

	Public school	Private school	Any Primary
Madagascar			
Girls	0.45	0.08	0.53
Boys	0.44	0.08	0.51
Uganda			
Girls	0.47	0.12	0.59
Boys	0.51	0.12	0.63

Sources: Madagascar 1993 EPM, Uganda 1992 IHS

Table 5.2 – Secondary enrollment rates, children 14-18

	All	Primary completers only
Madagascar		
Girls	0.10	0.56
Boys	0.12	0.53
Uganda		
Girls	0.13	0.35
Boys	0.17	0.43

Sources: Madagascar 1993 EPM, Uganda 1992 HIS

Descriptive data on health care for adults and children reporting a recent illness are shown in Table 5.3. The different categorizations of care in the Madagascar and Uganda panels of the table reflect the structures of the two survey questionnaires. In Madagascar, there are no gender differences to speak of in the percentages of ill adults (age 15 and older) getting any treatment or getting treatment from specific types of providers, though among children under 15 girls appear slightly more likely to get care. In Uganda, rates of formal curative care are generally similar for females and males, if slightly higher for men than women. With respect to this difference, as discussed earlier the nature of illnesses of men and women may differ, so not much can be read into the observed small gap in the shares receiving care. Note the large difference between the two countries in the percentages of ill individuals who receive formal health care: for example, about 30% of adults in Madagascar compared with over 50% in Uganda.

Table 5.3 – Children and adults reporting recent illness/injury: percent seeking formal care

	Adults (age 15+)		Children under 15	
	Women	Men	Girls	Boys
		Madagascar		
Hospital	0.08	0.07	0.08	0.06
Basic care facility	0.13	0.14	0.22	0.18
Private formal care	0.07	0.07	0.07	0.07
All formal care	0.28	0.29	0.37	0.32
		Uganda		
Public facility	0.20	0.21	0.21	0.22
Private facility	0.31	0.33	0.35	0.34
Private doctor	0.02	0.04	0.02	0.04
All formal care	0.53	0.58	0.58	0.59

Sources: Madagascar 1993 EPM, Uganda 1992 HIS

5.3.2 Demand Determinants

We turn now to the econometric results. Although we will discuss both the parameter estimates and marginal effects calculations, only the marginal effects are presented here: they are grouped together in Appendix 5.2 at the end of this section. The full multinomial logit results for Madagascar for health and primary education are relegated to Annex 3 of this report. All the other models are simple binary probits. For these, we present the full marginal effects in Appendix 5.2 and dispense with the presentation of the probit estimates themselves. We discuss the results in the following order: distance, cost, other provider characteristics ('quality'); household resources; and other covariates of interest.

5.3.2.1 Distance

We consider the schooling findings first. Nested logit estimates for choice of primary school in Madagascar are shown in Annex 3, Tables 1-2; the marginal effects calculations for selected independent variables are presented in Appendix Table A5.2.1. The latter show the impacts of unit changes in the variable on the public enrollment probability, the private enrollment probability, and the overall primary enrollment probability. The nested logit results indicate that distance has a strongly significant negative effect on utility from public primary school for both girls and boys but no effect on utility from private school.[70] The marginal effects calculations for public school distance show that an

[70]The parameter estimates in the discrete choice model show the effect of the explanatory variables on the utility from the alternative. See the Appendix to this section for details.

increase in distance reduces the probability of public enrollment, increases the probability of attending private school, and reduces overall primary enrollment probabilities for girls and boys. Note that there is only a small amount of substitution into private schooling, so the reduction in overall primary enrollment is not much smaller in absolute value than that for public primary alone. This is due in large part to the fact that we are considering the sample as a whole and only a minority of observations in the sample has access to a local private school option.

The last two columns of Table A5.2.1 show the female-male difference in marginal effects and the t-statistics for the null hypothesis that the difference is zero. As shown, we cannot reject the null for any of the marginal effects of public school distance.

In Uganda, distance to the nearest primary school also has a highly significant negative impact on (any) primary enrollment (Table A5.2.2). Also as in Madagascar, changes in distance have statistically similar impacts on girls' and boys' enrollment probabilities.

For secondary schooling as well, distance to schools has strong negative effects on school attendance in both Madagascar (Table A5.2.3 – lower secondary distance only) and Uganda (Table A5.2.4). Again, in each case we find no difference in the marginal effects for girls and boys. For Madagascar we are also able to include a dummy variable for the presence of a paved road in the village. Public investments in road construction may make a significant difference in the time involved in traveling back and forth to school when the distance to school is substantial, as is the case even for lower secondary schools in rural Madagascar (the mean distance is 15 kilometers).[71] Indeed, this variable has a positive and highly significant impact on secondary enrollment, and one that is similar for girls and boys.

The estimating sample for secondary schooling includes all children of secondary school age (that is, 14-18 years old). Hence it treats children the same way whether they have completed primary school, dropped out of primary school, or never even enrolled in primary school. Obviously, only those in the first group – primary completers – are actually able to go on to secondary school. Thus the simple current secondary enrollment model does not account for the sequential nature of education decisions. This in itself is not a problem as long as one is careful to interpret the results as showing the ultimate impacts of the independent variables on secondary enrollment, some of which operate through their impact on prior primary school enrollment and completion. Still, we are also interested in the factors that determine whether primary graduates continue on to the secondary level. Separate models run on the sample of primary completers (not shown here; see Annex 3, Tables 3 and 4)[72] yield results similar to the unconditional models: in each case distance to school (any secondary in Uganda, lower secondary in Madagascar)

[71]As one form of evidence of this, the community survey shows that 40 percent of villages with a local paved road also have a taxi-bus (*taxi-brousse*) stop, compared with just 3 percent of villages not served by a paved road.
[72]These estimations do not deal with the fact that the sample of primary completers is non-random: those who progress this far in their education who may differ in important unobserved ways from those who do not.

reduces secondary enrollment probabilities, and in each case we cannot reject the null of no difference in the effects for girls and boys.

Thus we find that in rural Madagascar and Uganda, distance to schools acts as a significant constraint on enrollment at both the primary and secondary levels. In contrast to findings reported from a number of studies, however, enrollments of girls are not more sensitive to distance than those of boys.

For health care services, somewhat surprisingly, in one of our countries (Uganda) we do not find significant impacts of distance to facilities on the use of health care for any of the four subsamples in our estimations: women, men, girls, and boys (Tables A5.2.7, A5.2.8). In contrast, the provider choice models for Madagascar point to distance as a constraint on demand for health services (Annex 3, Tables 5-8); only in the case of girls do we fail to find a significant distance effect. The marginal effects calculations for these models are shown in Tables A5.2.5 and A5.2.6. We report the calculations and tests only for changes in distance to the most important source of care, primary care facilities; the same inferences about marginal effects and their differences by gender obtain for changes in distance to each of the other providers. This is true for the other provider characteristics as well, so for these too we will report marginal effects for primary care characteristics only.[73] As shown, there are no significant gender differences in the effects of a change in distance to primary care on the probabilities of treatment at specific providers or overall.

5.3.2.2 Direct Costs

For primary schooling in Uganda (Table A5.2.2) we do not see significant impacts of local primary school cost on overall primary enrollment for either girls or boys. We do, however, find such effects in the nested logit primary school choice models for Madagascar (the logit estimates are in Annex 3 Tables 1-2). For both boys and girls, utility of attending public primary school relative to no school falls with school cost, but the effect is smaller for more affluent households, i.e., the sign of the interaction with log household expenditures is positive. Evaluated for the full girl and boy samples, the mean marginal effects (which incorporate the interaction with expenditures) are slightly larger for girls (Table A5.2.1). For all outcomes (public, private, and overall primary probabilities), however, girls' and boys' marginal effects do not differ statistically. For private school, there is a negative, if marginally significant, coefficient for boys but not girls. Equality of the marginal effects of private school cost cannot be rejected, reflecting the lack of precision of the nested logit coefficients.

[73] This is largely because in these multinomial logit models for health care choice (unlike the primary schooling models) the coefficients on provider specific covariates are restricted to be equal for different choices. Imposing this restriction is in fact standard in the literature based on theoretical considerations of consistency in preferences (Gertler et al 1987), although this justification has been disputed by Dow (1999). We maintain the standard restriction here in part to avoid having a huge number of parameters in these models, a problem given the smaller sample sizes combined with a larger number of provider alternatives.

Since the price effects vary with level of household expenditures, we also calculated the average marginal effects for the following subsamples: observations in the bottom two per capita expenditure quintiles only, and those in the top two quintiles (results not shown). As expected, the marginal effects of price are larger for the former group as well as being more precisely estimated. However, in both cases the boy and girl marginal effects remain similar in magnitude and are indistinguishable statistically.[74]

In the health care demand models, for adults and children in Madagascar (Annex 3 Tables 5-8) the coefficients on costs are insignificant with the exception of the boys' care logit. Given the lack of variation in the data due to the presence of so many zero values for price, such a result is not surprising. Still, the marginal effects results in Table A5.2.6 confirm that boys' probabilities of treatment when ill are more sensitive to price than girls'.

In the Ugandan sample, higher consultation costs at local providers reduce the probability of treatment at a formal care facility of men as well as boys (Tables A5.2.7, A5.2.8). The estimates are significant only at 10% and we are unable to reject equality of male and female marginal effects. Still, it is noteworthy that in both of our samples price effects are only detected for men or boys.

The first result reported in this section is somewhat troubling: the lack of any response of primary school demand to price in Uganda. We noted earlier that the introduction of free schooling in Uganda was associated, at the national level, with a sudden and massive increase in enrollments. The simple before-and-after comparison is not free of problems of interpretation. Among them is the fact that the fee removal was accompanied by other reforms as part of the country's universal primary enrollment strategy; these as well as the publicity campaign for UPE may have contributed to the increase in demand. Yet a shift in enrollment of the magnitude observed would lead us to expect to find at least some price responsiveness in the cross section data. The lack of such a response suggests problems in our school cost data: measurement error due to misreporting, or a positive association of higher fees with unmeasured school quality or local demand for schooling. These are common problems in demand estimation (see section 2.2.1.2) and suggest, as always, the need for caution in interpretation.[75]

5.3.2.3 Provider Characteristics

Initial specifications for primary school demand for both Madagascar and Uganda included, among other covariates, teacher/student ratios or a related variable, maximum class size. These covariates invariably turned out to be positively (though usually not significantly) associated with enrollment probabilities – a not uncommon finding (see

[74] It is also worth noting that the marginal effects and statistical inferences on the non-interacted covariates are essentially unchanged by the changes in sample used for the calculations. It was noted in Appendix 2.2 that this may not be the case given the dependence of the marginal effects on the values of each of the regressors.

[75] Note that not all covariates necessarily suffer from such problems or at least, not all suffer from them to the same degree. Variables such as distance, education, and family wealth or consumption are usually easier to measure and probably less contaminated by association with unobservables.

e.g., Glick and Sahn 2001; Alderman et al 2001). Rather than indicating that parents respond positively to more crowded schools, this is likely a reflection of the fact that high local demand for a particular school results in larger class sizes. Hence this type of variable seems particularly susceptible to simultaneity problems, and we exclude it from the final specifications of the models.[76]

Of the remaining covariates, the Madagascar estimates for public primary school suggest that enrollment choices do respond to school quality. Multigrade teaching, whereby a teacher is assigned to instruct more than one grade level at the same time, has a strongly significant negative impact on the utility from public school.[77] Although there is evidence from some developing countries that multigrade classes need not be detrimental to learning if teachers are trained in the appropriate techniques, it is thought to be a problem in Madagascar (see World Bank 2000) and our demand estimates bear this out. As Table A5.2.1 indicates, the negative impacts on public school and overall enrollment probabilities do not differ statistically for girls and boys.

Two other public school quality variables, 'good condition of windows' – which may be acting as a proxy for overall facility quality – and a building condition indicator, have generally significant positive impacts on the probability of choosing public school and overall primary enrollment. The exception is the impact of public school building condition which is significant only for boys. For these variables the point estimates tend to be larger for boys, but statistically we cannot reject the null of equality except (at 10% level only) for the case of the impact of good building condition on overall primary enrollment.

For Uganda, we have a somewhat broader array of primary school attributes at our disposal. Included in the models shown are the share of teachers with teaching certificates, the number of shifts of classes in the day, and an indicator of building maintenance.[78] However, in contrast to the case of Madagascar, we are unable to detect (plausibly signed) significant impacts on enrollments for these variables, let alone find differences in effects by gender (Table A5.2.2). The share of qualified teachers in the girls' probit is significant but negatively signed. This may be another reflection of simultaneity: where local demand is high, schools may have to expand their teaching staff by adding less qualified personnel.

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[76] Note, however, that parents may indeed respond positively to increased class size if they regard the level of attendance at a school as a signal of school quality: i.e., a school that is well attended or even crowded is one that others in the community apparently consider to be of good quality. In any event, inclusion of student/teacher ratios or maximum class size did not appreciably alter results for the other school covariates.

[77] The high prevalence of multigrade teaching is driven by a combination of inadequate supply of teachers, low population density, and the government's commitment to operate a primary school in each rural *fokontany* (a village or collection of villages): many such schools as a consequence have relatively few students in each level and only a couple of teachers, requiring that levels be combined.

[78] Other covariates tried include indicators of teacher experience, and the number qualified or experienced teachers per student. Since school quality variables are usually positively correlated, multicollinearity may obscure some significant impacts or lead to improbably signed results. However, experiments with entering these regressors singly or in smaller groups indicated that this was not the case.

For both countries, the health care models generally yield only a small number of statistically significant results on provider characteristics. To the extent that we find (even marginally) significant coefficients, they are for men or boys only: among adults reporting an illness in rural Madagascar, men's probabilities of using a primary care facility are higher if free malaria medicines are offered there (overall formal care demand also increases), and boy's primary and overall care probabilities are increased if the primary provider is staffed with a doctor and uses a refrigerator (Tables A5.2.5, A5.2.6).[79] However, with the exception of presence of a doctor in the children's models, we cannot reject the equality of male and female marginal effects.

With relatively few provider covariates in the health care models having significant coefficients in the first place, it is difficult to make conclusions about male-female differences – unless we are willing to take the results at face value and say that quality has only limited effects on health care demand of either gender. Given the possibility of errors in measurement of quality and other factors noted in Section 3.2.2, this is probably not advisable.

5.3.2.4 Household Resources

In almost all of the schooling models we find significant positive impacts of log household expenditures on enrollment. The only real exception is for secondary education in Madagascar in the estimates conditioning on having finished primary school. Since expenditures do have significant positive impacts in the unconditional secondary enrollment models, we can infer that the level of household resources affects secondary enrollment in rural Madagascar largely through its effect on primary enrollment (and completion). In two cases – Madagascar primary schooling and Uganda secondary schooling – the impacts differ by gender such that boy's enrollments – not girls' – are more responsive to household expenditures. In both of these cases the difference is large, with the calculated marginal effects for boys several times greater than that for girls.

For health care, in the Madagascar provider choice models for adults, an increase in log expenditures raises the probability of getting formal curative care when ill; this increase seems to come through changes in private formal care (Table A5.2.5). The impacts are not different for women and men. No significant effects of household expenditures are seen for children's health care in Madagascar. However, in the Uganda sample, household expenditures have strongly significant impacts on the probability of formal care for all groups: women and men, and girls and boys. In the children's case there is a pronounced gender gap: the marginal effect for girls is more than two times larger than the effect for boys. Since expenditures are entered in log form, this means that the proportionate increase in girls' probability of care from a given increase in household expenditures is more than double that for boys. The contrast between this result and the 'pro-male' gap in expenditure effects for primary schooling in Madagascar is noteworthy.

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[79] The negative signs on the marginal effects for the other providers (hospital and private formal care) reflect substitution from these alternatives to primary facility care when the primary care center has the characteristic.

5.3.2.5 Own or Parental Education and Household Composition

Although the focus of this analysis is on gender differences in the effects of provider variables and household resources, we also briefly consider the impacts of other household covariates that may have important influences on schooling and health care choices: education (of oneself, ones' parents, or the head of household as the case may be) and household composition.

The education levels of the mother and father generally have positive and significant effects on school enrollments in both of our samples at both the primary and secondary levels, in keeping with the usual findings in the literature. We do see some gender difference in these effects. For primary school in Madagascar, the point estimates of the marginal effects of mother's primary education on public primary (and all primary) enrollments are markedly larger for girls than boys, though the difference is at best marginally significant, and then only for primary school, not all school. A stronger impact on girl's primary enrollments can also be detected in Uganda (Table A5.2.2), but in this case it is for father's education, especially secondary education. These primary results point, if tentatively, to stronger impacts of parental education on girls' schooling . In the secondary enrollment models (not conditioning on primary completion) we find a less consistent pattern: mother's education seems to matter more for boys than girls in Uganda, while in Madagascar, mother's schooling seems to benefit boys more while father's schooling benefits girls. Note, however, that the gender differences in marginal effects are at most marginally significant in these cases.

The health care models include variables for the individual's own schooling in the case of adults and schooling of the household head in the case of children under 15. For adults in Madagascar we find no significant impacts of own schooling on care probabilities (Annex 3 Tables 5-6). It should be kept in mind that these schooling effects are net of the impacts of education coming through increases in household expenditures. In Uganda, we do see some significant schooling impacts on men's care probabilities for particular school levels (completed primary and secondary) but statistically these impacts cannot be distinguished from those for women (Table A5.2.7).

For children's health care, years of schooling of the head increases the demand for hospital care, private doctor/clinic care, and overall care for girls in Madagascar but has no significant effect on boys' care probabilities; however, we are unable to reject the null of equal marginal effects. In Uganda, schooling of parents generally has no impacts, controlling for household resources, on the probability of treatment at a health care facility. The only exception is a strongly significant *negative* impact of the head having secondary or better schooling on girls' care probability. This anomalous result is not reliable, however, since only 4% of the children in the sample live in households where the head has a secondary education.

For household composition, while a number of covariates are significant in the education and health models, it is generally difficult to discern patterns (and even more so difference in patterns by gender) as well as to interpret the coefficients. The composition effects may reflect substitution between household members in home (and market) activities, the possibilities for which are affected by household size and composition. However, composition variables are often also correlated with the level of household resources and thus may pick up income effects not captured by the household expenditures estimates. Further, as is well known, household composition may be endogenous to the education and health care outcomes being examined.

Still, several interesting results are seen in the secondary enrollment probits that lend themselves to plausible if not conclusive interpretations. In Uganda, having more children under 5 in the household reduces girl's secondary enrollment probabilities while having no impact on boys (Table A5.2.4). Another notable gender difference is in the effect of the number of adults (16 and older) in the household, especially the number of women, for which there is very strongly significant positive impact on girls' secondary schooling. The gender difference in marginal effects is significant both for this variable and children under 5, and these results are robust to conditioning on primary completion.

In the Madagascar secondary schooling models fewer household composition covariates are significant. Consistent with the Uganda results, however, girl's secondary enrollments are reduced by the presence of a greater number of children under five years of age (though in the unconditional model only) and are increasing in the number of children 5 to 14 (Table A5.2.3). Neither of these variables has any effect on boy's secondary enrollments, though we cannot reject equality of the marginal effects at conventional significance levels. The negative impact of young children on girls' secondary schooling in both the Uganda and Madagascar samples suggests – especially as no similar effect is found for boys – that girls' post-primary schooling is constrained by household responsibilities, in this case the need to care for younger siblings.

5.4 Summary and Conclusions

This analysis of gender and the demand for services has been broad in scope. It has examined education and health care demand in two countries and considered the impacts of a range of factors: distance, cost, provider characteristics, household resources, and schooling and household composition. Gender differences in impacts were assessed through statistical comparisons of male and female changes in probabilities of enrollment or curative care in response to changes in these variables. A reading of the published research on this issue gives the impression that such gender differences are prevalent, especially for education demand, for which a number of studies have reported greater sensitivity of girls' enrollment or academic achievement to many of the factors just enumerated. However, this is not the conclusion one would draw from our two case studies. With relatively few exceptions, we do not find gender differences in the effects of each of these factors. Further, where the null that female and male marginal effects are equal can be rejected, it is as often in favor of showing a stronger response of male demand than female demand. These results are particularly noteworthy for schooling in Uganda, where gender gaps in utilization existed at the time of the survey; the findings tell us, broadly, that these gaps are not explained by gender differences in the effects of the wide range of factors included in the models estimated.

As in many other studies, distance to education and health facilities consistently emerges as a deterrent to the use of these services in the estimations. However, significant gender differences were not found. For schooling this is particularly noteworthy since it has become fairly common to assert that girls' access to education is more constrained by distance than boys'. While in many societies this may indeed be the case, it is not so in Madagascar and Uganda. For direct (monetary) costs of services, there are for the most part no gender differences in impacts, though we find that in rural Madagascar, boys' treatment is more sensitive to price than girls'. However, for many services we found no significant impacts of cost at all for either gender. For Uganda primary schooling this result is sharply at odds with the historical record of enrollment increases following the elimination of cost recovery in 1997, which further suggested a stronger effect on girls' enrollments. Here we would be inclined to put more faith in the historical data.

In general we did not find many significant impacts of non-cost provider characteristics– provider 'quality' – on demand. The rather strong exception to this was the impact of public school attributes in the primary school choice models for Madagascar, and in this case, the effects on girls' and boys' enrollments were by and large similar. Impacts of health care provider quality were fewer and limited to Madagascar. Significant effects were found only for men or boys, but still it was difficult to establish differences by gender statistically.

We should note that although our datasets contain a large number of provider characteristics, they do not include a number of factors that might be expected to have differing impacts by gender. This is probably especially relevant for education: the list would include such variables as the presence of female teachers, of separate bathroom facilities for girls and boys, and teacher attitudes toward girls' education. We also were unable to assess whether various aspects of schools have differential effects on girls' and boys' learning as opposed to simply enrollment.

The level of household resources was found to influence schooling and health care utilization in each country in almost all subsamples (women, men, girls, and boys). Some gender differences emerged here. For Madagascar primary education and Uganda secondary schooling, boy's enrollments are significantly (statistically as well as in terms of magnitude) more responsive to household expenditures than are girls'. In contrast, for children's visits to formal health care facilities in Uganda, girls' demand increases more strongly with income than boys'. The 'male bias' in income effects in several of the schooling models are the opposite of the pattern frequently noted in the literature. However, we have argued in Section 3.2 that the overall evidence of greater relative benefits of household income growth to girls' schooling is not as strong as is typically assumed. Recall as well that our benefit incidence analysis on a sample of nine countries detected few cases where gender gaps in service benefits, in education or elsewhere, changed over the income distribution.

What do our findings imply for policy? The first conclusion is very general and was made earlier in this report. Investigation of gender differences in the impacts of various policy levers must be conducted on a country-by-country basis. Although previous studies may suggest the existence of certain patterns by gender – patterns which themselves need to be considered cautiously, as we have endeavored to point out – policymakers cannot assume that country specific results will conform to them.

Second, if, as in our case studies, there appear to be few supply side factors – price (at least in Madagascar), distance, quality – that affect female and male demand for services differently, policies may have to directly target gender to rectify gaps in benefits where they exist. For example, if price responses do not differ by gender, subsidies for girls' schooling can be set higher (or user fees set lower) than for boys'. Examples of this approach, cited earlier, include the Bangladesh secondary school stipend program (Bellew and King 1993) and the Quetta Urban Fellowship program in Pakistan (Kim et. al. 1999).

Where (as in Madagascar for education and health care, and Uganda for health care) one finds that there is little gender bias to start with *and* that policy variables do not by and large affect the genders differentially, the implication is favorable. We can infer that interventions to improve schooling and health care availability or quality, or to increase cost-recovery through user fees, will not lead to the emergence of gender imbalances in access.

Lastly, our results suggest the potential in some instances of reducing gender gaps in access to social services through interventions outside the education and health sectors themselves. The estimates for secondary school enrollment in both Madagascar and Uganda provide inferential evidence that girls' secondary schooling – but not boys' – is constrained by domestic responsibilities, namely, the need to care for younger siblings. In the case of Uganda, girls' secondary enrollments lag behind boys'. In this environment, public initiatives to provide substitute childcare services may function indirectly to target girl's secondary enrollments and thereby reduce the gender gap.

Appendix 5.1: Econometric Model of Provider Choice

We use a standard framework for estimating choice of school or health care alternatives, described briefly in section 2.2.1. Here we present a somewhat more detailed description, using slightly different notation. The household is assumed to choose the school or health care alternative (including non-enrollment or no care) that brings the highest utility. We illustrate the model for the case of schooling but the model for health care is formally the same. The utility associated with each alternative *j* can be represented as:

$$\text{(A5.1)} \quad U_{ij} = U_{ij}(S_{ij}, C_{ij}) + e_{ij}$$

where S_{ij} is the increment to child *i*'s human capital from another year of education at school *j* and C_{ij} is the level of household consumption possible when the child is sent to this school alternative. e_{ij} is a disturbance term representing unobserved determinants of utility from option j. In practice S_{ij} is not usually observed (the same applies to health improvement from treatment in the health care choice case) so instead a reduced form equation for utility is posited such as the following:

$$\text{(A5.2)} \quad U_{ij} = \alpha Z_j + \beta_j X_j + e_{ij}$$

where Z_j is a vector of characteristics of provider *j*, including price, distance, and quality attributes that determine S_{ij}. X_j is a vector of individual, household and community variables. Note that the coefficients β have *j* subscripts, meaning that the effects of the variables vary depending on the alternative. This familiar formulation is necessary because the X_j themselves do not vary over alternatives. Since the choice of provider is based on differences in utilities from different alternatives, the $\beta_j X_j$ would drop out of the decision rule unless the β_j were permitted to vary over alternatives. On the other hand, the variables in Z_j do vary over options so there is no need to index the α on j, and indeed it was argued early on in this literature that to index on alternative implies inconsistent preferences (Gertler et al. 1987). However, Dow (1999) has recently provided an argument against this view. For reasons explained in the text, we index the α on the options in one case (primary schooling) but not the other (health care).

The probability of choosing an option j is the probability that utility from j exceeds that from all other options, i.e., $P(j) = P(U_j > U_k)$, all $j \neq k$. As in many recent studies of provider choice, we estimate these choice probabilities as nested multinomial logits. This is a generalization of the multinomial logit model that allows error terms to be correlated across alternatives within a subgroup of related choices but not across subgroups (Maddala 1983). Following standard practice we assume that the error terms of the schooling choices, which in the present case consist of public school and private school, are correlated. Letting K=3 be the total number of alternatives and numbering them 1 for non-enrollment, 2 for public school, and 3 for private school, the probability of choosing option j from among the choices in the school subgroup (2,3) takes the form:

$$\text{(A5.3)} \qquad \Pr ob_j = \frac{\exp\left(\frac{V_j}{\sigma}\right)\left[\sum_{k=2}^{K}\exp\left(\frac{V_k}{\sigma}\right)\right]^{(\sigma-1)}}{\exp(V_1)+\left[\sum_{k=2}^{K}\exp\left(\frac{V_k}{\sigma}\right)\right]^{\sigma}}$$

where σ-1 is the correlation in the error terms for private and public school. A value σ outside the 0,1 range is an indication that the nesting structure grouping public and private choices together is inappropriate. If σ equals 1 the correlation of the error terms is zero and the model reduces to the simple non-nested multinomial logit model. As indicated in the text, these probability expressions are adjusted as needed to accommodate the fact that all individuals do not have the same number of schooling (or health care) options from which to choose.

Appendix 5.2: Regression Results for Education and Health Care

Table A5.2.1 - Madagascar primary school choice model for children 6-12: female and male marginal effects and their differences

	Girls		Boys			
Variable/outcome	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
Log household expenditures:						
public school	0.032	1.010	0.169	3.474	-0.136	-2.342
private school	0.034	2.390	0.037	2.675	-0.003	-0.150
any primary school	0.067	1.995	0.206	3.974	-0.139	-2.259
Public school distance						
public school	-0.127	-2.787	-0.084	-2.190	-0.043	-0.726
private school	0.023	2.124	0.016	1.555	0.007	0.432
any primary school	-0.105	-2.915	-0.068	-2.343	-0.037	-0.800
Public school cost:						
public school	-1.560E-03	-3.135	-1.140E-03	-2.586	-4.100E-04	-0.624
private school	2.200E-04	2.165	1.400E-04	1.387	8.000E-05	0.539
any primary school	-1.340E-03	-3.272	-1.000E-03	-2.801	-3.400E-04	-0.621
Private school cost:						
public school	3.000E-05	0.526	1.100E-04	1.471	-8.000E-05	-0.890
private school	-5.000E-05	-0.664	-1.600E-04	-1.752	1.200E-04	0.970
any primary school	-2.000E-05	-0.961	-6.000E-05	-2.290	4.000E-05	1.075
Public school multigrade teaching:						
public school	-0.145	-3.473	-0.096	-2.281	-0.049	-0.818
private school	0.026	2.450	0.019	1.564	0.007	0.453
any primary school	-0.119	-3.679	-0.078	-2.476	-0.041	-0.918
Public school window condition:						
public school	0.100	1.453	0.198	2.554	-0.098	-0.941
private school	-0.018	-1.303	-0.038	-1.790	0.020	0.803
any primary school	0.082	1.478	0.160	2.690	-0.077	-0.949
Public school building condition:						
public school	-0.014	-0.426	0.059	1.533	-0.073	-1.437
private school	0.003	0.420	-0.011	-1.226	0.014	1.257
any primary school	-0.012	-0.427	0.048	1.599	-0.059	-1.467
Mother primary education:						
public school	0.155	2.990	0.058	1.669	0.097	1.552
private school	-0.028	-1.535	0.014	0.701	-0.043	-1.544
any primary school	0.127	2.799	0.073	2.058	0.055	0.947

Mother secondary education:						
public school	0.259	2.638	0.207	2.570	0.052	0.406
private school	-0.012	-0.478	0.020	0.603	-0.032	-0.769
any primary school	0.247	2.628	0.227	2.578	0.020	0.154
Father primary education:						
public school	0.051	1.385	0.074	1.595	-0.024	-0.400
private school	0.033	1.713	0.045	1.720	-0.013	-0.385
any primary school	0.083	2.157	0.120	2.903	-0.036	-0.641
Father secondary education:						
public school	0.248	3.080	0.232	2.582	0.016	0.135
private school	0.062	2.358	0.072	2.336	-0.009	-0.231
any primary school	0.311	3.465	0.304	3.473	0.007	0.055

Note: Based on nested multinomial logit model estimates. For a unit change in the indicated variable, shows the change in probability of public enrollment, private enrollment, and overall primary enrollment. This and subsequent tables show the sample averages of the marginal effects for the girl or boy samples.

Standard errors calculated using the delta method

Table A5.2.2 – Uganda current primary enrollment probit model, children 6-12: female and male marginal effects and their differences

	Girls		Boys			
Variable	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
Intercept	-0.401	-37.274	-0.347	-40.822	-0.054	-3.973
Age	0.385	8.237	0.385	9.894	0.000	-0.004
(Age)2	-0.018	-7.098	-0.018	-7.947	-0.001	-0.234
Father less than primary	0.074	2.751	0.043	1.636	0.031	0.830
Father completed primary	0.167	5.538	0.104	3.390	0.063	1.467
Father secondary plus	0.204	6.599	0.092	2.722	0.112	2.447
Mother less than primary	0.088	4.049	0.101	4.866	-0.014	-0.457
Mother completed primary	0.190	5.110	0.144	3.366	0.046	0.812
Mother secondary plus	0.106	2.425	0.115	2.524	-0.009	-0.136
Fostered in child	-0.131	-5.228	-0.040	-1.248	-0.091	-2.216
# children <5	-0.001	-0.157	0.017	2.063	-0.019	-1.603
# males 6-16	-0.008	-1.004	0.006	0.686	-0.014	-1.185
# males 17+	0.011	1.044	-0.015	-1.491	0.025	1.788
# females 6-16	0.005	0.461	0.016	1.760	-0.011	-0.814
# females 17+	0.013	1.102	0.019	1.475	-0.006	-0.359
Log household expend.	0.108	6.346	0.102	5.277	0.006	0.243
Distance to school (km)	-0.025	-3.919	-0.020	-3.449	-0.005	-0.614
School costs	0.000	0.451	0.000	1.302	0.000	-0.591
Share teachers qualified	-0.078	-1.932	-0.037	-1.040	-0.040	-0.750
# shifts/day	0.015	0.449	-0.024	-1.215	0.038	1.013
Bldg. maintenance	0.007	0.396	0.012	0.636	-0.004	-0.167

Notes: 1/ Based on probit model estimates. For continuous variables, shows the derivative of the probability with respect to the variables. For discrete variables, shows the difference in probability for 0,1 values of the variable. Standard errors calculated using the delta method.

2/ Model also includes region dummies.

Table A5.2.3 – Madagascar current secondary enrollment probit model, children 14-18: female and male marginal effects and their differences

	Girls		Boys			
Variable	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
Intercept	-0.333	-0.909	-0.621	-2.104	0.287	0.610
Age	-0.027	-3.337	-0.017	-2.048	-0.010	-0.856
log household expend.	0.051	2.848	0.056	2.840	-0.006	-0.206
# children <5	-0.024	-2.060	0.001	0.116	-0.025	-1.512
# children 5-14	0.023	2.953	0.001	0.127	0.022	2.001
# males 15-20	0.016	1.328	-0.001	-0.068	0.017	0.943
# females 15-20	0.012	0.839	0.051	2.841	-0.039	-1.710
# females 21+	0.011	0.769	0.019	1.541	-0.009	-0.454
# males 21+	0.000	0.028	-0.007	-0.506	0.008	0.384
Mother primary	0.038	1.622	0.080	2.983	-0.042	-1.186
Mother secondary plus	0.146	1.866	0.294	3.204	-0.148	-1.230
Mother ed missing	-0.099	-2.662	0.053	0.500	-0.152	-1.363
Father primary	0.045	1.804	-0.012	-0.444	0.057	1.529
Father secondary plus	0.236	3.392	0.125	1.831	0.111	1.138
Father ed missing	0.115	1.368	-0.073	-1.584	0.189	1.960
Distance to lower secondary school (km)	-0.007	-3.828	-0.007	-3.324	-0.001	-0.272
Distance to upper secondary school (km)	0.000	-0.102	-0.001	-1.525	0.001	0.935
Paved road in village	0.053	2.046	0.033	1.252	0.021	0.563

Notes: 1/ Based on probit model estimates. For continuous variables, shows the derivative of the probability with respect to the variables. For discrete variables, shows the difference in probability for 0,1 values of the variable. Standard errors calculated

2/ Model also includes region dummies.

Table A5.2.4 – Uganda current secondary enrollment probit model, children 14-18: female and male marginal effects and their differences

	Girls		Boys			
Variable	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
Intercept	-0.855	-84.990	-0.804	-64.627	-0.051	-3.212
Age	0.012	2.002	0.062	6.259	-0.050	-4.340
Father some primary	0.006	0.208	0.081	2.578	-0.076	-1.820
Father completed primary	0.115	2.912	0.126	2.687	-0.011	-0.178
Father secondary plus	0.159	3.590	0.101	2.103	0.059	0.901
Mother some primary	0.021	0.792	0.008	0.317	0.013	0.367
Mother completed primary	0.060	1.482	0.125	2.899	-0.065	-1.109
Mother secondary plus	0.008	0.267	0.130	2.374	-0.122	-1.920
Fostered in child	-0.041	-2.002	-0.030	-1.238	-0.012	-0.369
# children <5	-0.015	-1.913	0.013	1.421	-0.027	-2.328
# males 6-16	0.012	1.701	0.018	1.412	-0.006	-0.390
# females 6-16	0.013	1.495	0.015	1.810	-0.002	-0.155
# males 17+	0.014	1.713	-0.003	-0.364	0.017	1.436
# females 17+	0.027	3.413	-0.005	-0.528	0.032	2.497
log household expend.	0.064	5.155	0.128	7.190	-0.064	-2.952
distance to secondary (km)	-0.005	-2.798	-0.008	-4.195	0.003	0.992

Notes: 1/ Based on probit model estimates. For continuous variables, shows the derivative of the probability with respect to the variables. For discrete variables, shows the difference in probability for 0,1 values of the variable. Standard errors calculated

2/ Model also includes region dummies.

Table A5.2.5 – Madagascar health care provider choice model, adults 15+: female and male marginal effects and their differences

	women		men			
Variable/outcome	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
Log household expenditures:						
hospital	0.0089	0.474	0.0272	1.433	-0.0183	-0.686
primary care facility	0.0204	1.106	0.0161	0.693	0.0043	0.144
formal private care	0.0316	2.839	0.0148	1.628	0.0168	1.166
any formal care	0.0608	2.538	0.0581	2.273	0.0028	0.079
Distance to primary facility:						
Hospital	0.0005	3.109	0.0003	2.037	0.0002	0.702
primary care facility	-0.0074	-3.594	-0.0040	-2.183	-0.0034	-1.242
formal private care	0.0007	2.948	0.0004	1.943	0.0003	1.102
any formal care	-0.0063	-3.555	-0.0034	-2.180	-0.0030	-1.263
Consultation cost at primary facility:						
Hospital	-0.0001	-0.577	0.0000	-0.097	0.0000	-0.268
primary care facility	0.0008	0.581	0.0001	0.097	0.0006	0.326
formal private care	-0.0001	-0.579	0.0000	-0.098	-0.0001	-0.332
any formal care	0.0007	0.580	0.0001	0.097	0.0006	0.329
Availability of free malaria medicines at primary facility:						
hospital	-0.0003	-0.229	-0.0027	-1.679	0.0024	1.212
primary care facility	0.0045	0.229	0.0356	1.707	-0.0311	-1.082
formal private care	-0.0004	-0.231	-0.0031	-1.724	0.0027	1.072
any formal care	0.0038	0.228	0.0298	1.685	-0.0259	-1.063

Notes: 1/ Based on multinomial logit model estimates. For a unit change in the indicated variable, shows the change in the probability of treatment at public hospital, public primary care facility, private formal care (doctor/clinic) and any formal care.

2/ Standard errors calculated using the delta method

Table A5.2.6 – Madagascar heath care provider choice model, children under 15: female and male marginal effects and their differences

	Girls		Boys			
Variable/outcome	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
Log household expenditures:						
Hospital	-0.0343	-1.227	0.0044	0.213	-0.0386	-1.115
Primary care facility	-0.0057	-0.213	0.0180	0.785	-0.0237	-0.673
formal private care	0.0080	0.493	0.0093	0.805	-0.0013	-0.065
any formal care	-0.0320	-0.995	0.0317	1.151	-0.0637	-1.504
Distance to primary facility:						
Hospital	0.00008	0.578	0.00028	2.194	-0.0002	-1.071
primary care facility	-0.00106	-0.577	-0.00353	-2.286	0.00248	1.034
formal private care	0.00007	0.572	0.00031	2.099	-0.00024	-1.274
any formal care	-0.00091	-0.577	-0.00294	-2.278	0.00203	0.998
Cost at primary facility:						
Hospital	0.0000	-0.028	0.0006	2.372	-0.0006	-2.199
primary care facility	0.0000	0.028	-0.0071	-2.582	0.0072	2.34
formal private care	0.0000	-0.028	0.0006	2.417	-0.0006	-2.303
any formal care	0.0000	0.028	-0.0060	-2.567	0.0060	2.314
Doctor present at primary facility:						
Hospital	0.0013	0.509	-0.0042	-1.645	0.0055	1.532
primary care facility	-0.0170	-0.510	0.0533	1.796	-0.0703	-1.574
formal private care	0.0011	0.505	-0.0047	-1.753	0.0058	1.677
any formal care	-0.0147	-0.510	0.0444	1.797	-0.0590	-1.558
Refrigerator in use at primary facility						
hospital	-0.0014	-0.364	-0.0084	-2.379	0.0070	1.348
primary care facility	0.0185	0.364	0.1061	2.502	-0.0876	-1.322
formal private care	-0.0012	-0.363	-0.0094	-2.257	0.0082	1.538
any formal care	0.0159	0.363	0.0883	2.492	-0.0724	-1.285
Schooling of household head						
hospital	0.0069	1.747	-0.0014	-0.374	0.0083	1.532
primary care facility	0.0017	0.446	0.0034	1.056	-0.0018	-0.359
formal private care	0.0037	1.662	0.0012	0.752	0.0024	0.875
any formal care	0.0122	2.653	0.0033	0.781	0.0089	1.422

Notes: 1/ Based on multinomial logit model estimates. For a unit change in the indicated variable, shows the change in the probability of treatment at public hospital, public primary care facility, private formal care (doctor/clinic) and any formal care.

2/ Standard errors calculated using the delta method

Table A.5.2.7 – Uganda: determinants of treatment at a health facility by adults age 15+ reporting an illness: female and male probit marginal effects and their differences

	Women		Men			
Variable	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
Intercept	-0.4922	-30.258	-0.4665	-27.718	-0.0257	-1.098
Age	-0.0017	-1.736	-0.0009	-0.981	-0.0008	-0.595
some primary	0.0532	1.528	0.0385	0.988	0.0147	0.281
completed primary	0.0736	1.467	0.0931	1.809	-0.0195	-0.271
secondary plus	0.0684	0.851	0.1406	2.199	-0.0723	-0.704
# females 17+	0.0188	1.143	0.0266	1.122	-0.0079	-0.272
# males 17+	-0.0172	-0.990	-0.0342	-1.837	0.0170	0.667
# females 6-16	0.0238	1.930	0.0546	3.680	-0.0308	-1.595
# males 6-16	0.0275	2.104	0.0413	3.097	-0.0138	-0.738
# children <5	-0.0040	-0.213	-0.0440	-2.179	0.0399	1.445
log household expend.	0.1556	6.476	0.1328	4.849	0.0228	0.626
Distance to provider	-0.0029	-1.243	0.0004	0.138	-0.0033	-0.873
cost	0.0000	0.024	-0.0001	-1.773	0.0001	1.191
doctor present	-0.0164	-0.783	0.0181	0.781	-0.0345	-1.104
nurse present	0.0255	0.850	0.0049	0.139	0.0206	0.447
malaria meds avail.	-0.0374	-1.032	0.0832	2.248	-0.1206	-2.328
bldg. maintenance	-0.0001	-0.002	-0.0866	-2.269	0.0866	1.605
Refrigerator	0.0334	0.910	-0.0169	-0.448	0.0503	0.956

Notes: 1/ Based on probit model estimates. For continuous variables, shows the derivative of the probability with respect to the variables. For discrete variables, shows the difference in probability for 0,1 values of the variable. Standard errors calculated using the delta method.

2/ Model also includes region and season dummies.

Table A.5.2.8 – Uganda: determinants of treatment at a health facility by children under 15 reporting an illness: female and male probit marginal effects and their differences

	Girls		Boys			
Variable	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
Intercept	-0.442	-24.809	-0.438	-17.744	-0.004	-0.118
Age	-0.012	-3.118	-0.018	-4.768	0.006	1.105
Head some primary	-0.047	-1.041	0.038	0.932	-0.085	-1.397
Head completed prim.	-0.005	-0.106	-0.004	-0.083	-0.002	-0.024
Head secondary plus	-0.214	-2.773	0.101	1.302	-0.315	-2.877
# females 17+	0.020	0.999	0.053	2.784	-0.033	-1.201
# males 17+	-0.013	-0.637	0.041	2.153	-0.054	-1.928
# females 6-16	0.003	0.214	0.013	0.837	-0.010	-0.470
# males 6-16	0.025	1.491	0.016	1.350	0.009	0.437
# children <5	0.001	0.039	-0.012	-0.567	0.012	0.443
log household expend.	0.176	5.033	0.078	2.618	0.098	2.137
Distance to provider	-0.001	-0.417	-0.002	-0.917	0.000	0.138
cost	-6.0E-05	-0.607	-6.0E-05	-1.601	0.000	0.023
doctor present	-0.018	-0.732	0.005	0.197	-0.022	-0.655
nurse present	-0.010	-0.330	-0.023	-0.634	0.013	0.286
malaria meds avail.	-0.055	-1.324	-0.065	-1.508	0.010	0.163
bldg. maintenance	0.018	0.446	0.013	0.335	0.005	0.090
Refrigerator	-0.004	-0.093	0.038	0.967	-0.042	-0.719

Notes: 1/ Based on probit model estimates. For continuous variables, shows the derivative of the probability with respect to the variables. For discrete variables, shows the difference in probability for 0,1 values of the variable. Standard errors calculated using the delta method.

2/ Model also includes region and season dummies.

6. Water Infrastructure Investments and Time Allocation in Uganda and Madagascar

In this section we present an econometric analysis of the impacts of water supply infrastructure on female and male time use in two of our sample countries: Madagascar (using the 1993 EPM survey) and Uganda (using the 1992 IHS survey). These countries and survey years were chosen because they feature detailed survey data on time use, including the time devoted to water collection. This allows us to implement the methods for analyzing water supply impacts discussed in Section 2.3. Based on that discussion, we consider the effects on time in the following activities: water collection itself, all domestic activities, market oriented work, and leisure.

We begin by discussing the data in more detail and the methodology used in the analysis. We then present some descriptive statistics on water supply and time allocation, followed by the econometric analysis. The concluding section discusses the implications of the findings for investment policies in the water sector. In particular, in view of the fact that the burden (especially) of water collection as well as of overall work is higher for females, we consider the potential of such policies for influencing the gender distribution of the burden of work and leisure.

6.1 Data and Methodology

The information on time use was collected in different ways in the two surveys. The 1993 EPM follows a more standard practice of asking how many hours during the past week the individual spent in specific activities, such as housework, collecting water, collecting wood, and child care. The 1992 IHS time use module is more elaborate, and presumably, more accurate. From a long list of potential activities, individuals were asked to indicate the number of hours spent doing each activity in each of the last 3 days; the sum per day must add to an assumed 12 'active' hours.[80] We use the average per day over the three days for our analysis and multiply by 7 to get weekly figures comparable to the Madagascar data.

Both data sets contain information on the drinking water source used by household members. The Madagascar survey, but not the Uganda survey, also records the distance to this source. Both household surveys are also complimented by community surveys that collect information on local infrastructure. Though these do not include detailed information on water infrastructure, they do include a number of characteristics that should affect time use (both in domestic and market activities) such as presence of

[80] Note this is not a 'time-diary' method since individuals are not asked to provide an actual chronology of activities in the day. The IHS approach is nevertheless more detailed than a standard time use module. Still it has some shortcomings: the 12 hour limit is arbitrary, and like the standard questionnaire, the structure does not allow for the recording of time spent simultaneously engaged in multiple tasks, e.g., house cleaning and childcare.

markets and roads and availability of electricity. These variables representing economic and infrastructural development are important to include as controls in the models, especially since they are likely to be correlated with the water infrastructure variables. Unfortunately, we are only able to include these covariates in the Madagascar analysis (for which only rural community data were collected), because of a difficulty in the Uganda data of matching households to specific communities listed in the community level files. Our models of time use also include standard household and individual determinants, e.g., age, schooling, and household composition.

The first dependent variable considered in the analysis is hours in water collection, which shows what might be termed the direct effect of water supply investments. The second outcome variable, hours in all domestic work, is inclusive of water collection time: hence the coefficients on water supply variables in the regression will measure their effects on the total burden of domestic work, incorporating substitution into or from other household tasks. ‘Market work’ includes hours in wage or self-employment activities (including family agriculture). Finally, ‘all work’ is the sum of hours in both domestic and market activities. Therefore this last model estimates the impact on the total burden of work, after reallocations among the full set of productive activities. This of course is the negative of the impact on leisure hours.

For each of these activities, at least some individuals report no time in the activity in the reference period, i.e., there is censoring of the dependent variable at zero. For example, 54 percent of females and only 10 percent of males age 15 and older engaged in water collection in the 3 reference days in Uganda. To deal with censoring, we estimate the determinants of these outcomes using the tobit model. The model can be expressed, for observation *i,* as

$$Y_i = \beta' x_i + u_i \quad \text{if} \quad \beta' x_i + u_i > 0$$
$$Y_i = 0 \quad \text{otherwise}$$

where Y_i is the outcome variable, say hours in water collection, β is a vector of parameters, x_i is the vector of explanatory variables, and u_i is an error term, assumed to be independently and normally distributed. A well known potential limitation of tobit is its implicit assumption of a strict relationship between the determinants of the probability of observing a non-zero value of a continuous variable (e.g., the probability that the individual participates in water collection) and the determinants of the level of that variable (hours of water collection) conditional on being above the zero limit (Mroz 1987). Essentially, the two decisions are considered to be the same. The model introduced by Heckman (1979) provides a more flexible alternative that distinguishes participation and hours decisions. Ilahi and Grimard (2000) apply this procedure in their study of water supply and time use in Pakistan. However, identification in this model requires that there be variables determining participation that do not influence hours conditional on participation. No plausible candidates for such variables exist in our datasets, so we use tobit in this analysis.

Section 3.2. above discussed alternative specifications of the water supply variables. As noted, datasets typically lack direct information on the availability of specific sources in the community, or household level information on distances to each relevant source, and this is the case for our surveys. Instead, we construct indicators of availability for different water sources as follows: the indicator for source *j* equals one if any household in the cluster reports using the source and zero otherwise. The coefficients on these indicators can be interpreted, broadly speaking, as showing the average effect on time use of investments in specific types of water supply infrastructure (wells, public taps, etc.) controlling for household and other community factors. Using these indicators we estimate the time allocation effects of providing wells in rural communities and of providing exterior taps and access to individual piped connections (interior taps) in urban areas.

The validity of our interpretation of these estimates depends on several factors. First, the cluster or primary sampling unit of the survey should correspond to a 'community' or village, since one can most easily imagine policies directed toward this administrative or geographical unit. The practicalities of survey sample design (including in our two cases) usually insures this, at least for rural areas: for cost reasons, interviewed households are usually grouped into clusters corresponding to villages. Second, the households in the sample must be drawn randomly from the community or cluster (which is almost always the case in principal), and the number per cluster should be adequately large--both serving to minimize the risk of failing to sample from among the subgroups of households using each available choice. In Madagascar each sample cluster has 16 households while for Uganda most have 10. Certainly for the latter case, we need to be somewhat cautious about assuming that we are correctly assigning as 'unavailable' any source that is not used by any of the households visited in the cluster.

Third, a household may report using a source even if is not available in its community, in which case the indicator would not represent the local investment we seek to measure. We can assess this for Madagascar, where we can look at the distribution of reported distance to source, if we make some assumptions about how large in area a cluster or community is. The median distance to well and lake/river sources among households using these alternatives is the same (and not very large): 100 meters (Table 6.1). 90% of households using lake/river sources walk less than 400 meters. For wells, the 90^{th} and 95^{th} percentiles are about 2000 meters and 4000 meters, respectively. Therefore the great majority of households using either of these sources do not travel very far, and even the upper end of the distribution for wells probably still represents travel within the area of a village.

Therefore we can be reasonably assured for this rural sample that our availability indicator does measure the presence of the water supply source in the community in which the household is located. In urban areas this is harder to judge. While villages tend to be discrete geographical units, in urban areas communities are generally contiguous. Still, reported distances are within what could be considered the range of a *quartier* or neighborhood: 90% of all urban well users in Madagascar walk less than 150 meters (median=20 meters) and 90% of exterior tap users walk less than 300 meters (median=100 meters).

Table 6.1 – Household water source in Madagascar: Frequencies and distances from domicile, by area

Rural

	lake/river/ stream	well	exterior tap (public)	exterior tap (private)	Other	Total
No. of households	1888	518	137	30	63	2636
mean distance	167	335	77	28	98	192
median distance	100	100	50	25	20	100

Urban

	Interior tap	exterior tap (public)	exterior tap (private)	well	lake/river/ stream	other	Total
No. of households	356	907	99	336	117	37	1852
mean distance	0	141	50	76	83	33	92
median distance	0	100	10	20	50	10	50

Note: distance is in meters

Another estimation approach discussed in section 3.2 is to use these availability indicators as instruments for the actual source used by the household in structural models of time use. This approach indicates the effect of household use of the source, controlling for the endogeneity of this choice. As discussed, these structural estimates tend to have less policy content than the reduced form estimates. Water infrastructure investments usually merely make a source available; they do not insure that all households in the catchment area will make use of it. An exception to this would be a subsidy for connections to the water system, that is, a public investment that provided hookups to every domicile in a neighborhood, so that all residents would have access to (and of course, would make use of) internal piped water. Therefore for urban areas we also estimate a structural model of the impacts on time use of the household having an interior tap.[81]

[81] As our discussion implies, the instrument is availability of interior taps in the cluster as defined in the text. Other community infrastructure variables that might be considered as instruments are not available for urban areas (the community survey was restricted to rural clusters). Even if they were, however, their validity as instruments in structural models of time use would be doubtful: most such infrastructure variables (e.g., distance to roads) would likely have direct effects on time use.

Finally, in Section 3.2 we discussed why regressions including the distance to the source actually used by the household (available for our Madagascar sample) is problematic. Without accounting for choice among alternative sources that have attributes other than distance (e.g., water quality, price) and that households also value, the estimated effect of distance to the chosen source is not very meaningful for policy. What we want instead is to be able to estimate the effect of constructing a specific water source nearer to households. Since the problem arises from the endogeneity of water source choices, an alternative is to instrument household distance using cluster mean distance. We implement this methodology too, specifically using (as Ilahi and Grimard (2000) do) 'leave-out' cluster means as the instrument.[82] The effect of distance estimated this way is of some interest, though note that it too does not quite address the policy question just posed (it does not indicate the effect of changing distance to a specific source type).

6.2 Empirical Results

6.2.1 Descriptive Analysis

Table 6.2 presents descriptive data on time in water collection in Madagascar and Uganda, disaggregating by gender, age group, and rural/urban location. Women and girls spend more – usually much more – time in water collection than men and boys. Another clear and expected pattern is that hours per week in water collection are higher in rural than in urban areas. For example, women 15 and older in rural areas of Madagascar spend an average of 3.8 hours in this activity compared with 2.3 hours for their urban counterparts; the analogous figures for Uganda are 3.5 and 2.2 hours.

Table 6.2 also shows that the time of children in water collection can approach or even, in the case of Uganda, exceed that of adults. Girls 10-14 in Uganda actually spend more hours per week collecting water than do women 15 and older; boys' time is somewhat lower than girls, but boys still spend much more time than men.[83]

Another impression one gets from the table is that hours in water collection are neither trivial nor exceedingly large in absolute terms. This is the case even for rural areas. To put these hours in perspective, note that water collection accounts for about 19% of rural women's total domestic work hours in Madagascar and 12% in rural Uganda. It figures more prominently in girls' (lower) total domestic work: 33% of all domestic activity of girls 7-14 in Madagascar and 22% of domestic activity of girls 10-14 in Uganda. We

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[82] The cluster level leave-out mean for household *i* is the mean of the variable calculated over all households in the cluster other than *i*; this is done so the mean is not contaminated by *i*'s own choices, i.e, is not endogenous to the household choice in this sense.

[83] Note that the age range for children in Madagascar is 7-14 as compared with only 10-14 in the Uganda survey, which collected information on time use only for individuals 10 and older. If we restrict the children's sample in Madagascar to ages 10-14, water collection hours for girls become similar but not greater than those for women 15 and older.

should note as well that the hours in water collection of rural women in our samples are broadly in line with the 11.4 hours per month reported for rural Pakistan by Ilahi and Grimard (2000).[84]

Table 6.2 – Weekly hours in water collection by area, sex, and age: Madagascar and Uganda

	Women 15+	Men 15+	Girls[1]	Boys[1]
		Madagascar		
Rural	3.84	0.57	2.99	1.15
Urban	2.29	0.93	2.22	1.64
		Uganda		
Rural	3.54	1.22	5.36	4.46
Urban	2.19	0.90	4.33	3.69

Note: [1]Reflecting the different structure of the questionnaires, ages for girls and boys are 7-14 for Madagascar and 10-14 for Uganda.
Source: 1993/4 Madagascar EPM; 1992 Uganda HIS

The burden of water collection clearly falls disproportionately upon women and girls. So does the burden of overall domestic work (see appendix Table A.6.1a) What about the overall burden of work, including all domestic and market labor? This comparison is important for assessing the desirability of water supply investments that have different effects on time use by gender. In both countries male hours in market-oriented activities tend to exceed those of females, though participation rates of women are by no means low, especially in rural areas. However, because they perform so much more home work than men, the total work burden is larger for women (Table A.6.1c). These gaps are largest in rural areas and are especially pronounced in Uganda, where women work 55 hours per week in all activities compared with only 41 hours for men. The figures for rural Madagascar are 46 hours for women and 41 hours for men. Such gender gaps are commonly found in developing country time use data (United Nations 1995). Therefore there is interest in policies that can reduce the overall work burdens of women, both absolutely and, from the perspective of gender equity, relative to men.

[84] Our data are also consistent with other survey data from our two countries. In Madagascar, for example, the small-scale surveys conducted by Minten et al. (2002) indicate a rural mean travel time to water source (always a lake or river in the villages sampled) of just 12 minutes. In urban areas sampled, the average time to get water, including travel and queuing, for households that lacked their own connections was between 12 and 23 minutes.

Next we look at average hours in water collection by household water source. We consider rural areas first in Table 6.3. The categorizations of water source in the table (and in the subsequent regressions) for each country differ slightly, reflecting the different structures of the surveys and the need to aggregate small categories. For Madagascar a small but non-trivial portion (6%) of rural households report using a tap; in over 90% of these cases this is an exterior tap, and is usually public rather than private. For women and girls in these households water collection time is low. The bulk of rural households in both countries, however, get their water either from a well or from natural sources such as a lake, river or stream. In Uganda, wells are the most common rural source while for Madagascar it is lakes/rivers/streams.

Table 6.3 – Weekly hours in water collection by source in rural areas: Madagascar and Uganda

Madagascar

	lake/river/ stream	Well	exterior tap (public)	exterior tap (private)	Other
No. of households	1888	518	137	30	63
hours:					
Women 15+	4.0	4.0	2.6	1.7	2.6
Men 15+	0.5	0.9	0.7	0.4	0.8

Uganda

	lake/river/ stream	Well	exterior tap (public or priv.)	Other
No. of households	2647	3515	119	113
hours:				
Women 15+	3.9	3.4	2.2	2.9
Men 15+	1.1	1.3	1.0	1.5

Table 6.4 – Weekly hours in water collection by source in urban areas: Madagascar and Uganda

Madagascar

	Interior tap	exterior tap (public)	exterior tap (private)	Well	lake/river/ stream	other
No. of households	356	907	99	336	117	37
Hours:						
Women 15+	0.2	3.0	1.7	2.6	3.2	1.1
Men 15+	0.0	1.5	1.0	0.6	0.4	0.4

Uganda

	Interior tap	exterior tap (public & private.)	well	lake/river /stream	Other
No. of households	339	891	1812	316	169
Hours:					
Women 15+	0.3	1.4	3.2	2.4	0.9
Men 15+	0.2	0.5	1.3	1.3	0.3

These figures suggest that having access to a well does not confer any time savings relative to using a lake or river source in either country. For example, in rural Madagascar, average weekly water collection time of women using these two sources is exactly the same: 4 hours. In the case of Madagascar (where we have distance data) this appears to be due to the fact that wells tend to be no closer to the domicile than lakes or rivers. As already noted (see Table 6.1), in rural areas the median reported distance to lake/river and wells, respectively, is 100 meters for both sources. Hence wells do not appear to confer a distance (or, presumably, time) advantage.[85] However, it should be recalled from section 3.2 that simple comparisons of mean water collection times or distance calculated only from observed choices of households cannot be assumed to reveal the water collection time impacts of public investments in specific water supply sources.

In urban areas (Table 6.4), the main source of water is exterior public taps in Madagascar (50 percent of urban households) and wells in Uganda (51 percent of households). Piped connections (interior taps), which are virtually absent from rural areas, are present in

[85] Wells may also involve significant waiting time (one must wait one's turn) not incurred by those using a lake or river. This would add to the time involved in using a shared well relative to natural sources.

urban areas, but are only enjoyed by a minority of households (20 percent in Madagascar and 10 percent in Uganda). Not unexpectedly, having an interior tap implies less time (basically, zero time) in water collection. Use of well or lake/river sources implies non-zero hours in water collection for urban women in both countries. Rural-urban differences should be kept in mind however, as the time spent by women in urban areas using well or natural sources is lower than for women in rural areas using these sources.

6.2.2 Econometric Estimates

To address the time use effects of public investments in water supply we turn to the econometrics analysis of water collection hours using the source availability dummies. As noted, in section 3.2, multivariate analysis is also necessary to measure the impacts of water supply on the spent time in other activities. In addition to estimating the tobit models we calculate the comparative static effects (the marginal effects) of selected water supply regressors. These indicate the effect of a unit change in the explanatory variable on the outcome variable. Since our water infrastructure variables (other than distance) are categorical, we calculate the difference in the predicted hours when the variable takes the value of one and when it is zero. These calculations are done for each observation in the sample, and we report the means of the calculated derivatives or changes in predicted value over the relevant subsample. Standard errors were calculated using the delta method (see the appendix to Section 2). These comparative static results are shown in text tables, while the numerous tobit results are put in Appendix 6.1 at the end of this section to avoid clutter.

We also will have need to calculate comparative statics for continuous regressors, namely distance to source and log household expenditures. The marginal effect for a regressor x_j in the tobit model is calculated as:

$$\partial (E(y_i))/\partial x_j \quad = \quad \Phi(\beta' x_i / \sigma)\beta_j$$

where $E(y_i)$ is the predicted value of the dependent variable, Φ denotes the standard normal cumulative distribution function, β_j is the coefficient on x_j, and σ is the standard error of the disturbances in the tobit model. Note that this is the unconditional marginal effect, that is, it accounts for the effect of the change in the regressor on the probability of having a non-zero value of the independent variable as well as the change in y_i conditional on being above the zero limit (See McDonald and Moffit 1980).

6.2.2.1 Determinants of Water Collection Time – Rural Areas

We focus first on rural areas in each country, where the time costs of water collection are larger. Table A6.1.2 shows the tobit estimates of the determinants of weekly water collection time for women (age 15 and older), girls (age 7-14), men, and boys in rural Madagascar. We note first for women that that the time in water collection is affected by many covariates other than water source. The schooling of the head of the household, likely proxying income, reduces this time. The value of household agricultural land holdings has the same effect, either because it too captures wealth or income effects, or because it raises the demand for time in family agriculture. Having more girls and women in the household strongly reduces the time a woman spends in water collection. This may because of there are a greater number of substitutes for the women's time in water collection, but it also may reflect economies of scale in this activity. If the latter are operative, the total required household time in water collection increases less than proportionately with the number of 'potential water collectors', resulting in negative coefficients on the numbers of girls and women. [86]

With regard to the water source variables, since some (15%) rural communities in Madagascar have households using piped water (generally from exterior public taps), we include availability dummies for such taps in addition to wells (the dummy for the latter equals 1 for 55% of the rural sample). Neither indicator is a significant determinant of women's water collection time in rural Madagascar. Since almost all of these communities have access to natural sources, these estimates are saying that the availability of either well or exterior tap sources in the community does not reduce the time involved in water collection compared to areas where only natural sources are used.

Similarly, in the rural girls' water time regression for Madagascar, as with the women's regression, neither tap or well availability reduces the time for water collection. In fact, there is a positive and marginally significant coefficient on the tap indicator. As seen in Table 6.1, the distance to exterior taps among rural households (at least, among those who use them) tends to be low. This would make the task of fetching water less strenuous and hence more suitable for children.

Compared with women and girls, there are few significant covariates in the rural Madagascar men's or boys' water time regressions.(Table A6.1.2 cols. 3 and 4) Since the mean hours are very low for male water time, this is not surprising. Still, men's time in water collection is reduced if there are more girls and women in the household, likely reflecting substitution of time in different tasks. With respect to the water infrastructure indicators, the only statistically significant result is a positive coefficient on well availability for men, but this is difficult to interpret in view of the low mean hours in this activity.

For rural Uganda, the results for weekly hours of water collection are presented in Table A6.1.3. Note that for only about 6% of the rural sample is the (exterior) tap availability indicator equal to 1, but more than 80% have access to well in their communities. For women (first column), as in Madagascar, neither the presence of a well or a community tap in the cluster affects time in water collection. A number of other household covariates have impacts similar to those seen for Madagascar. For example, the schooling of the head of household is associated with reduced hours in this activity, as are

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[86] While household composition covariates are used in most time use regressions found in the literature, variables such as the number of children are potentially endogenous to time use outcomes. We do not focus on this problem here, except to note that fertility is also an important control variable that is correlated with other included covariates, and that leaving it out, even if partially endogenous, may lead to biases on those estimates (See Browning 1992 for a general discussion).

the numbers of women and girls in the household. Note as well the large negative effect of log per capita expenditures.

For rural girls 10-14 in Uganda we see fewer significant parameter estimates than for women. Here, however, there are significant coefficients for water source variables: the effect of well availability is positive and that of taps is negative (though only marginally significant). Hence girl's time in water collection, but not women's, appears to be sensitive to the local water infrastructure. Among demographic covariates, only the number of boys age 6-16 is significant: having more boys in the household reduces girls' time getting water. Since boys do participate significantly in water collection in this environment (recall Table 6.2), this result is sensible and points to substitution among girls and boys in this task.

For men in rural Uganda (3rd column of Table A.6.1.3) , we find no significant impacts of either well or exterior tap availability. Few other covariates are significant except for the negative effect of the number of women, which is expected, and the strongly significant positive effect of household expenditures, which may be unexpected. However, higher household income might in part be leading to a substitution from market work to some domestic chores as well as to leisure; in any case it is worth recalling that the time men spend in water collection is very low. For boys as for girls in rural Uganda (last column), we observe a positive coefficient on well availability and a negative coefficient on exterior tap availability, but for boys the well estimate is insignificant.[87]

By and large, these rural regressions indicate that the availability of wells (and for that matter, public taps) does not lead to reductions in the time that individuals allocate to water collection. A likely reason for this in that in villages where wells or public taps have been put in, they are approximately equally close—or more to the point, no closer—to most households than the lake or river source that would be relied upon otherwise. Especially if the natural and well sources were located near each other (say at the center of the village), we would expect little average reduction in distance or time. The descriptive data on the distance to the source used by households shown above for Madagascar are consistent with this notion as they show similar median distances to natural sources and wells. As stressed, however, however, comparisons based on distances reported only by those who choose a particular source are likely to be misleading. It would be more informative to compare average (over all households, using any source) distances in communities where wells (and taps) are available and where they are not. We did this analysis in a multivariate framework, regressing household distance to water source on well availability, public tap availability, and the

[87] The regressions for both countries include controls for region and for season. The former are often highly significant, which is not surprising given regional differences in climate and hydrography (this is more the case in Madagascar) hence potentially in the accessibility of various sources of drinking water. We also explored the possibility that our measured water supply investments have a greater effect on time use in some areas than others. For Uganda, interactions of region dummies with the source indicators were insignificant for women. For girls, there is a positive but only marginally significant interaction of well availability and residence in the Eastern region. Similarly, the interactions were generally insignificant for women and girls in Madagascar.

usual set of controls to account for other differences among households and communities. In these models, neither well availability or tap availability was associated with a reduction in the distance to water source reported by households, confirming the pattern suggested by the descriptive data.[88]

6.2.2.2 Determinants of Water Collection Time – Urban areas

For the urban samples we consider the impacts on water collection time of exterior and interior tap availability. The distribution of these indicator variables leads us to specify the availability regressors slightly differently from the rural models. In urban Madagascar, the large majority (87%) of households live in communities where some tap source is available. Almost all of these communities have at least exterior taps, while about two thirds have interior taps in use as well. In very few communities do we have households reporting using interior taps but not also exterior ones, and we assume in these cases the latter source is also available. Therefore we can specify the availability indicators as a set of mutually exclusive dummies: no tap source; exterior tap only available; and interior (plus exterior) taps available. We make 'exterior taps only'[89] the base category. For urban Uganda we use the same groupings, though the means are somewhat different. Only about 53% of urban communities have households using any kind of piped source, of which half have interior taps. Again, almost all communities with interior taps also have some households reporting use of an exterior tap.

Table A6.1.4 reports tobit estimates for hours in water collection in urban areas of Madagascar. For women (first column), living in an urban community where interior taps are used is associated with a statistically significant reduction in hours in water collection relative to the case of having an exterior tap only available. The coefficient on 'no tap source' is also negative though not significant, indicating that the presence of exterior taps does lead to reductions in water collection time relative to communities where tap sources are not available. Comparative statics calculations shown in Table 6.5 (col. 1) indicate that the mean predicted reduction in weekly water hours for interior over exterior only tap availability is .85 hours. To put this in perspective, note that the mean time in water collection of women in urban communities lacking interior taps is about 2.7 hours per week, so the presence of such taps implies about a 25% reduction. Again it should be kept in mind that we are estimating the *average* change for women in the community (controlling for other factors) including those in households that do not use interior taps. Below we consider two stage regression estimates of the time use impacts of interior tap use, not merely availability.

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[88] This is especially notable for taps in view of the low reported distance and water collection times for those who actually use them (Tables 6.1, 6.3). But in fact, where taps are available in rural communities in Madagascar, on average less than half (6 of 16) of the interviewed households use them, so the average effect on distance and water time of the presence of the water network, which is what the coefficient on the availability dummy captures, is low.

[89] This means 'exterior taps but not internal taps'; it does not mean that there are no other sources such as wells or river.

Table 6.5 -Urban areas: Effect of internal tap availability and predicted internal tap use on domestic, market, and all work

	Internal taps available in cluster				*Internal Tap use (predicted)*			
	Water collection	Domestic work (incl. Water)	Market work	All (Domestic + market) Work	Water collection	Domestic work (incl. Water)	Market work	All (Domestic + market) Work
				Madagascar				
Women 15+	-0.85	-1.40	-2.95	-2.37	-2.69	-4.06	-12.43	-10.83
Girls 7-14	-0.45	-1.79	-1.10	-2.11	-1.62	-4.33	-2.65	-5.84
				Uganda				
Women 15+	-0.59	0.67	0.23	1.28	-1.80	1.40	1.28	4.06
Girls 10-14	-3.47	-4.04	3.11	0.98	-5.43	-9.48	9.82	0.47
Men 15+	-0.58	-0.52	-1.89	-3.95	-1.11	-1.20	-5.36	-7.98
Boys 10-14	-1.67	-4.91	-4.31	-9.12	-3.70	-7.90	-6.39	-16.09

Note: Left side panel shows effect (change in predicted weekly hours in the activity) of having internal taps available relative to only external taps available in cluster. Right panel shows effect of predicted internal tap use. Light shading:effect significant at 10%; Dark shading: significant at 5%

In contrast to women, for urban Malagasy girls there is no water time reduction associated with interior tap availability. Among other covariates, as in the rural sample, women's and girls' water collection time is reduced when there are more women and girls in the household (girl's water time is reduced as well by having more boys in the household). Water collection time of both women and girls also falls with the level of household expenditures.

For urban men and boys in Madagascar (A6.1.4 cols. 3 and 4), the availability of interior hookups in the cluster does not have significant impacts on water time (which, it should recalled, is low relative to female time). The coefficient on 'no tap sources' is negative for both men and boys, indicating as for females that the presence of exterior taps does not reduce average male time in water collection and instead may increase it. This result is not implausible, because queuing may be a factor at public taps but not at wells or natural sources. Among other covariates in the men and boys urban regressions, household composition does have some impacts (in expected ways), especially for men.

The urban Uganda water time tobits are shown in Table A6.1.5. Availability of interior piped connections is associated with reductions in women's and girls' water collection hours relative to the base of exterior taps only. In contrast to urban Madagascar, exterior tap access also appears to lead, for women at least, to a reduction relative to the no taps case (that is, the coefficient on 'no taps' is positive and significant). The predicted reductions in collection time from having local interior taps relative to exterior taps only is about .6 hours per week for women and 3.5 hours for girls (Table 6.5). The women's estimate is roughly comparable to that for urban Madagascar (as are the mean water hours for women in the two urban samples, as shown in Table 6.2). Although the estimated hours reduction for girls is quite large, so is mean time of girls in this activity (4.3 hours—Table 6.2). For women, the collection time reduction from access to exterior taps (relative to having no piped water source in the community at all) is about 1.5 hours. Comparatively few other covariates have significant impacts in these regressions.

We also see negative water time effects of interior tap availability for men and boys in urban Uganda (cols 3 and 4). The implied reduction of water collection time is .58 hours for men (recall the mean actual time is 0.9 hours) and 1.7 for boys (mean actual time of 3.7 hours).

Structural estimates: effect of having a piped connection

As discussed above, it is of interest to know how time allocations will change if households receive individual hookups to the system, as opposed to the average effect (over all households) of making this option available in a neighborhood. This corresponds to a plausible policy in urban areas: to provide these connections free of charge or at highly subsidized rates. To control for the endogeneity of the choice of interior tap, we predict the use of this source using the availability indicator as an instrument (See the discussion in section 3.2). The tobit results are shown in Tables A6.1.6 and A6.1.7. In urban Madagascar, household use of an interior water connection has a negative and strongly significant effect on water time for women but no effect for girls (which is expected since the instrument itself was insignificant in the reduced form model for girls). The comparative static effect for women is a reduction of 2.7 hours per week (shown in the first column in the right hand panel of Table 6.5). This is a plausible reduction, since it is essentially equal to the mean water time for urban women who do not use an interior tap, and women with piped connections in their homes should spend zero time in water collection. Use of interior taps has no impacts on the (already low) hours in water collection of men and boys.

In urban Uganda (Table A6.1.7), predicted interior tap use has large negative effects on water collection time of both women and girls 10-14. For women the reduction is 1.8 hours a week—again of the same order of magnitude as the estimate for women in urban Madagascar, and also quite plausible in light of mean water collection hours (2.3). For girls the predicted reduction of 5.4 hours is large but also is approximately equal to the reported mean hours in water collection for girls who do not use interior taps.

For men in urban Uganda, predicted use of interior taps has a strongly significant negative effect on time in water collection; the reduction in water collection time is about 1 hour per week. For boy, too, there is a negative and (at 10%) significant effect. The implied reduction in hours is large—3.7 hours—but sensible in light of the relatively large amount of time boys spend in this activity for boys (table 6.2).

Thus we do find evidence that public investments in water systems in urban areas not currently served by them—particularly investments that make piped household connections possible—will lead to reductions in time spent in water collection by women and in some cases by girls, men, and boys as well. The time benefits are usually not very large because the time in water collection in these environments tend not to be very high to begin with. Still, for girls and boys in urban Uganda water collection hours are fairly high (about 4.3 and 3.7 hours per week, respectively) and the availability of interior water connections in the neighborhood is associated with large reductions in this time. For Uganda, even exterior tap availability leads to time reductions for women over other (well or natural) sources (in urban Madagascar such taps are already pervasive). These reductions are seen even though the estimates capture the mean effect over all individuals in neighborhoods where this water source type is made available, including those using the source and those not. The predicted effect of individual use of interior taps is significantly larger, as we would expect.

For interior taps in particular, the consideration of the unconditional effect begs the question of who in these communities is more likely to benefit from the possibility of access to such a source. We would anticipate that affluent households would be more likely to have piped connections, because they are more willing to pay a connection fee or because they are more likely to buy or rent domiciles with connections in place or with easier/closer physical access to the network. Indeed, in probit models run on the sample of communities with internal taps available, household expenditure per capita had strongly significant positive impacts on the household use of interiors taps in both Madagascar and Uganda. In water time tobits in which the interior tap availability dummy was interacted with our household income measure, the interaction term was usually negative and significant, as hypothesized[90]: this was the case for each urban Madagascar subsample (women, girls, men, boys) and for girls and men in urban Uganda. This finding points to potential equity concerns with regard to policies that extend water systems in urban areas, unless the policy insured that connections were provided free of charge or at highly subsidized rates to poorer residents.[91] Note that this equity issue concerns not just the distribution of time savings among rich and poor households but also of the health benefits of piped water.

6.2.2.3 Determinants of Water Collection Time (Madagascar) – Effect of Distance to Source

In Table 6.6 we summarize results from additional water collection time tobits for Madagascar including predicted distance to source used by the household, using cluster leave-out mean distance as instruments. The table shows the effects of increasing

[90] That is, where interior taps are available, the negative impact on collection time is larger among more affluent households as they are the most likely to use them.
[91] The willingness to pay analysis of Minten et. al. (2002) finds that households are highly sensitive to potential cost of a new water source.

distance 100 meters (the tobit results themselves are suppressed to save space but are available from the authors). Perhaps surprisingly, distance is not always significantly associated with water collection time once the endogeneity of distance is controlled for. Further, where the effects are significant—in urban areas only—the implied impacts of reductions in distance are small. For example, for urban women, a 100 meter reduction in predicted distance to water (equivalent to the median distance that those who use exterior public taps actually must travel—see table 6.1) reduces weekly time in water collection by just .22 hours. For rural areas, instrumented distance simply did not have an effect on water use time. Reported (i.e., uninstrumented) distance in contrast did have significant or marginally significant positive effects for most of the rural subsamples (women, girls, and men) but even here the implied comparative static effects were very small.

Table 6.6 - Effect of predicted distance to source on weekly hours in water collection in Madagascar

	Women	Girls	Men	Boys
rural	0.051	0.012	0.017	-0.001
urban	0.217	0.396	0.083	0.359

Notes:

Shows effects of 100m increase in distance. Marginal effects calculations based on tobit estimates.
Source: 1993/4 Madagascar EPM

One possible explanation for these weak impacts of distance (other than the methodological concerns noted in section 6.1) is that, as already mentioned, water collection may involve not just walking to the source and back, but queuing at certain sources. Or, households may compensate for greater distance by making fewer trips or economizing on water use. These factors would imply that distance is not the only factor affecting water collection time.[92]

6.2.2.4 Income-Gender Interactions in the Determination of the Burden of Water Collection

The focus so far has been on the impacts of public water supply investments, proxied by availability indicators or distance. Less has been said about the effects of income, and in

[92]The notion is supported by the study of Minten et al. 2002. They report that for their urban sample, a longer travel time to the source was associated with reduced monthly per capita water consumption. This could mean that fewer trips are taken (as hypothesized in the text) or that less water is carried per trip. The survey also found that queuing at public taps is typical in urban areas—and is usually significantly more time consuming than the (often minimal) travel to the tap.

particular, whether these effects differ by gender. This was a major concern of the benefit incidence analysis in earlier sections of this paper, so we return to it here in a multivariate context. The advantage of multivariate analysis over the descriptive benefit incidence analysis is that we are better able to assign a causal interpretation to any income effects that we find, since we include controls for other factors that affect outcomes and are correlated with incomes. Still, interpretation is not as straightforward as this statement implies. The coefficient on the income variable shows what happens when income increases and other covariates are held constant. Many of the latter are themselves affected by income, and this is especially likely to be the case for water supply variables. For example, as suggested above, better off urban households are more likely to pay for piped connections into their homes if these are available where they live; or they may choose to live in such neighborhoods in the first place. If both the water infrastructure variables and income are included in the model, the coefficient on the latter captures only the effect of income net of its impacts on other factors, including location, that affect water collection time.

This would argue for excluding the water covariates to capture the 'full' or gross impacts of changes in income on water collection time. The coefficient on income would then capture the direct effect of income plus the effects coming through income-induced changes in the water related covariates. Here too, however, one must be careful. This interpretation would be valid only if the relationship between income and the excluded water supply variables is truly causal rather than a simple association; otherwise we will under- or overestimate the impact of increases in income, depending on the direction of the effect of the water supply variables that are associated with income. Given this ambiguity, we estimated both specifications, using as before log household expenditures per capita to represent the level of household resources.[93] To compare male and female impacts, we calculated the marginal effects from the tobit estimates, evaluated at the means of the data for the male and female samples. These results are shown in Table 6.7; to save space the table only shows the results excluding the water supply variables, though we note the alternative estimates in the text below.

We first discuss Uganda. Recall that the analysis of gender/quantile shares in water collection for Uganda in Section 4.2.7 indicated that female shares of time in this activity (out of the total time in the activity in the population) fell moderately with expenditure quintile while male shares did not; that is, the gender gap in the burden of water collection is smaller the higher the quintile. In the tobit regressions household per capita expenditures has negative and generally significant effects on water collection time of women and girls, in both urban and rural areas. Since water collection is work, these negative effects are consistent with leisure being a normal good. For rural men expenditures has a significantly *positive* impact and we can easily reject equality of the men's and women's marginal effects (last column): in other words, the gender difference

[93] A separate issue we are not able to address satisfactorily is the endogeneity of household expenditures to time use outcomes. Non-labor income would have a greater claim to exogeneity but, perhaps because it has much less variation (it equals zero for many households) this variable was usually not significant in the water time regressions. Another reason for using expenditures is that it facilitates comparison with the descriptive benefit incidence results, for which per capita expenditures were used as the welfare proxy.

in water collection time falls with expenditures, consistent with the earlier analysis. In the case of rural boys, in contrast, the expenditure effect is negative and significant, and in this case the marginal effect is virtually identical to that for girls. Recall from Table 6.2 that in rural areas of Uganda both boys and girls have a significant burden of work collecting water. These tobit results suggest that they will receive equivalent direct (water collection time) benefits from increases in household resources.

In the urban Uganda sample, there is a negative impact of expenditures on adult female water collection time and essentially no effect on male time, and the difference is significant at the 10% level. For adults, therefore, both the rural and urban Uganda estimates suggest that as household income rises, the large overall female-male gap in the time burden of water collection will fall. This is further confirmed by regressions (not shown) that, like the descriptive analysis, aggregate rural and urban areas as well as age groups. In these models expenditures has a significant negative effect on female water collection time and no effect on male time, and the gender difference is significant (t=3.4): again, the gender gap in the burden of water collection falls with the level of household resources. Finally, we note that these conclusions are qualitatively robust to the addition to the models of the water source availability dummies.

Table 6.7 – Effects of log household expenditures on water collection time: female and male marginal effects and their differences

	Females		Males			
location/age group	Marginal effect	t-statistic	Marginal effect	t-statistic	Difference	t-statistic
			Madagascar			
rural:						
Adults (15+)	0.158	1.124	0.054	1.308	0.104	0.712
Children (7-14)	0.403	2.183	0.150	1.359	0.253	1.177
urban:						
Adults (15+)	-0.857	-6.673	-0.191	-2.515	-0.665	-4.458
Children (7-14)	-0.788	-3.965	-0.051	-0.356	-0.737	-2.831
			Uganda			
rural:						
Adults (15+)	-0.495	-4.257	0.161	2.328	-0.495	-4.848
Children (7-14)	-0.728	-2.313	-0.722	-2.550	-0.006	-0.014
urban:						
Adults (15+)	-0.386	-1.925	0.043	0.465	-0.429	-1.944
Children (7-14)	-1.617	-0.669	-0.838	-1.525	-0.779	-0.315

Note: Based on tobit model estimates. Standard errors calculated using the delta method.

For Madagascar we find for the urban sample that increases in household resources have negative and strongly significant impacts on women's and girls' hours of water collection. Effects on male time are much weaker, and we can easily reject equality of the male and female marginal effects. In contrast, in rural areas the marginal effects of household expenditures for both genders are positive but mostly not significant. Regressions on samples aggregating over age and rural-urban location show no significant effect of expenditures for either males or females. These findings are by and large consistent with the gender/quintile share patterns for all Madagascar presented in Section 4; note that those results, as with the pooled rural and urban sample regression, would be dominated by rural patterns given that the country is some 80% rural. The descriptive results actually showed a rising, then falling, gap in shares with only a small overall reduction in the gap. The multivariate specifications including a simple log expenditure term are not nuanced enough to reproduce this pattern, though polynomials in expenditures might be able to capture the pattern.

However, the overall conclusion from this exercise is that the descriptive benefit incidence and multivariate approaches do (in the present case) provide a similar impression: the gender gap in water collection time closes with increases in income in Uganda but does not, or does so only slightly, in Madagascar. Finally, we note that the for Madagascar, as for Uganda, our conclusions about relative male and female income effects do not change qualitatively when we add the water availability indicators, though the effect of expenditures on girls' water hours is no longer significant.

6.2.2.5 Impacts on Time in Other Activities

As we emphasized earlier in this report, the time benefits of public water supply investments cannot be assessed just by looking at changes in the hours allocated to water collection itself. Time saved in water collection may be reallocated to other home activities, to market activities, or to leisure. Therefore it is possible that there will be no reduction in the overall burden of work for women (or others) despite reductions in their water collection time. On the other hand, reallocations from water collection to certain other activities, in particular remunerative work, may confer other benefits to the individual. We are also interested in substitution impacts across individuals and genders, not just across activities for an individual. The implications of different outcomes for the evaluation of the benefits to females and males were discussed earlier.

Conceptually, the impacts of water infrastructure changes on time in other productive activities are indeterminate – no predictions as to the direction (and hence also, the magnitude) of the effects emerge from theory. This is due primarily to the fact that while improvements in water supply can free up time for other work activities, they also imply an increase in the real income of the household, which would tend to increase the demand for leisure and reduce the supply of labor to market work or home work, or both.

In our models of other time use outcomes (all domestic work, work for income in wage or self-employment, and total work in domestic and market activities) the set of explanatory variables is exactly the same as for the water time regressions. Each of the dependent variables exhibits censoring to a lesser or greater degree, so we use tobit here as well. We do not present the results from these regressions as they are very large in number once we disaggregate by outcome variable, gender, age, country, and rural/urban location. Rather, in keeping with our focus, we look to see whether, in cases where local access to a water source was found to have significant impacts on water collection time, these translated into significant changes in time in other activities. These cases, as seen above, are largely limited to taps in urban areas.

We return to table 6.5 for the comparative static calculations. Recall that in urban Uganda, for both genders and for adults as well as children, the presence of interior hookups in the cluster (as well as predicted use of this water source) had significant negative impacts on water collection time. Despite this, for women and girls in this sample, we see few significant non-water time impacts. The sign on market work hours is positive but the effect is only significant for girls. For the latter at least, the result suggests that the time savings in water collection from the household having access to (or using) interior taps are offset by increases in other work activities. For women in urban Madagascar, whose water collection time similarly fell with the introduction of interior taps, the sign on the coefficients suggest the opposite (a negative) effect on market work but the estimate was not statistically significant.

For boys and men in urban Uganda, unlike for women and girls in the same environment, the comparative static effect of interior tap availability (and predicted use) on market work is negative though only significant for boys, whose expected total work burden also is reduced.

Although these results suggest some indirect time impacts (though not very consistent across samples and genders), there is a problem with the results: the magnitudes of changes in time devoted to non-water activities are often too large to be consistent with the changes in water time itself. For example, use of a piped connection is estimated to reduce girls' water collection time in urban Uganda by about 5.4 hours while increasing the time allocated to market work by more than twice that amount. This would appear to be implausible under any reasonable set of hypotheses about income elasticities and elasticities of substitution among different tasks. The time use results for boys in Uganda for interior taps are similarly problematic in terms of relative magnitudes. It is likely that the water source availability indicators in these models are picking up the effects of unmeasured community factors that directly affect time in domestic and market-oriented activities. The addition of community level controls would potentially improve the reliability of the estimates, but such data were not collected for urban areas in our surveys. As they stand, the estimates suggest the need for caution when estimating these kinds of models. In particular, one can and should check for plausibility by examining the relative magnitudes of the estimated effects as done here.[94]

Although concerns about the reliability of the estimates may tempt one to discount these models of water supply impacts on household and market labor, in fact we can make a more basic point about these effects based on the water time regressions alone. Although

[94] Another check would be to see if the water infrastructure variables had significant impacts on time in other activities for cases where they had no effect on water time itself. This result would generally be implausible.

a number of significant impacts of either distance or source type were found for water collection time, the magnitude of these effects are relatively small: even where the impacts are largest – use of interior taps in urban areas – the savings in water collection time over alternative sources amounts to no more than a couple of hours per week, with the exception of girls (and to a somewhat lesser extent boys) in urban Uganda. This obviously does not allow much room for water infrastructure investments to lead to increases either in leisure time or time in other productive activities.

6.3 Conclusions

In Madagascar and Uganda, as in most developing countries, the burden of water collection time falls highly disproportionately upon women and girls. Also as in most developing countries, overall hours of work (home plus market) are higher for women than men. The question addressed in this analysis has been, will specific public investments in the water sector serve to reduce the burden on women of water collection and of work overall, both in absolute terms and relative to men?

The evidence presented above suggests that, in the countries studied, such investments can have at best only limited impacts on these goals. In rural areas of Madagascar and Uganda, the most feasible public infrastructure investment would be well construction, but our regressions found that the presence of wells (relative to the presence of natural sources alone) did not by and large affect water collection times. Local access to an exterior tap does reduce slightly girl's water collection time in rural Uganda, but such taps are rare in rural areas and extending the water network to all rural areas is clearly not a feasible policy except in the long term.

In urban areas, availability of interior taps consistently leads to time savings in water collection relative to communities where such connections are not available. Yet these savings generally do not amount to more than a few hours per week. In urban Uganda, the provision of exterior taps (which unlike in Madagascar remain absent from a large share of urban neighborhoods) would similarly provide modest time savings for women and girls in water collection.

When we estimate the effect not of local availability, but of household use of interior taps in urban areas (using availability to predict individual household use), the impact on water time can be significantly larger, especially for girls in Uganda. This specification essentially estimates the effect of a policy that induces households to use interior taps, for example through free hookups. Such a policy may have non-trivial benefits for girls—though our (admittedly problematic) estimates of indirect effects do not show a reduction in overall housework time or all work time.

In other cases, the fact that we generally see only small (or non-existent) reductions in the time burden of water collection from availability of wells (rural areas) or taps (urban areas) is probably due simply to the fact that in the environments studied, the sources households otherwise would use are already fairly close at hand. Even in rural areas the time in water collection of women and girls, while greater than in urban areas, is usually is no more than 3 to 4 hours per week. This is not a trivial burden, but it naturally puts limits on the time-related benefits to public water supply investments, both with respect to water collection time itself and other labor.

In closing, two points bear emphasis. The first is that these results must be regarded as country-specific. In Madagascar and Uganda, even in rural areas, people generally do not have to walk great distances to fetch water, whether from natural sources or wells. The situation may different elsewhere, especially in more arid climates (though even in an environment like Pakistan, hours in this activity are surprisingly modest). Second, it is important not to lose sight of the fact that investments in clean water supply potentially have very important health benefits for all household members, and indeed this has traditionally been the main rationale for such investments.

Appendix 6.1: Additional Descriptive Tables and Water Collection Time Regression Results

Table A6.1.1a - Weekly hours in domestic work (including water collection) by area, sex, and age: Madagascar and Uganda

	Women 15+	Men 15+	Girls[1]	Boys[1]
		Madagascar		
rural	19.87	3.66	8.79	3.42
urban	19.02	5.16	7.32	3.58
		Uganda		
rural	30.28	8.12	24.22	14.93
urban	31.73	6.62	26.13	15.65

Notes:

[1]Reflecting the different structure of the questionnaires, ages for girls and boys are 7-14 for Madagascar and 10-14 for Uganda.
Source: 1993/4 Madagascar EPM; 1992 Uganda HIS

Table A6.1.1b - Weekly hours in market work by area, sex, and age: Madagascar and Uganda

	Women 15+	Men 15+	Girls[1]	Boys[1]
		Madagascar		
rural	26.26	37.07	6.19	10.09
urban	22.87	32.17	2.81	3.34
		Uganda		
rural	24.58	32.42	8.01	10.84
urban	20.83	43.05	3.78	4.02

Notes:

[1]Reflecting the different structure of the questionnaires, ages for girls and boys are 7-14 for Madagascar and 10-14 for Uganda.

Source: 1993/4 Madagascar EPM; 1992 Uganda HIS

Table A6.1.1c - Weekly hours in all (domestic and market) work by area, sex, and age: Madagascar and Uganda

	Women 15+	Men 15+	Girls[1]	Boys[1]
		Madagascar		
rural	46.08	40.7	14.9	13.51
urban	41.64	37	10.13	6.92
		Uganda		
rural	54.87	40.54	32.23	25.77
urban	52.56	49.66	29.91	19.67

Notes:

[1]Reflecting the different structure of the questionnaires, ages for girls and boys are 7-14 for Madagascar and 10-14 for Uganda.

Source: 1993/4 Madagascar EPM; 1992 Uganda HIS

Table A6.1.2 - Rural Madagascar: Tobit estimates for weekly hours in water collection, regressions including source availability indicators

	women 15+		girls 7-14		men 15+		boys 7-14	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
Age	0.051	2.01	2.925	4.62	-0.263	-5.01	2.994	3.55
(Age)2	-0.002	-5.17	-0.111	-3.72	0.002	3.66	-0.127	-3.18
Head sex	-0.298	-1.02	0.436	0.91	-0.563	-0.69	-0.026	-0.04
head schooling	-0.231	-6.78	-0.018	-0.34	0.063	1.00	0.051	0.70
# children <5	-0.195	-1.80	0.197	1.21	-0.227	-1.05	0.483	2.16
# girls 5-14	-0.474	-4.37	-0.262	-1.54	-1.336	-5.78	-0.877	-3.36
# boys 5-14	-0.034	-0.34	-0.125	-0.66	-0.535	-2.91	-0.056	-0.27
# females 15+	-0.446	-4.11	-0.457	-2.62	-1.948	-4.35	-0.674	-2.18
# males 15+	-0.024	-0.24	-0.249	-1.57	0.591	3.10	-0.037	-0.16
march	-0.321	-0.58	-0.932	-1.44	-1.793	-1.50	0.882	0.83
april	0.911	1.59	1.551	2.34	-1.894	-1.46	0.985	1.02
may	0.521	0.96	-0.439	-0.52	-0.552	-0.39	1.481	1.16
june	0.109	0.21	0.892	1.37	-0.994	-0.86	1.428	1.00
july	-0.444	-0.74	0.748	1.30	-1.966	-1.74	-0.545	-0.48
september	0.862	1.29	0.778	1.19	-1.261	-1.08	-0.185	-0.18
october	0.970	1.83	0.601	0.76	-2.260	-1.61	0.118	0.10
november	0.720	1.27	-1.036	-1.50	-1.598	-1.18	-0.245	-0.25
december	1.441	1.83	0.962	1.36	-1.433	-0.99	0.642	0.49
Fian province	3.523	7.33	1.264	2.25	-0.082	-0.13	-0.545	-0.62
Toam province	3.298	9.02	1.268	2.32	0.481	0.72	-0.647	-0.77
Maha province	3.634	9.13	0.769	1.45	-1.279	-1.33	-3.614	-3.33
Toli province	2.504	4.67	-0.134	-0.20	1.984	1.81	-1.832	-2.06
Antsir province	2.495	4.54	0.502	0.84	1.390	2.19	-1.117	-1.12
ln expenditures per capita	1.68E-08	0.40	3.71E-07	1.06	2.60E-08	0.19	-1.19E-07	-0.27
value of agric. land	-1.10E-07	-3.22	1.34E-08	0.3	-1.35E-07	-1.51	5.00E-08	0.49
well in community	0.227	0.78	0.273	0.73	0.902	2.05	0.555	0.93
ext. tap in community	-0.364	-0.95	0.911	1.86	1.408	1.54	0.932	1.32
local paved road	0.436	1.10	-0.516	-1.02	0.155	0.25	0.575	0.78
community has electric.	-0.559	-1.13	-1.427	-1.65	1.134	1.17	1.037	1.06
distance to market (km)	-0.020	-2.38	-0.023	-2.02	0.021	1.11	-0.019	-1.38
Intercept	3.496	4.73	-15.788	-4.81	3.558	1.95	-17.884	-4.13
sigma	4.379	4.081	4.358	3.943	5.913	4.996	5.320	4.462
No. of observations		3486		1327		3442		1394

Standard errors adjusted for clustering at the community level

Table A6.1.3 - Rural Uganda: Tobit estimates for weekly hours in water collection, regressions including source availability indicators

	women 15+		girls 10-14		men 15+		boys 10-14	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
age	-0.140	-17.49	0.087	0.64	-0.223	-14.62	-0.035	-0.24
head some primary	-0.238	-0.98	0.061	0.12	-2.372	-5.45	1.360	2.31
head compl. primary	-0.589	-2.01	-0.370	-0.55	-3.630	-5.42	1.312	1.71
head compl. secondary	-1.793	-1.79	0.506	0.35	-3.363	-2.33	2.269	1.50
# children <6	-0.080	-1.01	0.048	0.27	-0.325	-1.58	0.244	1.21
# girls 5-16	-0.411	-3.88	0.201	1.01	0.157	0.69	0.055	0.24
# boys 5-16	-0.364	-3.83	-0.393	-2.04	0.461	2.57	0.175	0.85
# females 17+	-0.297	-2.41	0.094	0.42	-0.701	-2.37	-0.038	-0.14
# males 17+	-0.050	-0.44	-0.171	-0.74	0.521	2.27	-0.192	-0.89
february	-0.479	-1.04	-1.378	-1.69	1.360	1.79	-0.286	-0.31
march	-1.687	-2.02	-4.492	-3.21	-1.874	-1.23	-3.769	-1.91
april	-0.498	-0.71	-3.423	-2.48	1.299	1.34	-2.901	-2.04
may	-1.040	-1.86	-3.325	-3.17	0.566	0.61	-2.150	-2.03
june	-1.633	-2.31	-3.205	-3.65	0.975	0.85	-2.521	-1.87
july	-1.334	-1.81	-0.942	-0.81	-0.010	-0.01	1.135	0.88
august	-0.870	-1.19	-4.096	-3.48	0.344	0.29	-3.464	-2.18
september	-0.589	-1.20	-1.919	-2.10	1.015	1.08	1.046	0.99
october	0.627	0.80	-4.016	-2.43	2.586	1.26	-0.182	-0.11
november	0.395	0.66	-0.928	-0.92	-0.426	-0.37	-2.125	-1.79
december	-0.020	-0.05	-1.982	-2.28	-0.945	-1.01	0.176	0.14
East region	2.775	7.08	-2.446	-3.28	-4.297	-6.77	-5.088	-5.63
West region	-2.865	-6.64	-5.219	-6.83	-4.335	-6.54	-4.969	-6.15
North region	3.810	8.40	-0.852	-1.20	-8.612	-10.47	-8.122	-8.85
ln expenditures per capita	-0.860	-4.15	-1.040	-2.28	1.022	2.44	-1.232	-2.50
well in community	-0.210	-0.46	1.674	2.61	0.137	0.18	1.101	1.25
ext. tap in community	-0.137	-0.21	-1.614	-1.65	-0.959	-0.99	-1.634	-1.66
household has elect.	-1.867	-2.12	-3.130	-1.95	-1.690	-1.12	0.061	0.05
Intercept	17.292	6.75	17.814	3.20	-8.106	-1.58	19.194	3.11
/sigma	6.352	6.073	7.283	6.858	9.762	9.165	7.868	7.310
No. of observations		7885		1864		7228		2032

Standard errors adjusted for clustering at the community level

Table A6.1.4 - Urban Madagascar: Tobit estimates for weekly hours in water collection, regressions including source availability indicators

	women 15+		girls 7-14		men 15+		boys 7-14	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
Age	-0.116	-2.89	0.627	5.60	-0.411	-5.85	0.687	6.26
(Age)2	0.000	0.11			0.003	3.82		
Head sex	-0.663	-2.04	-0.502	-0.95	0.494	0.88	-0.601	-0.94
head schooling	-0.262	-6.35	-0.258	-4.28	-0.118	-2.35	-0.201	-3.51
# children <5	0.152	0.88	-0.064	-0.25	-0.491	-2.10	0.549	1.75
# girls 5-14	-0.628	-3.84	-0.371	-1.57	-0.552	-2.72	-0.432	-1.69
# boys 5-14	-0.209	-1.40	-0.649	-2.61	-0.446	-1.98	-0.110	-0.38
# females 15+	-0.771	-4.71	-0.684	-3.32	-1.089	-4.80	-0.581	-2.40
# males 15+	-0.436	-2.92	-0.257	-1.56	-0.239	-1.32	-0.362	-1.83
march	0.252	0.28	1.104	1.33	-1.096	-1.22	0.110	0.08
april	-0.427	-0.48	1.348	1.35	-0.896	-0.80	1.117	0.80
may	0.879	1.01	1.942	2.58	1.129	1.21	1.904	1.50
june	-0.074	-0.08	0.541	0.64	-0.031	-0.04	2.138	1.54
july	-0.132	-0.15	2.326	2.12	0.766	0.79	1.843	1.34
september	0.470	0.56	1.792	1.28	1.171	1.02	2.794	1.60
october	1.657	1.77	3.091	3.09	1.572	1.13	0.575	0.42
november	0.918	1.01	1.854	1.66	-0.301	-0.33	2.168	2.02
december	0.496	0.56	1.804	2.36	1.786	1.87	2.814	2.56
Fian province	2.306	3.93	1.356	1.81	0.052	0.07	-0.172	-0.16
Toam province	-0.230	-0.38	-0.632	-0.86	-0.149	-0.22	-0.168	-0.17
Maha province	1.193	2.25	0.137	0.16	0.347	0.57	-1.448	-1.68
Toli province	0.386	0.64	-0.716	-0.66	-0.894	-1.07	-1.118	-1.20
Ants province	1.456	2.30	-1.250	-1.51	-0.872	-0.96	-1.488	-1.53
ln expenditures per capita	-2.22E-06	-2.57	-4.22E-06	-3.26	-1.11E-06	-1.43	-2.78E-07	-0.63
int. taps in community	-1.623	-3.68	-0.826	-1.35	0.136	0.24	-0.343	-0.49
no tap source in community	-0.931	-1.63	-1.141	-1.46	-2.352	-3.12	-2.737	-2.40
Intercept	10.291	7.28	-0.255	-0.16	9.313	4.75	-4.915	-2.65
sigma	5.283	4.660	4.774	4.022	5.649	4.933	4.960	4.384
No. of observations	2951		976		2650		967	

Standard errors adjusted for clustering at the community level

Excluded water availability category: external taps only present

Table A6.1.5 - Urban Uganda: Tobit estimates for weekly hours in water collection, regressions including source availability indicators

Variable	women 15+		girls 10-14		men 15+		boys 10-14	
	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
age	-0.164	-5.68	0.849	1.93	-0.295	-7.77	0.459	0.85
head some primary	-1.324	-1.74	-0.228	-0.18	-1.207	-1.10	-1.651	-0.84
head compl. primary	-1.320	-1.57	-0.609	-0.38	-3.702	-3.33	0.481	0.23
head compl. secndry	-3.460	-3.25	0.739	0.45	-4.363	-2.96	-1.602	-0.52
# children <6	0.220	0.86	-0.232	-0.43	-0.808	-1.91	0.713	1.55
# girls 5-16	-0.193	-0.78	-0.205	-0.53	-0.357	-0.96	-0.866	-1.66
# boys 5-16	-0.689	-2.52	-0.669	-1.02	1.551	3.59	0.403	0.87
# females 17+	-0.236	-0.64	-0.744	-1.09	-1.447	-2.41	-1.758	-2.97
# males 17+	0.464	1.72	-0.172	-0.32	0.342	0.86	-0.236	-0.46
february	-0.362	-0.28	-5.063	-2.04	-1.592	-0.99	1.115	0.49
march	2.488	1.07	-2.601	-0.96	-12.536	-3.39	-1.583	-0.39
april	5.419	3.58	-0.278	-0.06	0.540	0.26	6.949	2.84
may	4.074	4.01	-1.206	-0.56	0.490	0.31	3.063	1.16
june	1.616	1.65	-0.351	-0.18	-0.180	-0.11	0.087	0.04
july	0.959	0.51	-2.567	-0.92	-1.411	-0.76	-0.293	-0.09
august	-2.137	-1.67	0.487	0.25	0.514	0.31	-1.556	-0.77
september	3.336	1.96	1.116	0.48	1.672	0.78	1.596	0.75
october	1.572	0.53	-0.781	-0.18	-6.598	-2.02	-6.683	-1.11
november	2.518	1.91	1.210	0.61	-4.454	-1.97	-0.284	-0.13
december	-0.148	-0.12	-1.227	-0.63	-1.909	-0.88	-0.797	-0.35
East region	1.003	1.05	-2.647	-1.75	-0.099	-0.10	-4.555	-2.36
West region	-2.765	-2.94	-3.466	-2.05	-1.386	-1.28	-5.332	-2.77
North region	4.351	4.08	-2.264	-1.23	-0.963	-0.64	-6.639	-3.33
ln expenditures per capita	-0.788	-1.34	-1.747	.	0.840	1.03	-1.489	-1.15
household has elect.	-2.831	-3.77	-1.024	-0.71	-2.121	-2.15	-3.935	-3.05
Int. tap in community	-2.165	-2.10	-8.350	-5.21	-5.008	-4.16	-4.130	-1.64
No tap source in community	2.326	2.32	1.685	1.17	1.557	1.30	1.487	0.76
Intercept	11.641	1.75	19.003	1.54	-6.028	-0.56	19.476	1.05
sigma	7.733	7.020	7.782	6.562	9.018	7.463	8.356	7.154
No. of observations		4170		1057		3735		875

Standard errors adjusted for clustering at the community level

Excluded water availability category: external taps only present

Table A6.1.6 - Urban Madagascar: Two-stage tobit estimates for weekly hours in water collection--

	women 15+		girls 7-14		men 15+		boys 7-14	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
Age	-0.114	-2.85	0.630	5.61	-0.410	-5.83	0.692	6.31
(Age)2	0.000	0.30			0.003	3.79		
Head sex	-1.049	-3.08	-0.642	-1.17	0.444	0.78	-0.720	-1.08
head schooling	-0.088	-1.47	-0.214	-2.86	-0.133	-1.52	-0.167	-1.53
# children <5	0.050	0.29	-0.079	-0.31	-0.497	-2.13	0.519	1.58
# girls 5-14	-0.568	-3.35	-0.396	-1.69	-0.555	-2.74	-0.413	-1.58
# boys 5-14	-0.404	-2.53	-0.631	-2.53	-0.438	-2.03	-0.178	-0.54
# females 15+	-0.490	-2.79	-0.537	-2.18	-1.092	-4.56	-0.520	-2.09
# males 15+	-0.391	-2.59	-0.240	-1.45	-0.250	-1.40	-0.388	-1.91
march	0.290	0.32	0.972	1.18	-0.923	-1.07	0.297	0.23
april	-0.625	-0.70	0.924	0.93	-0.731	-0.66	1.102	0.81
may	0.545	0.61	1.303	1.61	1.075	1.15	1.760	1.32
june	-0.641	-0.69	0.101	0.11	-0.067	-0.08	2.067	1.49
july	0.065	0.07	1.938	1.77	0.671	0.70	1.833	1.36
september	-0.136	-0.16	1.224	0.84	1.173	1.00	2.705	1.51
october	1.239	1.30	2.765	2.85	1.505	1.05	0.560	0.41
november	0.554	0.61	1.668	1.50	-0.079	-0.09	2.283	2.10
december	0.670	0.77	1.655	2.17	1.722	1.83	2.790	2.48
Fian province	2.161	3.61	1.419	1.91	0.081	0.11	-0.202	-0.20
Toam province	-0.194	-0.33	-0.470	-0.65	-0.063	-0.09	-0.252	-0.25
Maha province	1.103	2.08	0.279	0.32	0.274	0.46	-1.394	-1.60
Toli province	0.296	0.50	-0.616	-0.58	-0.893	-1.11	-1.164	-1.28
Ants province	1.782	2.66	-0.884	-0.96	-0.537	-0.60	-1.088	-0.99
ln expenditures per capita	-1.14E-06	-1.23	-2.81E-06	-1.83	-1.12E-06	-1.28	-2.34E-07	-0.48
household uses interior tap**a**	-7.960	-3.78	-3.788	-1.35	0.350	0.14	-1.418	-0.36
ext.tap available in community	0.634	1.28	0.974	1.49	2.850	4.67	2.527	2.66
Intercept	8.428	6.11	-1.849	-1.05	6.744	3.44	-7.532	-4.08
sigma	5.277	4.653	4.770	4.019	5.635	4.918	4.957	4.382
No. of observations	2951		976		2650		967	

Standard errors adjusted for clustering at the community level

[a]Household use of interior water connection is instrumented using cluster interior tap availability indicator

Table A6.1.7 - Urban Uganda: Two-stage tobit estimates for weekly hours in water collection--effect of

	women 15+		girls 10-14		men 15+		boys 10-14	
Variable	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic	Coefficient	t-statistic
age	-0.163	-5.64	0.787	1.77	-0.292	-7.71	0.279	0.51
head some primary	-1.282	-1.66	-0.509	-0.40	-1.246	-1.13	-2.011	-1.04
head compl. primary	-1.416	-1.69	-2.081	-1.31	-3.978	-3.59	0.067	0.03
head compl. secndry	-3.153	-2.83	0.285	0.17	-3.509	-2.33	-1.452	-0.47
# children <6	0.164	0.64	-0.314	-0.59	-0.949	-2.23	0.231	0.43
# girls 5-16	-0.050	-0.20	0.387	1.02	-0.005	-0.01	-0.578	-1.14
# boys 5-16	-0.488	-1.79	-0.143	-0.22	1.847	4.07	0.699	1.53
# females 17+	-0.138	-0.36	-0.570	-0.84	-1.141	-1.90	-1.419	-2.36
# males 17+	0.591	2.07	0.349	0.62	0.615	1.46	0.047	0.08
february	0.016	0.01	-1.892	-0.75	-0.286	-0.17	0.141	0.06
march	1.784	0.76	-3.440	-1.31	-14.186	-3.83	-3.171	-0.82
april	5.674	3.83	2.427	0.52	0.915	0.45	7.719	3.02
may	3.797	3.72	0.022	0.01	-0.355	-0.22	2.492	0.98
june	1.391	1.39	1.656	0.87	-0.749	-0.46	-0.260	-0.13
july	0.720	0.38	-1.535	-0.56	-1.783	-0.96	-0.760	-0.23
august	-2.613	-1.95	1.931	1.07	-0.418	-0.26	-2.903	-1.37
september	3.294	1.91	4.372	1.92	1.611	0.75	1.112	0.53
october	1.226	0.43	-1.828	-0.43	-6.868	-2.11	-6.970	-1.14
november	2.506	1.91	3.194	1.71	-4.720	-2.08	-1.165	-0.53
december	-0.733	-0.60	-0.621	-0.34	-2.451	-1.13	-1.432	-0.61
East region	1.477	1.50	-0.310	-0.20	1.186	1.12	-3.450	-1.96
West region	-2.519	-2.60	-3.963	-2.38	-0.782	-0.72	-4.577	-2.49
North region	4.736	4.30	-2.275	-1.26	-0.563	-0.38	-6.114	-3.19
ln expenditures per capita	-0.192	-0.29	-0.330	.	1.636	1.99	-1.154	-0.86
ext.tap available in comm	-1.920	-2.04	-2.179	-1.60	-1.331	-1.20	-1.693	-1.01
household uses interior ta	-7.844	-2.85	-19.371	-5.60	-15.244	-4.67	-11.607	-1.88
household has elect.	-1.867	-2.13	1.020	0.65	-0.307	-0.28	-1.755	-1.01
Intercept	5.729	0.63	-0.098	-0.01	-16.157	-1.51	17.606	0.93
sigma	7.747	7.036	7.754	6.546	9.017	7.465	8.350	7.145
No. of observations	4170		1057		3735		875	

Standard errors adjusted for clustering at the community level

[a]Household use of interior water connection is instrumented using cluster interior tap availability indicator

7 Conclusion

Two questions were posed at the outset of this study: first, how does the incidence of public expenditures vary by gender and income? Second, how can existing allocations of public expenditure be changed to improve gender equity? This report has addressed these questions through a detailed review and interpretation of existing evidence and through primary analysis on a large sample of developing country data sets. The answer to both questions, emerging from the literature review and our own findings, is that “it depends” – on the country context, and on the service being considered.

Our analysis of the question of public expenditure equity focused on the intersection of the two dimensions of gender and income, rather than on just income (the focus of standard benefit incidence analysis) on the one hand, or gender on the other. The existing empirical literature that considers these dimensions together is sparse and typically not based on rigorous statistical comparisons. There is some evidence in this literature that in countries where there is a large overall gender gap in the use of education services, the gap is narrower among the well-off than among the poor. However, the evidence for this is certainly not conclusive. Our own analysis of benefit incidence and gender on a sample of eight developing and one transition country finds few statistically significant correlations between the size of gender gaps in public health and education services, on the one hand, and the level of welfare (measured by per capita household expenditures), on the other.

This is not to say that large (and statistically significant) *overall* gender gaps do not exist for certain services or activities affected by public expenditures. They certainly do. The existing literature makes plain that gender gaps are common (though by no means universal) in education in developing countries. For health care utilization, gender gaps seem to exist in a much smaller number of countries. Among the nine countries examined in this study male advantage is concentrated in just a few countries and usually limited to post-primary schooling; the identities of the countries where most of these gaps are found (Ghana, Uganda, and Pakistan) do not come entirely as a surprise. For public curative health services and vaccinations, there is essentially no evidence in our nine country sample of females suffering an average disadvantage in access. Therefore, even when considering simple mean differences by gender in the use of services (rather than differences by income or welfare level), it is important not to assume that gaps exist for a given country or type of public spending.

On the other hand, the time burden of water collection (and hence the potential benefits of public water infrastructure investments) falls heavily disproportionately on women and girls throughout the developing world. Data from the two countries in our sample with information on time in this activity are consistent with this hypothesis, as are accounts from many other countries. Finally, if we are willing to think of public sector employment as a public expenditure ‘benefit’, this is a case where benefits indeed accrue highly disproportionately to men. For almost all countries in our sample, rates of public sector employment are substantially higher for men than women.

The possibilities for redirecting public spending to close gender gaps (where they exist) in the benefits from public expenditure depend largely on the existence of differential demand responses by gender to specific policies. Existing empirical research on this question largely concerns education and provides some evidence that girl's schooling is more sensitive than boys' to the distance to schools, to monetary costs, and to the quality of the service. Changes in the level of household resources also often have larger effects on girls' schooling and (though the evidence is comparatively thin) on their health care or nutrition. However, examples also exist of no gender differences or of greater male demand response to the above factors, most notably household resources.

Our own analysis of education and health care demand in Uganda and Madagascar found relatively few statistically significant gender differences in response to distance, cost, and service quality, or to changes in household resources. Where differences were found, they were as likely to show a stronger male demand response as a stronger female response. These findings underscore the need to conduct careful country-specific analysis, and to use appropriate statistical methods to make gender comparisons when doing so.

Our multivariate analysis for these two countries also examined the water sector. Although in both Madagascar and Uganda women and girls disproportionately bear the burden of water collection, the results suggest that feasible public investments – in particular, providing wells in rural areas – will usually not lead to large reductions in water collection times (which are not extremely high to start with) or change the relative burdens of overall work done by women and men. Time savings may be larger in other countries, especially in more arid climates, and of course investments in clean water supply potentially have important health benefits for all household members. Nonetheless, our findings caution against assuming that investments in water infrastructure will have dramatic effects on female time use and on the division of the overall burden of work between genders.

Our conclusions from our own findings and our review of previous research do not mean that where gender gaps are found, policy cannot remedy them. On the contrary, there is evidence from a number of such contexts, especially with regard to education, that specific public investments can effectively target girls. School construction programs that build more facilities in rural areas will likely strongly favor girls' enrollments in these environments. Policies that explicitly target girls' enrollments through subsidies to girls' schooling or the construction of separate girls' schools can be highly successful in reducing gender enrollment gaps. However, it is clear that the evidence for both the existence of gender gaps and for differences in the responsiveness of males vs. females to various policy levers is quite varied, convincing us that broad generalizations are inappropriate. Country- and service-specific analyses should always be undertaken before drawing conclusions on the necessity and efficacy of gender-focused policies.

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