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Evaluation of Jamaica's PATH Program: Final Report

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EXECUTIVE SUMMARY

This report summarizes the findings of an evaluation of a social safety net initiative, the Programme of Advancement Through Health and Education (PATH), which was undertaken by the Government of Jamaica, beginning in 2001. The main objectives of the initiative, which is operated by the Ministry of Labour and Social Security (MLSS), are to achieve better targeting of welfare benefits to the poor and to increase human capital by conditioning receipt of the benefits on participants meeting certain requirements for school attendance and health care visits.

Under the programme, PATH eligibility is determined using a scoring formula which draws on detailed information about household circumstances. Benefits to eligible households are disbursed through the postal system. The conditioning requirements to increase human capital are enforced, based on information which is obtained through schools and health care centres.

OVERVIEW OF THE EVALUATION

The PATH programme has been evaluated by an independent research contractor, Mathematica Policy Research, and Mathematica's subcontractors, Hope Enterprises, Ltd, and Impaq International. In order to evaluate PATH's impacts, a comparison-group research design was developed. The design involves comparing outcomes for a sample of 2,500 households eligible for PATH (participant group) with another group of households which were similar to the participant group but were not deemed eligible for PATH (comparison group).

Data for the evaluation were obtained in several ways:

- Qualitative interviews were conducted with PATH programme staff, as well as the staff of schools and health care facilities serving PATH clients. Interviews were also conducted with post office staff.
- Focus group discussions were held with PATH clients about their experiences with the programme.
- Data were extracted from the management information system (MIS) used to operate the programme.
- Household surveys were conducted at several points of time, including a baseline survey of participant and comparison group households and a follow-up survey of the same households after the participant group had received several disbursements of benefits.

The evaluation included:

- A *qualitative* analysis of programme implementation

- A *tabular* analysis of benefit targeting, based on the management information system data
- A *multivariate* analysis of the survey data to assess programme impacts, which was also supplemented by other analysis techniques.

Key analysis findings are discussed below.

IMPLEMENTATION

It is clear that the Ministry of Labour and Social Security (MLSS) has been basically successful in setting up operations which implement the PATH concept. Key accomplishments include:

- Developing a functioning scoring formula for determining eligibility for the programme.
- Setting up procedures for publicizing the availability of PATH and enrolling clients into the programme, largely through the functions of the MLSS parish offices
- Setting up a process for disbursing benefits through the postal system
- Setting up an MIS for compiling key information about client accounts and for supporting the benefit disbursement process
- Setting up information procedures for obtaining information from schools and health centres about compliance with PATH's requirements, and feeding that information into the system for determining payments

Together, these accomplishments have created a programme which is widely viewed by stakeholders as having accomplished its basic mission successfully. Furthermore, survey results indicate that most PATH participants report having had satisfactory transactions with the programme. Furthermore, the results from the impact analysis also suggest that PATH is having a positive impact on attendance in schools and health centres. All of these findings point to a programme which has been successfully implemented.

However, the findings from the qualitative analysis also suggest a number of areas where additional improvements may be possible. We note several important examples of this here:

- There are often delays in making PATH payments available to clients. The resulting uncertainty about the timing of benefit availability imposes substantial costs and sometimes hardships on clients.
- Many stakeholders, including clients and staff at schools and health clinics, believe that there are problems in the accuracy of the information used for enforcing the sanctioning requirements related to school and health care requirements. There

appear to be particular issues in getting medical excuses for school absences reflected in the system and in dealing with situations where clients use a health care facility other than the one they most frequently used.

- Clients report having difficulties and delays in obtaining information from MLSS representatives about their cases and in making changes in their cases.

These and similar issues warrant review by Jamaica officials, in order to ensure that the PATH concept is being implemented as completely as possible.

BENEFIT TARGETING

The evidence from the PATH management information system, as reviewed in Chapter IV indicates that PATH has been successful at targeting its benefits to the Island's poorest households. In particular, 58 percent of benefits were found to go to the poorest quintile of the population. Comparisons made in Chapter IV with similar data from other programs in Jamaica and in Chapter VII with other countries suggest that this figure compares favorably with results that have been attained in similar programs elsewhere.

As with the implementation area, however, there may be room for at least some additional improvements in the targeting area. During interviews and focus groups, not only clients but also school and health centre staff indicated that they were aware of households receiving PATH benefits who were probably not deserving of them, while they also knew of certain other households who had been turned down by PATH, even though their need for assistance seemed very high. During these discussions, it was suggested that greater in-person monitoring by MLSS staff—including more visits to clients' homes to verify information the households provided—could be useful in improving the targeting.

IMPACTS ON SCHOOL ATTENDANCE

PATH has been effective at accomplishing its objective of encouraging households to send their children to school with greater regularity. Multiple regression analysis results reported in Chapter V indicate that PATH has increased school attendance by approximately 0.5 days per month. The estimated increase is about three percent over the baseline level, and it is statistically significant.

The size of this estimated effect should be viewed in the context of the fact that school attendance was already quite high in Jamaica. In particular, the starting attendance rates were on the order of 85 percent. Also, as in other countries, illnesses and other legitimate reasons for school attendance are not uncommon. Together, these factors have constrained the possible size of the estimated effect of attendance—there was only limited room for improvement.

Comparisons with other countries are somewhat complicated by the fact that most other conditional cash transfer (CCT) programs have tended to focus on increasing *enrollment* rather than *attendance*, which is the focus of PATH. However, in general, countries with baseline

conditions similar to those in Jamaica have experienced education-related effects similar to those found with PATH (Chapter VII).

The results of discussions with parents and with school staff were consistent with the statistical findings. Parents had a reasonably clear understanding of the programme and the conditionality of the benefits. School staff indicated that, in their experience, the parents were highly aware of the requirements of the PATH programme and were making good-faith efforts to increase their children's attendance.

In response to survey questions about reasons for their children sometimes being absent from school, parents provided answers that suggested the PATH had relieved some of the financial pressures associated with attendance, such as providing lunch money and payments for transportation.

IMPACTS ON HEALTH OUTCOMES

PATH was also successful in meeting its objective of increasing the use of preventive health care for children in PATH families. The results of the statistical analysis suggest that health care visits for children 0-6 increased by approximately 38 percent as a result of PATH. As with educational outcomes, the magnitude of this effect appears to be broadly consistent with the corresponding effects in other countries which have enacted CCTs.

Regression analysis of the impact of PATH on health care visits by the elderly show no evidence of effects on this group. One potential explanation of this is that early in the programme a decision was made not to enforce the conditionality requirements for this group of PATH recipients.

IMPACTS ON OTHER OUTCOMES

While PATH was successful at increasing school attendance and preventive health visits, there is no evidence that it was able to affect longer term outcomes such as grades, advancement to next grade or health care status. While this finding is subject to several methodological caveats described in Chapter VIII, it suggests that to improve the human capital of poor households through PATH, policymakers may want to pay particular attention to the interaction between PATH and factors related to the delivery of education and health services in Jamaica.

I. INTRODUCTION

In response to concerns about poverty in Jamaica, the Government of Jamaica (GOJ) developed the Programme of Advancement Through Health and Education (PATH) as a means to (1) restructure several important social safety net programmes; (2) improve the targeting of programme services; and (3) link individuals to the assistance most appropriate for them. To assess the expected impacts of these programme reforms, GOJ has conducted a rigorous, comprehensive evaluation of PATH. The evaluation is structured around a quasi-experimental design that used detailed household survey data to determine whether PATH is reaching its intended population and increasing the human capital of poor households as measured by school attendance and health care usage. The evaluation also includes qualitative analysis based on focus groups and executive interviews to assess how the programme has been implemented.

Based on a competitive procurement, Mathematica Policy Research (MPR), together with its subcontractors Hope Enterprises, Ltd., and Impaq International, was selected by GOJ to conduct the evaluation. This final report describes the evaluation results. In particular, we focus on the evaluation findings with regard to the impacts of PATH on two key variables, school attendance and health care usage, which have not been examined in earlier reports. In addition, for the convenience of policymakers, we present important results from other parts of the study, including targeting and implementation, thus consolidating all of the study's findings into a single "stand-alone" document.

Section A of this introductory chapter describes the background for the safety net reform, including Jamaica's overall social and economic context, the nature of poverty on the Island, and Jamaica's social safety net. Section B describes GOJ's efforts to reform the safety net, including the salient features of PATH.

The rest of the report is organized as follows: Chapter II identifies the key objectives of the evaluation and provides overviews of the methodologies used to address them. Subsequent chapters present our findings with regard to the process used to implement PATH (Chapter III); how well benefits are targeted to the poor (Chapter IV); and the effects of PATH on education attendance and health care usage (Chapters V and VI). Chapter VII presents comparisons of targeting effectiveness and impacts between PATH and similar programmes implemented in other countries. Finally, Chapter VIII summarizes overall conclusions.

A. THE IMPETUS FOR REFORM

1. Economic and Social Context

During most of the 1990s, the Jamaican economy experienced low or negative growth. In 1996, a crisis in the financial sector exacerbated matters when the solvency of several insurance companies and other domestic financial institutions emerged as an issue. While Jamaica has made remarkable progress in some social sectors,[1] the upward trend in some social indicators is likely to be temporary unless the economy improves.

In addition, focus group research conducted in Jamaica by the World Bank in 1999 found "significant decreases in the well being of the poorest individuals over the last ten years" and, in rural areas, "an emerging category of severely poor households, often including the elderly" (The World Bank 2001b, p. 6).

In light of this situation, the challenge for GOJ was to prevent a reversal in the significant declines in poverty witnessed in the 1990s and to ensure that the poor were adequately protected.

[1]For example, the poverty rate in Jamaica decreased from 28 percent in 1990 to 17 percent in 1999 (The World Bank 2000). The forces that worked to lower poverty during a period of sustained recession remain poorly understood and in need of rigorous investigation. Some of the explanations for this paradox are the effects of migration, remittances from abroad, and informal sector opportunities.

2. Poverty in Jamaica

A major force behind the development of PATH is the nature of poverty in Jamaica and its relationship to education and health care. For instance, poverty is concentrated:

- ***Among the young and the old,*** with almost half of the poor younger than 18 years of age and another 10 percent over age 65 (Blank 2001)
- ***In rural areas*****,** with nearly 80 percent of the poor living in rural areas and less than 10 percent living in the Kingston Metropolitan Area (The World Bank 1999)
- ***Among female-headed households,*** with 66 percent of poor households headed by women, although women head only 44 percent of all households (Blank 2001)
- ***Among larger families,*** with 40 percent of poor families consisting of six or more members (The World Bank 2001c)

In addition, Jamaica's comparatively favorable social indicators mask a significant lack of access to education, especially among the poor. Although poor children are typically enrolled in school, they sometimes do not attend regularly. Poor families tend to attribute erratic attendance to "money problems." Indeed, the World Bank found that a lack of money has prevented parents from sending their children to school and providing them with food, clothing, and shelter. For instance:

> *"Education was widely associated with high well being and so it seemed reasonable to infer that schools are regarded as important because of the personal benefits that are seen to accrue from investing in education. In this vein, the costs of buying into education service were seen as a major impediment to social advancement by the poorer groups"* (The World Bank 2001b, p. 43).

Poverty is related not only to education but also to the quality of and access to health care. For instance, immunization rates for infants up to 11 months of age fell from 93 percent in 1993 to 85 percent in 1999 (The World Bank 2001c). While children are often immunized by the time they start primary school, they are not necessarily immunized early enough. Inadequate prenatal care has also been a serious problem. In addition, poverty poses a barrier to health care,

especially in rural areas. For instance, the poor perceive public health centres in rural areas, though not easily accessible to many communities, as highly important because private clinics charge far higher consultation fees (The World Bank 2001b). Both preventive and ameliorative programmes are necessary for improving the health of youth, pregnant and lactating women, the elderly, and the disabled. For youth, preventive programmes, which ought to begin during the early childhood development stage, should lay the foundation for better developmental outcomes and lead to high returns later in life. For adults, regular checkups should improve individuals' health and chronic illness monitoring and reduce emergency visits.

3. The Social Safety Net

Before the development of the PATH, GOJ financed 45 safety net programmes through 12 ministries, including three income support programmes—the Food Stamps Programme, Poor Relief Programme, and Public Assistance Programme—four school-based programmes, five labor market programmes, two subsidized drug programmes, and an indigent housing programme, among others. However, the effectiveness of these programmes was perceived to be low. For instance, the World Bank recently found that the majority of these programmes does not adequately serve the poor (The World Bank 2001c). And, even though some of the programmes target the poor, they fail to reach a significant share of the affected population. In addition, other programmes are not designed to target the poor at all (as is the case for most of the labor market programmes). For example, approximately 263,000 persons registered to receive food stamps in 1998. However, less than 30 percent of the households receiving food stamps fell into the poorest quintile (Blank 2001). In addition, a high proportion of the poor appears to be unaware of the range of available benefits or otherwise unable to afford the direct and indirect costs of obtaining such benefits (e.g., transportation costs). Finally, government

programmes' low benefit levels offer less-than-adequate support, which, given the cost of obtaining benefits, probably deters eligible individuals from even applying for benefits.

B. PATH OVERVIEW

1. Planning

To address the gaps in its safety net, GOJ formed an interinstitutional task force led by the Planning Institute of Jamaica (PIOJ) to develop a "policy matrix" for programme reform. The reform effort was intended to knit the safety net into a fiscally sound and more efficient system of social assistance for the poor and vulnerable and, in particular, for the extremely poor. To this end, the reform effort focused on four major areas:

1. Developing and implementing a universal targeting system based on a proxy means test. Ultimately, the goal was that several key safety net programmes would use the system to increase transparency in the selection of beneficiaries, reduce the administrative costs associated with each programme's performance of its own assessment of beneficiary eligibility, and improve targeting.
2. Consolidating major cash and in-kind transfer programmes into the PATH programme to ensure:
   - A meaningful level of benefits.
   - A cost-efficient and accessible delivery system.
   - Access to benefits linked to desirable behavioral changes that promote an investment in the human capital development of the poor, especially children.
   - Effective targeting of social assistance to special groups.
3. Improving the targeting, efficiency, and impact of various school-based and other safety net programmes.
4. Developing systems to monitor and evaluate the programmes by building on existing instruments such as the annual Jamaica Survey of Living Conditions (JSLC).

PIOJ and the Ministry of Labour and Social Security (MLSS) were responsible for advancing the reform programme and developing its universal targeting mechanism. To ensure that reforms achieved their goals, PIOJ and MLSS coordinated their efforts with the agencies administering the various safety net programmes.

2. Objectives and Structure

PATH was designed to replace three major income support programmes that provided cash or in-kind assistance: the Food Stamps Programme, the Poor Relief Programme, and the Public Assistance Programme. Reflecting a new approach that combines social assistance with the accumulation of human capital, PATH is intended to fight poverty in the present through monetary transfers and to reduce poverty in the future by encouraging poor households to invest in the health and education of their children. PATH identifies poor households through a scoring formula that ranks households from poorest to best off. Households scoring below a predetermined threshold level are eligible for programme benefits.

PATH is organized around two components:

1. ***Child assistance grants*** provide health and education grants for eligible poor children through age 17.[2] The receipt of health grants is conditioned on children through age 6 (not enrolled in school) visiting a health clinic (every two months during the first year and twice a year thereafter). The receipt of education grants is conditioned on regular school attendance (at least 85 percent of school days) by poor children age 6 through 17. The average monthly benefit per child receiving a health or education grant in 2005 was about US$6.50.[3]
2. ***Social assistance grants to adults*** provide grants to poor pregnant or lactating mothers, elderly poor (over age 65), and poor, disabled, and destitute adults under age 65. Initially, the receipt of benefits was conditioned on adults making regular health clinic visits. However, this changed shortly after the programme was launched and benefits for adults are no longer conditional. The average monthly benefit per person is the same as the benefit in the child assistance grants.

[2]Each child in the household is eligible to receive only one type of grant: health (if age 0 to 5) or education (if aged 6 to 17).

[3]As a reference, the minimum wage in Jamaica for general workers is currently about US$160 per month Economic and Social Survey of Jamaica, 2005). A household receives a monthly grant amount based on the number of eligible members. Hence, a household with two children eligible for the health grant, two children eligible for the education grant, and one eligible adult would receive US$32.50 per month in the first year (i.e., five eligible persons multiplied by US$6.50).

Another important benefit for many PATH households was the government's waiver or payment of certain education and health fees. Such fees covered the annual tuition fee that students must pay, and the cost of a visit to a health centre.

PATH was initially implemented in the pilot parish of St. Catherine in 2001 and began Island-wide implementation in 2002. MLSS and its 13 parish offices at the local level are in charge of executing the programme.

To participate in the programme, a household must apply to its MLSS parish office, providing detailed information about its demographic and socio-economic characteristics in order to allow MLSS to calculate a score on a scale used to determine eligibility. The information is then data entered into a management information system (MIS) at the central MLSS offices, and the household is notified of the eligibility determination. In general, home interviews are not conducted unless there is a specific reason to do so. The bimonthly benefits are disbursed through local post offices.

MLSS staff are responsible for assessing compliance with the education- and health-based conditioning requirements. Every two months, MLSS staff provide schools and health providers with lists of the PATH participants whom the providers serve. The lists include formats for the providers to report school attendance and health care data for the previous two months. About four weeks later, MLSS staff return to the providers to pick up the completed forms. Data from the forms are then entered into PATH's MIS and used as the basis for compliance determinations. Currently, about 245,000 individuals have signed up for PATH, of whom about 180,000 receive benefits in a typical month.

3. Logic Model

In order to interpret the findings from the evaluation, it is useful to first conceptualize what the programme is trying to accomplish through the use of a logic model. While we do not claim

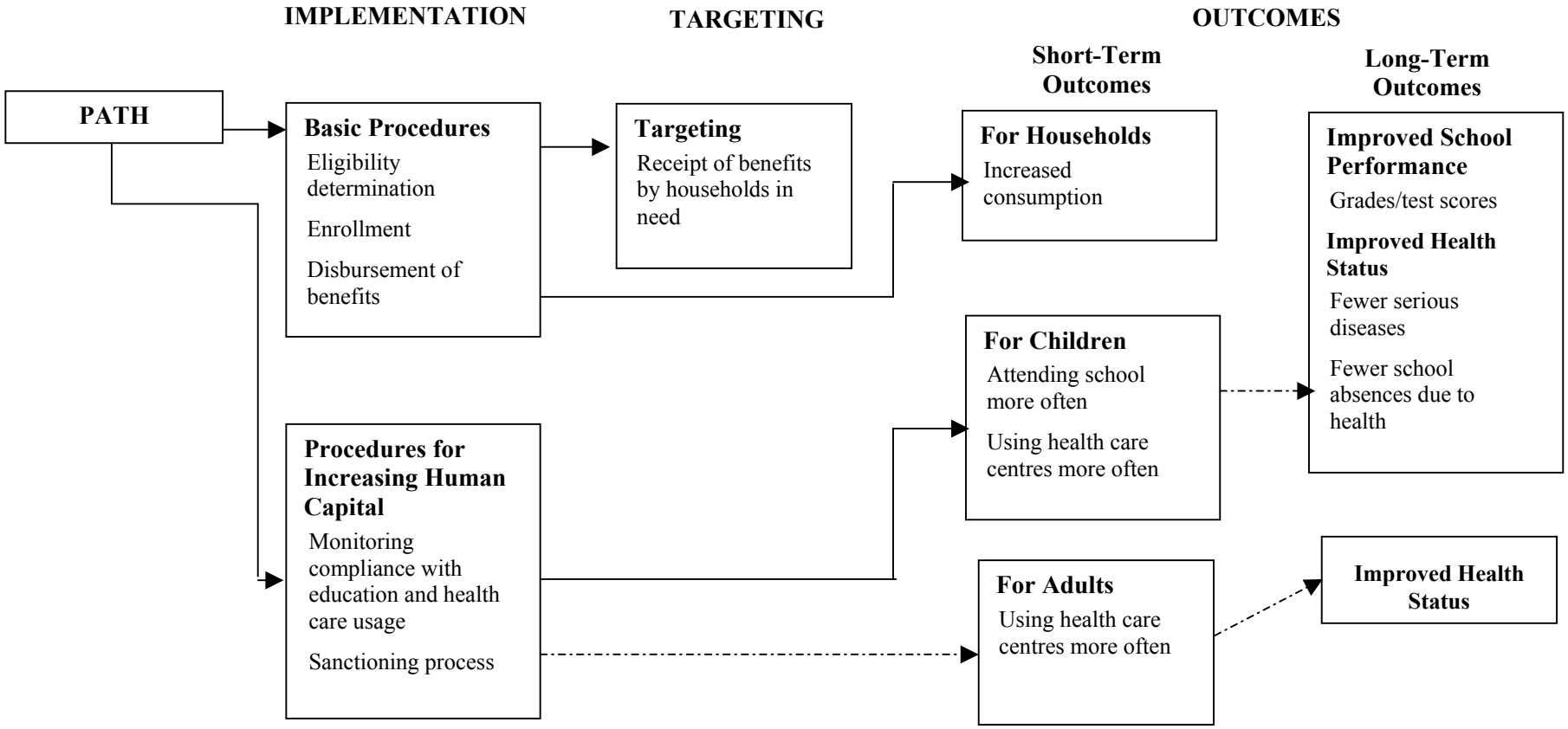
that the logic model proposed below (Figure I.1) fully characterizes every aspect of the programme, we believe it is useful for the purposes of the evaluation.

One aspect of the model is worth highlighting at this stage: the distinction between short and long term outcomes. The short term outcomes are the ones the programme is conditioning its benefits on (i.e. school attendance and preventive health visits). We would expect that if these outcomes will be affected by the programme, that they be affected immediately after the beneficiary enrolls in the programme. The long term outcomes are the ones we would expect to be affected if the programme can achieve impacts on the short term outcomes. They may take longer to affect, and fall in the broad areas of performance in school and health status. These outcomes are "deeper" in the sense that they may be more closely related to the ultimate goal of increasing human capital.

The dotted lines in the diagram reflect effects on the longer term outcomes. They also reflect the decision not to enforce the conditionality on health care for the elderly.

FIGURE I.1

PATH'S LOGIC MODEL



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II. EVALUATION OBJECTIVES AND METHODOLOGICAL APPROACHES

To set the stage for reporting the evaluation findings, we describe the objectives of the research and the evaluation's broad methodological approaches as well as the sources of quantitative data used in the evaluation. Subsequent chapters and a series of appendices present additional technical details about the methodological approaches.

A. KEY OBJECTIVES AND ANALYTIC METHODS

The PATH evaluation was organized around three main lines of research:

- ***Implementation analysis*** assessing the degree to which PATH was implemented along the lines intended
- ***Targeting analysis*** assessing the degree to which PATH benefits are channeled to the poorest households
- ***Impact analysis*** assessing PATH's effects on outcome variables such as school attendance and health care usage

1. Implementation Analysis

Knowledge of how PATH has been implemented, including milestones achieved and problems encountered, provides crucial background information for understanding the later components of the evaluation. An understanding of programme implementation can also potentially contribute to the interpretation of observed outcomes.

The implementation analysis relied largely on *qualitative methods.* We conducted site visits to selected areas on the Island to collect qualitative data at two junctures during the evaluation. These site visits focused on PATH participants' programme experiences (as reported in focus groups) and on information gathered through structured interviews of staff involved with the programme. Chapter III presents the results and describes additional methodological details of the work.

2. Targeting Analysis

An important objective of PATH has been to increase the ability of Jamaica's social safety net system to channel assistance to households and individuals most in need of financial help. Therefore, the targeting analysis focused on the degree to which the programme has succeeded in channeling assistance to the poor as defined by GOJ and as compared with other assistance programmes in Jamaica.

The main approach to the targeting analysis called for conducting (through the Jamaican survey organization, STATIN) a detailed survey of the consumption levels and circumstances of a random sample of PATH participants. (Section C provides more information on the survey.) To assess the circumstances of PATH recipients relative to other Jamaicans, we compared the survey data with data from Jamaican population surveys conducted externally from the evaluation. We also used the PATH survey data to assess the degree to which PATH recipients fell below the official Jamaican poverty line established by GOJ. We obtained quantitative information on participants for the targeting analysis from a second source, the MIS system used to administer PATH. Chapter IV presents the results of the targeting analysis.

3. Impact Analysis

As noted, a central objective of PATH has been to increase human capital among low-income Jamaicans by conditioning assistance on meeting the programme's standards for education attendance and health care usage. Accordingly, an important component of the evaluation involved an examination of PATH's effects on these programme standards.

To conduct the impact analysis, we compared key outcome variables for a sample of PATH participants (called the participant group) with those for a sample of non-participants (called the comparison group). We chose the groups to be as similar as possible (see below) in order to isolate the effects of PATH. STATIN conducted two surveys of the groups. One was a baseline

survey conducted before members of the PATH sample began receiving benefits, and the other was a post-implementation survey conducted toward the end of the evaluation. We analyzed the resulting information by using a regression technique that exploits information based on both the longitudinal design (which was used to control for environmental changes over time) and the similarity of the participant and nonparticipant samples (which helps isolate the specific effects of the programme). Chapters V and VI describe the technique and the analysis results.

B. MAJOR DATA SOURCES FOR THE TARGETING AND IMPACT ANALYSIS

As noted above, the evaluation involved three household surveys:

- The targeting survey of PATH participants was conducted on a random sample of PATH-eligible households at approximately the start of the evaluation in February-May 2003
- The baseline and post-implementation surveys of the longitudinal impact sample were conducted on PATH participants and comparison group members. In particular, participants and comparison group members were first interviewed in the first semester of 2004, with a follow-up conducted on the same samples in the summer of 2005

In addition, administrative data were extracted from the PATH MIS. Table II.1 presents the key data sources that were used in the evaluation, their target samples, main purpose, and reports in which they were used.

1. Targeting Survey

The targeting survey, conducted by STATIN as an in-person survey, was designed to provide a database with which to assess the targeting of PATH to low-income households. For the most part, interviewers were drawn from the pool of interviewing staff whom STATIN deploys for its ongoing work on other surveys, including the JSLC.

TABLE II.1

DATA SOURCES FOR THE EVALUATION
(Sample, Time Period, and Main Purpose of Each Data Source)

Data Source	Sample	Time Period	Main Purpose	Evaluation Report(s)
MIS data	All applicants to PATH as of May 2003 (Sample size: 196,628 households as of May 2003)	Collected at several points during the evaluation	Select participant and comparison groups for impact analysis, describe PATH participants at baseline, provide eligibility score for impact analysis	Methodology report Targeting report Interim report Final report
Survey of Living Conditions 2002	Representative sample of Jamaican households (Sample size: about 7,000 households)	Summer 2002	Compare PATH participants to the overall Jamaican population.	Targeting report
Targeting survey	Nationally representative sample of eligible PATH households as of November 15, 2002 (Sample size (target): 1,200 households)	February-May 2003	Describe PATH participants as they enter the programme and assess PATH targeting	Targeting report
Baseline survey	Participant group and comparison group (Sample size (target): 2,500 participant households and 2,500 comparison households)	January-June 2004	Provide baseline data on both the participant and comparison groups for the impact analysis	Interim report Final report
Follow-Up survey	Participant group and comparison group (Sample size (target): 2,500 participant households and 2,500 comparison households)	May-October 2005	Provide outcome data on both the participant and comparison groups for the impact analysis	Final report

Given the advisability of comparing information from the targeting survey to JSLC data, the targeting survey was closely patterned after the JSLC survey, though with the addition of several questions designed to obtain information about respondents' experiences with PATH, and more detailed questions about outcomes related to education and health to gain a richer understanding of the programme's impacts.

The sample for the targeting survey was a random sample of applicants who had been approved for PATH as of the time the survey was conducted. The initial sample included 1,205 households. The targeting survey achieved an 80 percent response rate, but only 82 percent of respondents actually entered the PATH programme after they were approved for it. Given these compounding sources of sample attrition, the final number of PATH participants available for analysis from the survey was 794 households.[4]

2. Longitudinal Survey

STATIN conducted the first wave of the longitudinal survey (called the baseline survey) in January-June 2004 and the second wave (called the follow-up survey) between May and October 2005. Here we describe both the sampling for the surveys and the data collection effort.

Sampling. Sampling for the longitudinal survey was designed to maximize the similarity between participants and the comparison group in the impact analysis. In particular, to maximize the reliability of the impact analysis, it was important to obtain data on samples of participant and nonparticipant households that would be as comparable as possible. From the perspective of pure research, the ideal way to define the samples would have been to randomly assign a group of approved applicants into treatment and control groups, and delay services to the control group until after the study. However, such an approach was judged by GOJ to be operationally

[4] 1205 times 0.80 times 0.82.

unfeasible. We therefore selected the participant and comparison groups in a different manner. The participant group was selected from a set of PATH participants who were *just barely eligible for PATH in terms of their score on the eligibility determination scale.* (See Chapter I for a discussion of how the score is used in administering PATH.) We selected the comparison group from among applicant households that were *just barely ineligible for PATH* (i.e., just above the scale cutoff). As discussed in more detail in Chapter V, we believed that the two groups of households would be similar to each other in that their scores on the eligibility scale were extremely close. Furthermore, because the eligibility score fully determines eligibility for PATH, any remaining differences can be statistically controlled for in regression analysis by including the eligibility scores in the regression specifications.

To implement the sampling approach, MPR obtained files with the records of both participants and ineligible applicants from the PATH MIS system. MPR then selected the participant and comparison groups, and sent lists of the samples to STATIN for interviewing.

Data Collection Instrument. As in the targeting survey, the instrument for the data collection was closely patterned after the standard JSLC, with the addition of a small number of PATH-specific questions. In administering the survey, MPR requested that STATIN make sure that the progress among participants roughly paralleled the progress among nonparticipants in order to prevent biased results associated with the different time patterns for interviewing the two groups. Periodic reports indicated that STATIN succeeded in minimizing bias. See Appendix 1 for details.

Response Rates. Overall, the longitudinal surveys achieved response rates of 90 and 82 percent (for baseline and follow-up respectively); Appendix 1 presents tabulations comparing the two samples in terms of their response rate patterns. According to measurable characteristics,

the tabulations indicate that the participant sample is indeed similar to the nonparticipant sample.[5]

3. MIS Data

The MIS system used by PATH contains detailed data on the household characteristics of PATH participants, as obtained on the application form, as well as records of compliance with "conditioning criteria" (education attendance and health clinic usage). The MIS system also maintains data on PATH status and payments made under PATH.

The MIS data served two important purposes in the evaluation. First, as noted above, the MIS provided the sample frames for the survey work. Second, it allowed MPR to conduct tabulations of characteristics for censuses of all PATH participants. This was useful both in maximizing the precision of certain tabulations and in allowing us to assess the representativeness of the samples relative to the universe of PATH participants.

Early in the project, MPR obtained lists of variables and file layouts for the MIS from GOJ staff. MPR made periodic extract requests. In general, the extracts included the complete records of the relevant households. MPR undertook the final extracting of needed variables and applied editing and consistency checks.

[5] Nonresponse weights were not created for two reasons: (1) the differences between respondents and nonrespondents tended to be relatively small, and (2) the response rates for both the baseline and follow-up survey were relatively high.

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III. IMPLEMENTATION ANALYSIS

Any social programme is subject to differences between its intended design and its operation "on the ground." Here, based on the site visit research conducted by MPR's subcontractor Hope Enterprises, Ltd., we assess the implementation of the PATH programme.

After a section describing the methodology used in the implementation analysis, we will discuss the following aspects of programme operation:

- Eligibility determination process
- Benefit adequacy
- Determining compliance with programme conditions concerning education and health care usage
- Disbursement of benefits
- Overall reactions to PATH among stakeholders

A. METHODOLOGY

The research reported in this chapter is based on two rounds of site visits. The first round occurred shortly after PATH's implementation; the second round took place in spring and summer 2005, toward the end of the evaluation. For each round, staff from MPR's subcontractor Hope Enterprises, Ltd., conducted site visits to five randomly chosen parishes throughout the Island.[6]

During each visit, Hope staff conducted focus groups with randomly selected PATH beneficiaries and then conducted one-on-one interviews with staff representing key government organizations involved in PATH, including:

[6] The sample was stratified by region and PATH compliance rate to ensure that various regions and compliance rate categories were represented in the selected sites.

- MLSS parish offices
- Schools
- Health care clinics
- Post offices

In the interviews, HOPE staff relied on structured protocols to obtain information on the implementation processes and any associated difficulties.

Senior members of Hope Enterprises conducted the qualitative data collection, with the participation of the company's president. Staff summarized the findings in reports developed after each round of visits.[7] Tables III.1 and III.2 summarize the sampled locations for the two rounds of data collection.

TABLE III.1

FOCUS GROUP DISCUSSIONS AND INTERVIEWS WITH STAKEHOLDER INSTITUTIONS BY LOCATION: FIRST ROUND

Parish	Location of PATH Clients (Postal Zone)	Location of Schools	Location of Health Centres	Location of Post Offices
Kingston and St. Andrew	Kingston	St. George's Girls	Maxfield Park	King Street
St. Ann	Blackstonedge	Guys Hill	Blackstonedge	Blackstonedge
Manchester	Walderston P.O.	Christiana	Mandeville	Christiana
Trelawny	Clarks Town	Clarks Town	Clarks Town	Clarks Town
St. Elizabeth	Seaford Town/ St. Leonards	Seaford Town	Seaford Town	Seaford Town/St. Leonards

[7] The two reports are: (1) Wedderburn, Maxine; Pansy Hamilton; and Deborah Bourne. "First Qualitative Assessment on Jamaica's PATH Programme" Hope Enterprises Ltd. April 2004; and (2) Wedderburn, Maxine; Tricia-Ann Morris; and Pansy Hamilton "Second Qualitative Assessment on Jamaica's PATH Programme" Hope Enterprises Ltd. December 2005.

TABLE III.2

FOCUS GROUP DISCUSSIONS AND INTERVIEWS WITH STAKEHOLDER INSTITUTIONS BY LOCATION: SECOND ROUND

Parish	Location of PATH Clients	Location of Schools	Location of Health Centres	Location of Post Offices
Kingston and St. Andrew	Jones Town	John Mills Primary and Junior High	Comprehensive Health Clinic	Cross Road
St. James	Cambridge	Cambridge High	Cambridge	Cambridge
Hanover	Dias	Riverside All Age	Dias	Dias
St. Thomas	Yallahs	Yallahs Primary	No interview conducted	Yallahs
Clarendon	Moravia	Moravia Primary	Moravia Health Centre	Moravia

B. ELIGIBILITY DETERMINATION

As noted, programme eligibility determinations are made by computing an eligibility score that is based on answers provided by households on their applications. Eligibility determination takes into account household demographics and resources.

Beneficiary focus group participants believed that the eligibility determination process was generally fair, but they also believed that mistakes sometimes occur. The beneficiaries recognized that a comprehensive system is necessary to make selections for the programme. However, they felt that the information used to determine selection, the length of time required for approval, and the gap between approval and disbursement of benefits are taxing and onerous. Several respondents believed that some persons seek to obtain benefits by unfair means, including the deliberate provision of incorrect information on the eligibility form. They felt that the situation is perpetuated because PATH's assessment and monitoring activities are not as rigorous as they could be. Some remarked that PATH officers neither visited applicants' homes/communities nor checked with the schools when assessing their eligibility.

Beneficiary focus group participants often felt that questions on the application about the possession of appliances such as refrigerators, televisions, and stoves, were unreasonable because such items are now basic household items, along with flush toilet facilities. Focus group participants believed that many currently unemployed parents had been formerly successful small business persons and had acquired appliances during prosperous periods. Thus, beneficiary focus group participants did not consider ownership of appliances to be indicative of their *current* state of well-being or ability to afford to send their children to school. As one participant stated:

> *"Not because you have furniture mean you have money. Like my mother dead left plenty of things for me but me can't afford food and the fridge need to fix and me can't fix it."*

Participants stated that a thorough initial home visit by PATH officials would help provide a better assessment of those in need.

In all the focus groups, beneficiaries spoke of knowing individuals who are in need but whose applications were rejected. They also indicated that some beneficiaries' economic status should have prevented acceptance into the programme. Respondents clearly felt that the assessment of applicants requires an in-depth investigation and that the application information should undergo routine evaluation through visits by PATH staff to the community and home to ensure that those most in need receive benefits.

As indicated in interviews conducted by HOPE staff, teachers and guidance counselors in the schools attended by PATH beneficiaries also believe that the eligibility determination process often leads to errors. Like the beneficiaries, teachers observed that some children in their schools should be on PATH but are not and that some beneficiaries on PATH should not be receiving programme benefits. Teachers and guidance counselors felt that schools should collaborate in making recommendations for student participation in PATH. Teachers voiced

strong concern about needy children who are not on the programme. School staff thought that guidance counselors are ideally placed in the school system to contribute to and coordinate the eligibility recommendations.

C. ENFORCING THE REQUIREMENTS FOR EDUCATION AND HEALTH CARE

While the system for enforcing compliance with the conditioning requirements has clearly achieved a basic level of functionality, many beneficiaries who participated in focus groups believed that the programme faces significant problems in providing education and health care. Programme staff included in the staff interviews also indicated some level of difficulties in these areas.

1. Education Requirements

Some beneficiaries noted a tendency for MLSS to suspend their PATH benefits in response to incorrect compliance information. Approximately 10 percent of the children in households of focus group participants had been suspended for various reasons, most often nonattendance at school. In the majority of cases, the parent disputed the reason for nonattendance. Generally, the error was thought by the parents to lie with the PATH office rather than with the school records. In fact, some parents were of the opinion that the suspensions were deliberate on the part of MLSS and, in support of their opinion, stressed the fact that MLSS did not make retroactive payments for lost payments. On the other hand, a few parents admitted that their children were still receiving grant payments even though they were not attending school as required.

Nonattendance at school by reason of illness also presented a problem. Nonattendance for illness generally requires a doctor's certificate, and the inability to furnish such a certificate has resulted in PATH benefit suspensions. The cost associated with furnishing the certificate includes not only the doctor's fee but also the cost of traveling to and from the doctor's office and the time and money lost from work. Parents felt that, depending on the illness, it is not

always necessary to seek medical assistance. To them, the PATH documentation requirements meant an unnecessary expense that is often beyond the means of the household, even though parents understood that the doctor's certificate is required. Parents felt that they should have discretion in determining when a letter from a parent to the school should suffice to explain a child's illness-related absence.

Beneficiaries also said that suspensions occurred when children moved from primary to high school or other schools. In such cases, the children were sometimes reported as not attending school, because the PATH programme records had not noted the transfer. Some parents indicated that suspensions occurred even when they informed the PATH office of the child's school transfer. Many parents indicated that they were sometimes uncertain as to the reasons for their children's suspension from the programme:

> *"I know someone who has a lot of kids and they hardly go to school but they still get it and my daughter go school everyday and don't get it."*

Some parents found that resolution of a suspension is a tedious process and therefore decide not to rectify matters. They tie their decision to what they saw as the negative attitude of some field officers, slow response to complaints and queries, and the distance and cost associated with traveling to the PATH office.

School teachers also saw problems with monitoring. Teachers regularly provide PATH with student attendance records, and make corrections, as appropriate, based on information from parents. Teachers, however, reported two sources of concern with corrections. First, they noted that parents do not always cooperate and are usually late in providing information with respect to an illness resulting in a child's absence. Second, teachers commented that staff at the MLSS office reportedly take a long time to correct the errors; as a result, reinstatement takes a long time, and the programme makes no retroactive payments for valid periods of attendance.

Staff of most schools indicated that PATH's monitoring is minimal, with main activities limited to collecting registers and dropping off reports. The suspension of compliant students is as much a problem as is reinstatement.

2. Health Care Requirements

Parents pointed to issues related to compliance with the health care usage requirements for their children and drew a distinction between health centres and other health care providers. Parents generally relied on the former to complete the required immunization for PATH children under age six and generally viewed their encounters with the centres as satisfactory. However, many clients believed that, once their children's immunizations were completed, they no longer had to take their children to the clinic regularly unless a child was sick. As a result, many families did not fully comply with the health care requirement and, as a result, frequently found themselves suspended from the programme.

Some parents also reported that they were unaware of how the health benefits work. Some believed that they should take a child to the health centre only in the case of illness. Other parents noted the high costs of travel to and from the clinic or hospital. In addition, they complained that taking the child out of school in order to obtain preventive care would interrupt the child's schooling and require time off from work ("hustling").

Beneficiaries successfully used the PATH health card to access services mainly at health centres, but they said they had trouble with the card at hospitals. Some hospitals accepted the cards while others did not; still others required payment with the card. Continuing uncertainty surrounds the entitlement of PATH beneficiaries at hospitals, and the experience of PATH beneficiaries at hospitals with respect to payments for medical consultation as well as diagnostic and therapeutic services is uneven and confusing. Moreover, PATH beneficiaries felt snubbed whenever they presented the PATH health card. In the words of one individual:

"When you go to the hospital them run you like they don't recognize the PATH."

D. BENEFIT ADEQUACY

While noting that the PATH benefit was not itself sufficient to sustain a family, most focus group members expressed a degree of satisfaction with the level of the benefit, agreeing that benefit levels are reasonable. Parents generally agreed that the PATH cash grant provides vital support for sending their child to school by covering the purchase of school shoes, books, and clothes and by providing access to health care. Sometimes benefits were sufficient to provide one or more meals for the family. Some parents, however, expressed concern that they could not always afford to provide their children with lunch money for school.

Many focus group participants also emphasized that the value of the programme goes beyond direct monetary payment. They viewed the waiver of tuition fees and health care payments as a particularly important component of the overall benefit package.

E. DISBURSEMENT OF BENEFITS

Beneficiaries receive PATH benefits in the form of checks sent to them by MLSS through the local post office. Focus group participants seemed to be generally satisfied with the logistics of disbursement, but they reported that the checks often do not arrive at the post office on the scheduled day, thus posing a significant problem. In particular, they indicated delays of up to one week past the announced date when the checks are to be available. The late delivery of checks causes problems for at least two reasons. First, many PATH households have no spare cash and therefore experience difficulty in managing their finances when checks do not arrive. For instance, the late arrival of a check can make the difference between lunch money or no lunch money for a child. Second, beneficiaries often have to make numerous trips to the post office to obtain their check, a process that can be expensive in terms of time and, in some

instance, bus fare or other travel expenses. Generally, beneficiaries viewed post office staff as helpful, although some beneficiaries reported a few incidents of unprofessional treatment.

Beneficiaries raised several issues concerning the cashing of checks. Some recipients reported that cashing the check at a bank requires valid identification, which is not always available; others said that they have to deposit the checks in an account and may not draw on the check immediately. Traveling to the bank in a main town was also costly. Others tried to avoid reliance on a bank by cashing the check at a place of business, usually a supermarket, but the business usually expected to the beneficiary to spend some of the proceeds in the store even though the beneficiary did not care to do so. Respondents suggested a need for a facility to cash the checks without restrictions.

Post office staff echoed beneficiary concerns about the late arrival of checks, saying that payments are due on the 15th of each month but sometimes arrive as late as the 28th. Post office staff noted that many beneficiaries do not have a telephone or access to one and so are unable to call to find out if their check has arrived. Post office staff also expressed concern about issues such as duplicate checks, incorrect names, erroneous suspensions, and the late removal of the names of deceased beneficiaries. In the last case, persons appointed to make collections sometimes do not report the death of the beneficiary and continue to collect the checks as long as post office staff are unaware of the beneficiary's demise. In other instances, MLSS continues to issue checks even after the postal staff notifies MLSS of a beneficiary's death.

F. SUMMARY

As with all complex undertakings, the implementation of PATH has encountered various problems, many of which are ongoing and some of which are significant. These include coordination problems between agencies involved in monitoring compliance, delays in the delivery of checks, and inconsistencies in the benefit determination process. Nonetheless, both

beneficiaries and key stakeholders involved in programme administration viewed PATH as generally successful.

IV. BENEFIT TARGETING

As noted in Chapter II, the second of the three overarching objectives of the PATH evaluation has been to assess the success of PATH in targeting benefits to the poor. One of the most important motivations for PATH was a concern that, instead of focusing on the poor, the benefits of the existing public assistance programmes were spread too broadly over households representing a cross-section of Jamaica's income distribution. This chapter discusses selected findings from the targeting analysis. An earlier report presents a complete assessment of PATH's targeting.[8]

To assess targeting, we must first define and operationalize what we mean by "targeting." Several definitions of targeting are useful in providing policymakers with needed information for assessing the distribution of benefits. Section A provides a methodological basis for the remainder of the chapter by discussing alternative targeting concepts as well as other methodological issues that need to be considered. Subsequent sections present the research findings.

A. METHODS

1. Measure of Well-Being

Targeting issues are closely related to measuring household well-being. Two commonly used measures of well-being are income and consumption. Given the policy context of the current study, we have chosen to focus our targeting analysis on consumption.

Consumption-based measures find wide application in the developing world and, for both theoretical and practical reasons, are commonly viewed as preferable to income-based measures

[8] Levy D. and Jim Ohls, "Evaluation of Jamaica's PATH Programme: Targeting Assessment" July 2003.

(Deaton and Zaidi 1999). The main theoretical reason is that estimates of current consumption are more likely than estimates of current income to provide a reliable estimate of a household's long-run standard of living. As Skoufias et al. (1999) report, current income may be more subject to temporary shocks, especially if households engage predominantly in agricultural and self-employment activities. In contrast, consumption can be smoothed to some extent by saving and borrowing.

As for the practical considerations involved in using consumption instead of income as a measure of household well-being, it is important to recognize that consumption is likely to exhibit lower seasonal variability than income. Moreover, obtaining a reliable measure of annual consumption is likely to require fewer interviewing visits than those required for obtaining a reliable measure of annual income. Furthermore, income is usually considered a highly sensitive topic for an interview; hence, households may be more likely to misreport income than consumption. Another salient issue in the Jamaica context is that there is more background data on household consumption than on income because consumption is the focus of the annual JSLC.

2. Relevant Indicators of Poverty of Recipients

The targeting analysis assesses the extent to which programme benefits reach the intended beneficiaries of households in poverty. The notion of households in poverty, however, raises another set of key issues--what indicators of poverty to use for the study. Three key indicators are used below:[9]

[9] The algorithms used to calculate these indicators are consistent with those used in the 2002 JSLC, the data source used to calculate official poverty-related statistics in Jamaica. The indicators are calculated at the level of the individual, not the household. So if the poverty rate is X percent, it means that, of all the people living in a household who are receiving PATH assistance, X percent are poor.

1. The ***poverty rate among PATH participants***[10] indicates the percentage of PATH beneficiaries considered poor. The higher the rate, the better is the targeting performance. To assess whether a PATH beneficiary is poor, we compare the adult-equivalent consumption for that beneficiary (adjusted for inflation) with Jamaica's official poverty line.[11] The adult-equivalent consumption takes into account the consumption needs of the household based on the number, age, and gender of its members. The *adult equivalency scale* for a household represents the household size in terms of the expected food consumption of an adult male. For instance, suppose that a household consists of one adult male plus two children who, based on their age and gender characteristics, are each expected to consume three-quarters of the amount of food consumed by an adult male. Then the household size is three, but the size in terms of adult equivalencies is 2.5.

2. The ***rate of extreme poverty among PATH participants*** indicates the percentage of PATH beneficiaries considered to be in extreme poverty, that is, unable to afford basic nutritional needs. The higher the rate, the better is the programme's ability to target the extreme poor. To assess whether a PATH participant was in extreme poverty, we compared the adult-equivalent consumption for that beneficiary with the food poverty line, defined as the food expenditure necessary to attain some recommended minimum food-energy intake. We used the food poverty lines reported in the 2002 JSLC and adjusted them for inflation.[12]

3. The ***quintile distribution*** indicates the percentage of PATH households that fall into each of the quintiles of the national distribution of per capita consumption in Jamaica. To assess the quintile into which a PATH beneficiary falls, we compare the per capita consumption of that beneficiary with the quintile cutoffs from the 2002 JSLC (adjusted for inflation). The higher the percentage of beneficiaries falling into the lower quintiles, the better is the targeting performance of PATH.

3. Coverage Rate

While an assessment of the extent of poverty among PATH beneficiaries is critical for determining PATH's success in targeting poor households, it is also useful for determining the extent to which PATH was able to reach the population of all poor households in Jamaica. An

[10] The poverty rate among PATH participants is the counterpart of the "leakage rate," an indicator often used in targeting assessments. While the poverty rate represents the percentage of programme participants who are poor, the leakage rate represents the percentage who are *not* poor. Hence, poverty rate + leakage rate = 100 percent.

[11] The poverty line STATIN used to calculate Jamaica's poverty rate varies by geographic area. For 2002, the poverty line in Jamaica was J$50,640 for the Kingston Metropolitan Area, J$48,262 for other towns, and J$44,940 for rural areas. To assess whether a PATH beneficiary was poor, we compared the inflation-adjusted adult-equivalent consumption level with the relevant poverty line for that beneficiary. If consumption was below the poverty line, the household was considered poor. Otherwise, the household was considered nonpoor.

[12] Essentially, the second definition is the same as the first, except that the threshold is lower, reflecting the "food poverty line" rather than the general poverty line.

indicator often used to quantify the extent of coverage is the *coverage rate*: the percentage of poor households in the country served by the programme. Given that we lack a data set both nationally representative of Jamaica and encompassing data on a sizable number of PATH households, we are somewhat limited in our ability to assess PATH's performance in terms of coverage.[13] However, we will provide an approximate assessment by using estimates of the total number of poor households in Jamaica.

4. Survey Timing Issues

In principle, the targeting assessment should be based on some measure of the well-being of participants before they received programme benefits. Therefore, the main targeting survey was originally designed to be administered before participants started receiving PATH benefits. However, owing to various delays in fielding the survey, about two-thirds of survey respondents reported that they had registered for PATH before responding to the survey.

Appendix 2 examines the sensitivity of our targeting estimates to potential error caused by the delays in survey administration, taking into account the size of the PATH payments in relation to the consumption information obtained in the survey. The appendix suggests that the delay had no substantial effect on the overall pattern of results reported in this chapter.

5. Data Sources

The primary source of data is the targeting survey conducted in 2003 with a random, representative sample of PATH beneficiaries (see Section II.B.1). In certain parts of the analysis, we also draw on information from the 2002 JSLC, a survey representative of the entire

[13] The 2002 JSLC covers a nationally representative sample of Jamaicans, but, given that it was conducted in 2002 (before PATH was fully implemented), few respondents were participating in PATH at the time.

Jamaican population of households. We also turned to information from the PATH MIS (see Section II.B.3).

B. ARE PATH PARTICIPANTS POOR?

Most PATH participants are poor. About three of every five PATH participants have consumption levels below the poverty line (Tables IV.1 and IV.2). By contrast, the overall poverty rate in Jamaica is about 20 percent (one of five). About four of every five PATH participants fall into the bottom two quintiles of the consumption distribution, which means they are among the lowest 40 percent of Jamaican households in terms of adult-equivalent consumption levels.

TABLE IV.1

OVERALL ASSESSMENT OF PATH'S TARGETING PERFORMANCE
(Percentage of PATH Beneficiaries Falling into Each Category)

Quintile	Percent
I	58
II	21
III	14
IV	5
V	1
Poor	59
Extreme poor	27

Source: PATH 2003 Survey, 2002 Jamaica Survey of Living Conditions (SLC 2002).

Quintile I corresponds to the percentage of PATH beneficiaries in the bottom 20 percent of Jamaica's per capita consumption distribution. Quintile V corresponds to the percentage of PATH beneficiaries in the top 20 percent of Jamaica's per capita consumption distribution.

Poor: People whose adult-equivalent consumption is below Jamaica's official poverty line. The percent of poor for the overall population in Jamaica is about 20%.

Extreme poor: People whose adult-equivalent consumption is below Jamaica's official food poverty line. The percent of extreme poor for the overall population in Jamaica is about 8%.

Among those participants who are poor, about half are in particularly dire circumstances. About one of every four participants is in extreme poverty (Table IV.1), which implies that

slightly fewer than half of PATH's poor participants are able to afford their basic nutritional needs. Furthermore, about a third of PATH participants are among the poorest 10 percent of Jamaicans (Table IV.3), and about 11 percent of participants have levels of consumption that are lower than *half* the poverty line (Table IV.2).

TABLE IV.2

ASSESSMENT OF PATH'S TARGETING PERFORMANCE
(Distribution of PATH Beneficiaries by Poverty Status)

Consumption/Poverty Line	Percent	Cumulative
Less than 50%	11	11
50–60%	9	20
60–70%	8	29
70–80%	9	38
80–90%	13	51
90–100%	8	59
100–110%	6	64
110–120%	8	72
120–130%	5	77
130–140%	4	81
140–150%	2	83
150–200%	11	94
More than 200%	6	100

Source: PATH 2003 Survey, 2002 Jamaica Survey of Living Conditions (SLC 2002).

Note: Consumption/Poverty Line represents the fraction of adult-equivalent consumption over the poverty line.

While most participants are poor or close to being poor, a small fraction of PATH participants seem to be relatively well off. About 6 percent of households fall into the top two quintiles of the consumption distribution (Table IV.1). About 17 percent of PATH participants have consumption levels greater than 1.5 times the poverty line, and about 6 percent have levels greater than twice the poverty line (Table IV.2).

TABLE IV.3

ASSESSMENT OF PATH'S TARGETING PERFORMANCE
(Distribution of PATH Beneficiaries by Decile)

Decile	Percent	Cumulative
1	33	33
2	24	58
3	13	71
4	9	79
5	8	87
6	6	94
7	3	97
8	2	99
9	1	100
10	0	100

Source: PATH 2003 Survey, 2002 Jamaica Survey of Living Conditions (SLC 2002).

C. TO WHAT EXTENT HAS PATH REACHED THE POOR?

As noted, it is useful to assess the extent to which PATH was able to reach poor households in Jamaica. An indicator often used to quantify coverage is the coverage rate--the percentage of poor Jamaican households served by PATH

We estimate PATH's coverage rate of the poor as follows: As of 2002, the overall estimated poverty rate in Jamaica was about 20 percent. With Jamaica's population at approximately 2.6 million, the total number of Jamaicans living in poverty is 520,000. As noted in Chapter I, the current number of beneficiaries receiving PATH benefits in a typical period is approximately 180,000 and, based on the data in Table IV.2, 59, percent of these enrollees are poor.[14] Thus, PATH is currently reaching approximately 106,000 poor beneficiaries. Overall, therefore, the programme's coverage rate is about 20 percent (106,000/520,000).

[14] Note that about 250,000 individuals are currently enrolled in PATH, but only about 180,000 receive benefits in a given time period. This difference is due to the fact that, for any given period, some beneficiaries do not comply with the PATH requirements.

In assessing this estimate, we should note that PATH has never been funded at a participation level that would allow full coverage. The target number of beneficiaries in PATH is about 236,000. Even with "perfect" targeting to the poor, the coverage rate would reach about 45 percent of beneficiaries, only about half of full coverage.

D. COMPARISONS WITH OTHER TRANSFER PROGRAMMES IN JAMAICA

Assessing whether PATH's targeting performance has been successful depends on how "success" is defined. This is a normative judgment, and we know of no explicit goal set by GOJ in terms of what it would consider a targeting "success." However, this section provides a benchmark by comparing PATH with other social programmes in Jamaica.[15]

Relative to other social programmes in Jamaica, PATH's targeting performance seems to be good (Table IV.4). In particular, PATH has achieved much better targeting than Food Stamps, one of the main programmes for which it was to provide a substitute. About 59 percent of PATH beneficiaries fall into the bottom quintile as compared with 36 percent of Food Stamp participants. In addition, about four of every five PATH beneficiaries fall into the bottom two quintiles of the consumption distribution as compared with only about three in five Food Stamp participants. In addition, 6 percent of PATH beneficiaries fall into the top two quintiles as compared with 18 percent of Food Stamp participants. Overall, PATH seems to be doing a much better job in targeting the poor than Food Stamps.

In targeting the poor, PATH also seems to be doing as well as or better than other Jamaican assistance programmes, although the small sample sizes available to estimate targeting for some of the other programmes suggest a need for caution in drawing this conclusion. PATH's

[15] Chapter VII compares PATH targeting with that of conditional cash transfer programmes in some other countries. The conclusion is that the degree of targeting achieved by PATH appears to compare favorably with the targeting of several similar programmes in other countries.

targeting performance seems to be similar to that of Public Assistance (about 60 percent of participants are poor, and about 80 percent fall in the bottom two quintiles). It is important to note, however, that the number of people receiving Public Assistance is much smaller than the number of PATH beneficiaries. Regarding other programmes, PATH seems to be better targeted than Poor Relief, the School Fee Assistance Programme (SFAP), and the Jamaica Drugs for the Elderly Programme (JaDEP). As the last column of Table IV.4 shows, the sample sizes used to calculate targeting indicators for these programmes are particularly small such that the conclusions presented here are tentative.

TABLE IV.4

ASSESSING PATH'S TARGETING RELATIVE TO OTHER SOCIAL PROGRAMMES IN JAMAICA
(Estimated Percent of Beneficiaries in Each Consumption Quintile)

		Distribution of Benefits by Quintile					
Benefit Programmes	Poverty Rate	Lowest 20%	21–40%	41–60%	61–80%	81–100%	Sample Size
Food Stamps	36	36	26	20	12	6	1,558
SFAP	22	20	21	25	23	11	605
Public Assistance	61	60	17	14	5	4	68
Poor Relief	36	35	23	22	15	4	100
JaDEP	13	9	19	42	13	17	113
PATH	**59**	**58**	**21**	**14**	**5**	**1**	**4,287**

Source: PATH's figures based on 2002 JSLC and 2003 PATH Survey.

Note: Figures for other social programmes are reported in Fawcett (2004). Social Safety Net Report; IMPAQ International. Prepared for Mathematica Policy Research and GOJ.

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V. PATH'S IMPACTS ON EDUCATION-RELATED VARIABLES

A key objective of PATH is to provide incentives for participants to increase their accumulation of human capital. Most important, GOJ hoped that PATH households would more regularly send their children to school and use health care facilities for preventive care, particularly for young children.

These two outcomes are the focal points of the PATH outcomes evaluation. The first is important in that higher attendance rates can potentially lead to better educational outcomes. While schools in Jamaica had a relatively high *enrollment* rate at the outset of PATH, there was concern that, for various reasons such as tuition fees and transportation costs, many students were not being sent to school as regularly as they could have been.

Similarly, there was concern that poor children were not being taken to health centres for preventive care as often as desirable, particularly in their first year of life. GOJ hoped to address this concern by requiring a minimum number of such visits as a condition of the grants. The same is true for elderly members of PATH households.

In addition to these two focal outcomes, there are a number of related useful outcomes that PATH could have affected and that have been included in the evaluation including improved school achievement (grades and advancement to next grade), lower rates of child employment, higher immunization rates, and improved health care status

This chapter and the one that follows document the impacts of PATH on both education and health outcomes. This chapter, which focuses on key education outcomes, covers the methodology for the impact analysis and the findings on PATH's impacts on school attendance. Chapter VI documents health outcomes.

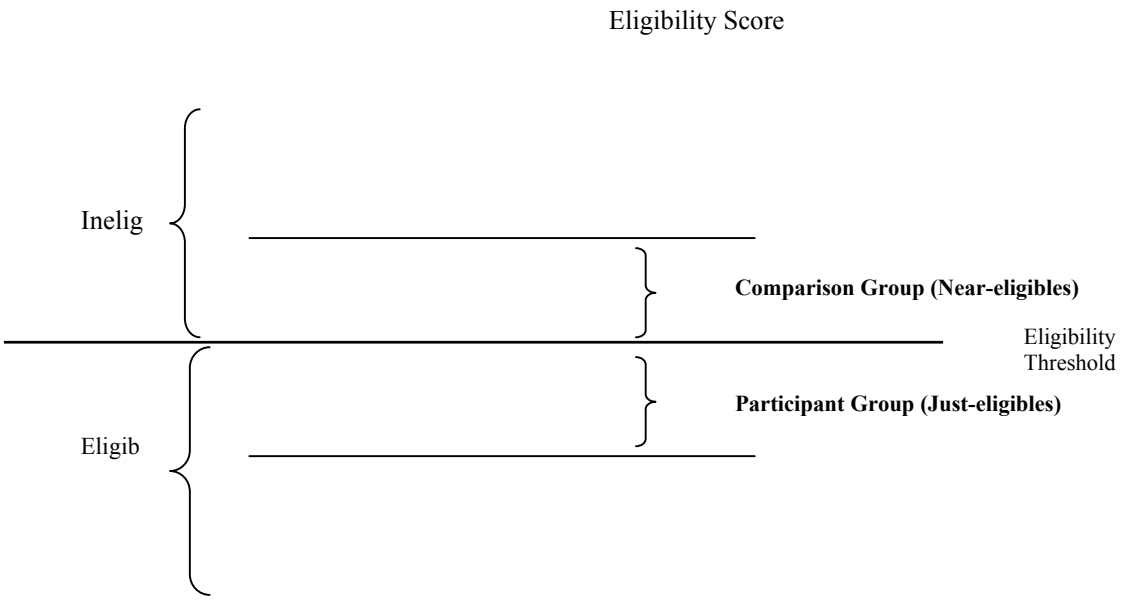
A. METHODOLOGY FOR IMPACT ANALYSIS

The objective of the impact analysis was to determine PATH's impact by comparing the outcomes *actually observed among participants* with the outcomes that we *would have observed for those participants in the absence of PATH.* The latter situation is called the "counterfactual," and it is one of the keys to ensuring that observed changes can be reasonably attributed to the programme and not to other factors. Unfortunately, the counterfactual can never be directly observed, since once the participants have been involved in a programme, it is not possible to observe what their circumstances would have been had the programme not occurred. Therefore, the best alternative to the ideal estimation of impacts is to identify a *comparison group* of households that are not participants but whose behavior can reasonably be assumed to mimic the behavior that would have been observed among the actual participants in the absence of the programme.

Our approach to doing this was based on the PATH eligibility score as discussed in Chapter I. In particular, we used a quasi-experimental design in which we compared households just below the programme eligibility threshold, (just-eligibles) with households *just above* the eligibility threshold (near-eligibles). The former group served as the participant group and the latter as the comparison group (Figure V.1). Drawing on an estimation technique known as *regression discontinuity,* we then used multivariate regression techniques to statistically account for the difference in "eligibility score" between the two groups. Box V.1 summarizes the quasi-experimental design.

FIGURE V.1

PARTICIPANT AND COMPARISON GROUPS



The rationale for the design is that the two groups are likely to be very similar to each other in terms of the criteria used to determine PATH eligibility (that is, the programme selection rules). Both groups exhibit similar eligibility scores (based on observable characteristics), although the scores are lower for the participant group because lower scores imply higher eligibility. Furthermore, because both groups made the effort to apply to PATH, they are likely on average to have had a similar level of motivation, and a similar willingness to apply for social assistance. So the only critical difference between the two for our purposes is that one group is offered the opportunity to participate in the programme, and the other does not. For these reasons, the outcomes of near-eligibles provide a reasonable counterfactual for the eligibles with very similar scores.

BOX V.1

SUMMARY OF EVALUATION DESIGN USED TO ESTIMATE PATH'S IMPACTS

- Exploits the use of *scoring formula*
  - PATH applicants fill in an *application form* with information on socio-economic and demographic characteristics
  - Information is entered into a computer which calculates an *eligibility score*
    - If score<=threshold, applicant is *eligible*
    - If score> threshold, applicant is *not eligible*
- Impacts are estimated by comparing:
  - Households just *below the threshold* (participant group) with households just *above it* (comparison group),
  - and statistically account for the eligibility score

Because the main differences in the characteristics of the two groups are embodied in their programme eligibility scores, which are available for the analysis, using the score as a variable in the statistical analysis provides an effective way to statistically control for any remaining differences. This design is being used more and more to estimate causal effects (Angrist and Lavy 1999; Todd and Vander Klaauw 1999; Ludwig and Miller 2005; Jacob and Legfren 2004). Furthermore, under certain technical assumptions,[16] which are very plausible in this context given that the scoring formula is used to select programme participants, the technique produces unbiased estimates of the causal effect of a programme.

Ideally, we would have liked to select the participant and comparison groups based solely on the proximity of their eligibility scores to the eligibility threshold. However, practical

[16] The key assumptions in this context are that there is a smooth continuous monotonic relationship between the score and the outcomes, and that scores could not be manipulated (i.e. households did not selectively change their behavior to get a score just below the cutoff).

considerations made this impossible.[17] In particular, because collecting baseline data was an important feature of the design (mainly for reasons related to ensuring adequate statistical power), the participant group was selected only from households who had not enrolled in the programme at the time of baseline data collection. This decision implied that the participant group households tended to have applied to the programme later than comparison group households. In this way, we were able to collect baseline information on participant group households before they received benefits from the programme. As a result, while the comparison group is fairly similar to the participant group in terms of characteristics that determined the eligibility score, it is much less similar in terms of the application date.

Finally, given sample size constraints for collecting household survey data, we decided to increase the statistical power of the analysis involving children by focusing the impact study on households with children. These households are probably the ones most likely to be affected by PATH.

B. SAMPLING, DATA COLLECTION, AND BASELINE CHARACTERISTICS FOR THE IMPACT ANALYSIS SAMPLES

1. Sampling and Data Collection

Just before the start of the baseline data collection (see below), MPR used MIS data from GOJ to identify the participant and comparison samples for the household survey. The participant sample consisted of 2,500 households who:

- Applied for the programme after April 2002
- Had scores at the cutoff value (1,035) or below
- Had children

[17] For a detailed description of the rationale for this, see "Evaluation of Jamaica's PATH Programme: Methodology Report" Mathematica Policy Research, September 2003.

Similarly the comparison households consisted of 2,500 households who:

- Applied for the programme after April 2002
- Had scores above the cutoff value (1,035)
- Had children

The survey data were collected by the Statistical Institute of Jamaica (STATIN). Baseline data were largely collected from January 2004 through March 2004.[18] Follow-up data were collected from the same households mostly from July 2005 through September 2005. STATIN was careful to interview the participant and the comparison groups at approximately the same rates over time in order to avoid any "timing" biases that could have occurred had one group been interviewed before the other.

The response rate for the baseline survey, 91 percent, was similar for the two groups. The response rate for the follow-up survey was about 82 percent, also similar for the two groups. These rates are well within the ranges that are generally obtained in similar survey work, and they thus lend strength to the analysis. A "nonresponse analysis," reported in Appendix 1, suggests that both groups of respondents are fairly representative of their respective survey samples. While respondents and nonrespondents exhibit statistically significant differences on some characteristics, the magnitude of these differences tends to be fairly small, and they are unlikely to affect the overall impact findings.

Data on the outcomes examined in the impact analysis come from the follow-up survey. One possible methodological concern is that participant group households had an incentive to over-report children's attendance at school and health centres because of a perceived threat that STATIN could report back to the MLSS and enforce sanctions for noncompliance with PATH.

[18] Interviews for the last two percent of respondents went slightly longer, until June 2004.

Appendix 3 explores this hypothesis. In our judgment, it is unlikely that this effect is large enough to substantially affect the findings.

2. Baseline Characteristics of Sample Members

In terms of demographic characteristics, the participant and comparison groups are fairly similar, although as expected, the participant group appears somewhat more disadvantaged than the comparison group in terms of socioeconomic characteristics. Tables V.1 and V.2 compare the two groups on some key demographic and socioeconomic characteristics).

TABLE V.1

BACKGROUND CHARACTERISTICS OF PARTICIPANT AND COMPARISON GROUPS
(Demographic Characteristics)

Characteristics	Participant Group	Comparison Group
Number of Household Members (%)		
1	—	—
2–3	20	27
4–6	66	62
7 or more	13	11
Age (%)		
< 3 years	9	9
3–5 years	11	10
6–16 years	39	38
17–59 years	35	37
60 years or older	6	6
Location (%)		
Kingston	14	7
Other town	15	18
Rural	70	75
Sample Size (number of households)	**2,500**	**2,500**

Source: Programme application data from PATH's MIS

While most participants are poor or close to being poor, a small fraction of PATH participants seem to be relatively well off. About 6 percent of households fall into the top two quintiles of the consumption distribution (Table IV.1). About 17 percent of PATH participants

have consumption levels greater than 1.5 times the poverty line, and about 6 percent have levels greater than twice the poverty line (Table IV.2).

3. Characteristics Related to PATH

As expected from the discussion of the evaluation design and sampling plan, there are larger differences between the participant and comparison groups in terms of characteristics related to PATH than in the characteristics just discussed. As specified in the design, the threshold scores on the PATH eligibility indicator are different, with the participant scores being lower. All comparison group scores are clustered within 2 points of the cutoff level. The distribution of participants, somewhat broader because of the need to obtain a large enough sample, ranges from just below the threshold to about 18 points below, with a median between 5 and 15 points below the cutoff level.[19]

Ideally, all participant group households should have enrolled in PATH and all members of the comparison group should not have enrolled. However, as is usually the case with evaluations of voluntary programs, this did not fully happen with PATH. The programme take-up rate for participants was 76 percent by October 2004 (i.e., about six months after the baseline survey was done and about nine months before the follow-up survey was launched). About 10 percent of the comparison group had enrolled in PATH by the time the follow-up survey was conducted. These rates are certainly in the range of typical rates for evaluations of this kind. Section D explores the implications of these rates for our impact estimations.

[19] This difference in the length of the score range between the participant and comparison households was produced by the decision to limit the sample to participants who had not yet received PATH Benefits. For more details, see "Evaluation of Jamaica's PATH Programme: Methodology Report" Mathematica Policy Research, September 2003. In Appendix 5, we do sensitivity analysis and find that the impact estimates are robust to changes in the range of scores for the participant group.

TABLE V.2

BACKGROUND CHARACTERISTICS OF PARTICIPANT AND COMPARISON GROUPS
Socioeconomic Characteristics (percent, otherwise noted)

Characteristics	Participant Group	Comparison Group
Ownership Status of Home		
Own	46	49
Source of Light		
Electricity	76	68
Kerosene	22	31
Sewer Type		
Pit	76	80
Water Source		
Indoor tap/pipe	6	6
Outside private pipe	30	28
Public standpipe	31	30
Other	34	36
Telephone		
Yes	9	12
No	52	55
Cellular	39	33
Toilet Facilities		
Exclusive use	74	79
Shared	26	21
Main Material on the Outer Wall		
Wood	54	46
Block and steel	32	40
Other	14	13
Weekly Spending (J$)		
Average	1,613	1,655
Median	1,500	1,500
Household Items		
Gas stove(s)	62	58
Electric stove(s)	0	0
Refrigerator(s) or freezer(s)	33	36
Fan(s)	24	24
Stereo equipment	14	18
Video equipment	8	8
Washing machine(s)	0	0
Television(s)	61	57
Motor bike(s)	0	1
Car(s) and/or other vehicle(s)	0	1
Sample size (number of households)	**2,500**	**2,500**

TABLE V.3

BACKGROUND CHARACTERISTICS OF PARTICIPANT AND COMPARISON GROUPS
(Programme-Related Characteristics)

Characteristics	Participant Group	Comparison Group
Eligibility Score		
Average	1,027	1,036
Minimum	1,017	1,035
Maximum	1,035	1,037
Distribution (%)		
<= 1,010	0	0
1,010 –<= 1,020	13	0
1,020 –< =1,030	55	0
1,030 –< =1,035	32	0
>1035	0	100
PATH Application Date (%)		
April 2002–June 2002	63	85
July 2002–September 2002	6	10
October 2002–December 2002	7	1
After December 2002	24	3
Enrolled in PATH		
By October 2004	76	7
By Time of Follow-Up Survey	82	10
Sample Size (number of households)	**2,500**	**2,500**

Source: Programme application data from PATH's MIS, Baseline Survey (2004), and Follow-up Survey (2005)

4. Values of Key Outcome Variables

While the participant and comparison groups differ somewhat in the socioeconomic and demographic characteristics that determine eligibility for PATH, they are fairly similar in terms of school enrollment and attendance, and frequency of visits to health centres. School enrollment rates are very high (around 96 percent) among children aged 0-17 in both groups. The two groups tended to both send their children to school (Table V.4) and take them to health centres for preventive care with approximately the same frequency. Moreover, they tended to give similar reasons for not sending their children to school, and to have similar patterns regarding which health centres they visited.

TABLE V.4

BASELINE OUTCOMES OF PARTICIPANT AND COMPARISON GROUPS
(Percent unless otherwise noted)

Characteristics	Participant Group	Comparison Group
Education[a]		
Enrolled in school	97	96
In a typical 4-week period, how many days was child sent to school?[b]		
Mean (days)	18	18
Median (days)	20	20
Attending 0-14 days	10	10
Attending 15-16 days	16	16
Attending 17-20 days	73	74
During the reference period, how many days was child sent to school?[b]		
Mean (days)	18	18
Median (days)	20	20
Attending 0-14 days	7	7
Attending 15-16 days	16	16
Attending 17-20 days	77	77
Health		
Visited health practitioner last 12 months		
Children 0-6 years old	70	71
Elderly 60 and over	75	77
Visited health practitioner last 12 months for check-up		
Children 0-6 years old	57	59
Elderly 60 and over	51	58
If currently pregnant, visiting health practitioner for antenatal care?	73	81
Sample Size (number of individuals)	**12,175**	**11,886**

Source: PATH 2004 Survey.

Note: The reference period is November 3-28, 2003.

[a]Among children 7-17 years old.
[b]For those enrolled in school.

Two questions in the baseline survey were related to attendance at school. One question asked about attendance in a typical four-week period, and the other asked about attendance in a specific reference period (November 3-28, 2003). In answering these questions, respondents in both groups reported that their children enrolled in school attended school an average of 18 days. About three in four children attended school for 17 to 20 days (which would meet PATH's attendance requirement of 85 percent of school days).

Both groups also visited health centres with about the same frequency in the 12 months prior to the interview. About 70 percent of children 0-6 years old visited a health centre in the past 12 months. The rate was slightly higher for the elderly (about 75 percent). In terms of preventive care, children 0-6 years old were slightly more likely than the elderly to visit a practitioner for preventive visits. Overall, there were no significant differences in the rates of health visits between the participant and comparison groups, except that the elderly in the participant group were less likely to conduct preventive health visits than their counterparts in the comparison group.

5. Conclusion

The preceding findings suggest that the participant and comparison groups are fairly similar to each other on a host of demographic, socio-economic, and programme-related characteristics. More important, given the use of the scoring formula as the exclusive determinant of eligibility for the programme, the eligibility score should account for any differences between the two groups. The next section explains how we statistically accounted for this score in estimating programme impacts.

C. STATISTICAL METHODOLOGY

This section is technically more complex than most of the rest of the report, but we have included it in order to allow readers with a technical background to fully assess the statistical

work. However, readers who are interested mainly in the findings as they relate to policy can skip this section and proceed directly to Section D.

1. Regression Specifications

To assess the robustness of our findings, we have estimated a number of different equations for each of the outcome variables of interest; the equations are based on multiple regression specifications. In each specification, the dependent variable reflects the outcome of interest, such as days of school attendance or use of preventive health care. Each specification also includes the policy variable of interest—i.e., whether the observation is connected with the participant or the comparison group. The multiple regression specifications vary, however, as to the variables that are included as independent regressors in the equations. The following types of variables are used in some or all of the equations.

- The ***eligibility score*** is a critical variable in the regressions because it controls for the factor on which the participants are chosen differently from the comparisons—namely, the score itself. Furthermore, by its construction, the score variable embodies many of the background variables used in computing it. More formally, this variable is important because the recently evolving literature on regression discontinuity modeling has shown that under certain assumptions, the use of this variable allows an unbiased estimate of the policy variable.[20]
- The ***outcome variable at baseline***, which is used in several of the regressions, is a lagged variable that is useful because it can control a substantial amount of the variance in the corresponding final outcome variable since it embodies the household's (or person's) "position" on this variable before being affected by PATH. Hence, the presence of this variable can improve the statistical precision of the impact estimates. A related interpretation is that the use of the lagged outcome variable is essentially a way to specify the regression equation to focus on *differences* between before and after PATH, rather than on absolute values in the period when PATH was set up.[21]

[20] For example, the U.S. Department of Education's What Works Clearinghouse stipulates that studies based on a regression discontinuity design provide "strong evidence" for an intervention's effectiveness (What Works Clearinghouse, September 2006).

[21] Technically, use of the pre-PATH variable is a *generalization* of a differences-in-differences approach, where the coefficient on the pre-PATH outcome is not constrained to be "1."

- A series of ***binary variables indicating application dates*** is also used to control for the differences between participants and comparison group members on this variable arising from logistical constraints in the design.
- ***Household demographic and socio-economic variables*** are used to control variance resulting from different household characteristics. Under the regression discontinuity design described above, this set of variables is not strictly necessary to obtain unbiased estimates, but the variables can increase the statistical precision of the estimates by controlling for variance resulting from background factors. If included, they also reduce potential analytical risks associated with an overly heavy reliance on the formal regression discontinuity modeling structure.[22]

As summarized in Section 2 below, our estimation work included estimating representative equations that embodied various combinations of the above variables. The rationale for this approach was that there is no single clearly "right" specification, and the estimation of multiple alternative specifications would provide information on the potential range of parameter values and, relatedly, would allow us to at least partly assess the degree to which the findings were sensitive to methodological issues.

In addition to the specification tests reported in the body of this report, we conducted several others, which involved both the quality of the data available for linking pre- and post-PATH observations and also the impacts of differences between participant and comparison group observations in the date of application. By and large, we have concluded that these issues do not substantially affect the reported results; a discussion of the tests appears in Appendix 5.

Another methodological issue that we have addressed empirically is the *functional form* of the estimated regressions. There is relatively little theory that would inform choices regarding

[22] These variables are the following: household size, whether household head is single, whether household head is female, age of head of household, ownership status of home, geographical area of residence, material used to build the home, source of light, source of electricity, source of water, sewage access, private toilette access, phone access, ownership of various assets (gas stove, electric stove, refrigerator, fans, stereo equipment, video equipment, washing machine, television, motor bike, and car), participation in other social programs, number of infants in the household, number of children in the household, number of teenagers in the household, number of adults in the household, number of elderly in the household, number of disabled in the household, weekly spending, and number of rooms in the household.

functional form in the regressions we have run. As a result, we tried the estimated equations with several different nonlinear forms to incorporate the score variable, including linear, quadratic, and cubic functions.

We performed several types of methodological analyses to test the equation specifications. The first component of this analysis involved inspecting the equations that were estimated on the basis of the *post-PATH* data. Among the factors considered in this work were such variables as "goodness of fit," the variance (or t tests) on key estimated coefficients, and the size of the coefficients themselves on variables considered "marginal," such as higher-order polynomials. Second, we conducted "placebo tests" by running alternative regressions using the *baseline* version of the outcomes as the dependent variable. The rational for this approach was that no effect of the participation variable should be expected in analyzing data obtained before participants were involved in PATH. Therefore any "effect" seen in these baseline data could be suggesting the existence of a methodological problem, either in the equation specification or some other factor. Findings from the first type of analysis mentioned above are discussed in subsection 2, where the illustrative regression results are presented. The results of the "placebo tests," which generally support the main analysis, are presented in Appendix 4.

2. Illustrative Regression Estimates

The results of four illustrative regression equation specifications used to estimate PATH impacts on school attendance are summarized in Tables V.5 and V.6. As noted, two survey variables on school attendance were available for the analysis—one reflecting attendance during a specific 20-school-day *reference* period just prior to the follow-up survey (Table V.5), and one reflecting attendance in a *typical* 20-day period (Table V.6).

TABLE V.5

IMPACT ESTIMATES FOR ATTENDANCE TO SCHOOL IN REFERENCE PERIOD
Dependent Variable: Number of days attended in Reference Period

	Specification			
	(1)	(2)	(3)	(4)
Participant group	0.483***	0.385*	0.443*	0.506**
	(0.135)	(0.223)	(0.226)	(0.218)
Eligibility score		-0.010	-0.008	-0.001
		(0.017)	(0.017)	(0.017)
Baseline school attendance in reference period			0.424***	0.406***
			(0.023)	(0.024)
Controls for household characteristics	No	No	No	Yes
Number of observations	**7,751**	**7,751**	**6,850**	**6,819**

Standard errors in parenthesis
Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.
*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level

TABLE V.6

IMPACT ESTIMATES FOR ATTENDANCE TO SCHOOL IN TYPICAL PERIOD
Dependent Variable: Number of days attended in Typical Period

	Specification			
	(1)	(2)	(3)	(4)
Participant group	0.586***	0.614***	0.527***	0.552***
	(0.141)	(0.206)	(0.207)	(0.212)
Eligibility score		0.003	-0.002	0.003
		(0.016)	(0.017)	(0.018)
Baseline school attendance in typical period			0.405***	0.389***
			(0.028)	(0.027)
Controls for household characteristics	No	No	No	Yes
Number of observations	**7,745**	**7,745**	**6,821**	**6,790**

Standard errors in parenthesis.
Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.
*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level

We begin our discussion of the regressions with the information in Table V.5. The key coefficient to focus on is the one on participant group, as it represents the impact estimates of PATH. The four numbered columns reflect different regression specifications, consistent with the alternatives identified in Section 1 above. The regression in column (1) includes only the binary variable that indicates whether the observation is in the participant group. The first entry in this column is the estimated coefficient for that variable, while the number directly below the coefficient is the estimated standard error of this coefficient. For instance, the estimated coefficient on the participant group variable is 0.483 with a standard deviation of 0.135. This estimated effect of PATH is thus highly statistically significant.

The second regression in the table (column 2) differs from the first in that the "score" on the eligibility formula is also included. In this regression, the coefficient on the participant group variable is slightly lower but still statistically significant; however, the score variable is not statistically significant. Columns 3 and 4 introduce an indicator of the household's baseline value for the outcome and then a set of household characteristics variables, respectively. None of these variables alters the statistical significance or basic magnitude of the impact estimate. As might be expected, the coefficient on the baseline indicator is also statistically significant.

Table V.6, in the same format as Table V.5, provides similar data on the regressions run by using the "typical" attendance variable. From an analytical point of view, the results are very similar to those in Table V.5. To be sure, the estimated coefficient on the participant group variable is slightly higher. But the overall patterns of coefficients and significance levels are very similar.

The policy implications of these findings are discussed in Section C below. From a methodological point of view, several aspects of these tables are noteworthy:

- The estimated coefficients on the participation variable are consistently positive and statistically significant. This provides strong evidence that PATH had a positive effect on school attendance, as intended. (The magnitude of the effect is discussed in the next section.)
- The "score" variable is not statistically significant, suggesting a very weak relationship, if any at all, between the eligibility rules and the education outcomes PATH is intended to affect.[23] This is confirmed in Table V.3, which shows that both the participant and the comparison group had similar baseline outcomes.
- The consistency of the results across regression specifications suggests that the findings are relatively robust to the exact regression specification employed.
- The consistency of the results across the two outcome variables supports the notion that both outcome variables are a reasonable measure of the outcome of interest. (This issue is discussed further in Appendix 3, where we discuss the validity of the education outcome measures.)

With regard to the policy implications of the results, we have focused the discussion on the estimated coefficients presented in the fourth column of Tables V.5 and V.6. However, the substantive policy-related findings in Section D below would not be substantially different if a different specification were used as their focus.

D. INTERPRETATION AND POLICY-RELEVANT ASPECTS OF THE FINDINGS

As explained in this section, the regression results shed light on the estimated effects of PATH on the key education variables of interest.

1. Discussion of Key Education Impacts

The key education findings are summarized in Table V.7. The first entry shows the average value of attendance per month for the participant group in the post-PATH period; that value is based on the question about days of schooling in a specific reference period. The average number of days of attendance is 17.11. The second number reflects the corresponding value for

[23] Even though the coefficient on score is not statistically significant, it is nevertheless important to statistically account for it in our regressions because it will rightfully reflect the lower precision on the participant group

the comparison group *after we used the regression results to adjust the characteristics of this group to correspond to those of the participant group.* The third entry, 0.51 days, reflects the estimated impact, which is the difference between the first two entries.

TABLE V.7

IMPACT ESTIMATES ON SCHOOL ATTENDANCE
Summary Table

	Participant Group	Comparison Group	Impact Estimate
Attendance to school in specific 20-day ***reference*** period (number of days)	17.11	16.60	0.51**
Attendance to school in specific 20-day ***typical*** period (number of days)	18.03	17.48	0.55**

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level

The second row of the table reflects the results based on the question about "typical" school attendance. This impact is slightly higher, at 0.55 days. While each of the estimated effects is statistically significant, the difference between them (i.e., between 0.51 and 0.55) is not.

2. Interpreting the School Attendance Estimates

a. Clustering of Observations at the Eligibility Cutoff

In interpreting the estimated effects on attendance, it is important to recognize that, given our design, the estimates in Table V.7 should be viewed as reflecting the impacts of PATH for a household *near the eligibility cutoff for the programme*. Essentially, the estimates reflect the expected rise in school attendance from enrolling in PATH *for a household whose eligibility score was approximately at the cutoff le*vel. This interpretation reflects the fact that, by virtue of

(continued)
coefficient implied by the quasi-experimental design. This lower precision arises because of the strong correlation between the participant group indicator and the eligibility score.

the evaluation design, our basic analytical comparisons are between households just below the cutoff (the participant group) and those just above (the comparison group).[24]

These impact estimates are therefore helpful in assessing the likely consequences of expanding or cutting PATH at the margin (i.e., by changing the eligibility cutoff score). In particular, the estimates are of interest to policymakers who may be considering whether or not to expand PATH.

It is difficult at a formal level to generalize these findings to the households whose eligibility scores were considerably below the cutoff and thus tend to have lower scores than those near the cutoff margin. The extent to which the estimated impacts can be generalized to PATH participants with lower eligibility scores depends on our assumptions about where the programme is most likely to have a larger impact. On the one hand, it is at least arguable that low-scoring households would, if anything, exhibit larger effects, since they had relatively more to gain by the assistance provided by PATH. To the extent that lower-income households can be expected to respond *more* strongly than other households to the PATH incentives, our estimates are conservative (in the sense of erring in the direction of being small), because they do not directly account for this design issue. On the other hand, poorer households could have been less likely to respond to PATH's incentives because they did not have the resources to do so.

b. Possible Dilution of Estimated Effects As a Result of Noncompliant Households

Another issue that affects the interpretation of the school attendance estimates (Table V.7) has to do with the "dilution" effect caused by survey respondents who "cross over" from the participant group to the comparison group and vice versa. As noted in Section B.2, some

[24] Limiting the sample in this way reduces the extent to which we can generalize from the results, at least at a formal level. However, it is necessary to limit the sample because of a number of logistical constraints that resulted from the timing of the start of PATH relative to the timing of the evaluation. These issues are discussed in detail in the Methodology Report for the project. (Levy and Ohls 2003)

members of the participant group have not actually been participating in PATH, and some comparison group members have received PATH benefits. We have not removed these observations from the regression samples, because doing so is likely to bias the results. However, including these observations will tend to reduce the size of the estimated impacts. While analytical techniques are available in the literature for making these corrections, we have chosen not to use them, as another way of remaining relatively conservative in our impact estimates.

c. Relative Magnitude of Estimated Impacts

It is also useful to further examine the relative magnitude of the estimated impacts. As shown in Table V.7, we estimated an effect for 0.51 days of attendance on a base of 17.11 days in the absence of PATH. Therefore, the *percent* increase is estimated to be approximately 3.07 percent. Basing the percentage on the "typical" attendance variable results in a similar, though slightly higher, estimate of the percent increase—3.15 percent.

At first glance, these changes may seem quite modest. But it is important to note that school attendance when PATH was implemented was already quite high, with the average child attending about 17 out of a 20-day school period, which means they were attending at least 85 percent of the time. Given that there are days when children are sick or have other legitimate reasons not to be in school, it appears that there was little room for change in this measure. Another noteworthy factor is that our estimated effects are point estimates and have a margin for error. While they are highly statistically significant, there is still considerable sampling error around them.

Another way to think about the issues having to do with the magnitude of the effects is to examine the findings in relation to findings from evaluations of similar programs in other countries, as discussed in Chapter VII.

3. Subgroup Estimates of the Key Education Outcomes

Table V.8 provides evidence on whether various subgroups of the population are affected in different ways by PATH. Entries in the table are the estimated impacts of PATH expressed in days of attendance—they are defined in the same way as the impacts in the third column of the previous table are defined. As shown in the Table V.8, estimated impacts are very similar for boys and girls, and for different age groups, although the impacts tend to be somewhat weaker in the age 10- to 12-group.[25] However, these differences are not statistically significant.

TABLE V.8

IMPACT ESTIMATES ON SCHOOL ATTENDANCE FOR SELECTED SUB-GROUPS
(Summary Table)

Subgroup	Attendance in Reference Period	Attendance in Typical Period
Boys	0.42	0.57
Girls	0.44	0.44
Kingston Metropolitan Area	1.81	2.58
Other urban	0.96	1.03
Rural	0.35	0.20
Age 6-9	0.59	0.63
Age 10-12	0.23	0.16
Age 13-17	0.59	0.84

Source: PATH 2005 Survey, PATH 2004 Survey, MIS data

The only substantial difference in impacts relates to the geographical area of residence. The residents of Kingston are estimated to have experienced noticeably larger effects than either residents of other cities or residents of rural areas. These differences in effects were statistically significant.

Some policymakers have speculated that the program may have a larger effect on older children (given that they tend to have lower enrollment and attendance rates). Table V.8 does not

[25] Given the policy concern with the relatively low rate of school attendance by teenage boys, we also explored whether boys aged 13-17 years old were more likely to be affected by PATH than girls 13-17 years old. While the sample sizes are fairly small to draw very definitive conclusions, we found no evidence that this was the case

seem to provide strong evidence in support of this assertion, as the magnitude of the impact estimates of children aged 13-17 tended to be higher than the corresponding magnitudes for other age groups but these differences were not statistically significant. We explored this hypothesis further by trying to more finely distinguish impacts within the 13-17 age group. We found no evidence suggesting that the impact of PATH on school attendance was larger for older children (Table V.9). It is important to keep in mind that this analysis involves fairly low sample sizes (and hence low statistical power).

TABLE V.9

IMPACT ESTIMATES ON SCHOOL ATTENDANCE FOR SELECTED SUB-GROUPS
(Summary Table)

Subgroup	Attendance in Reference Period	Attendance in Typical Period
Age 14-17	0.41	0.80
Age 15-17	0.26	0.57
All ages	0.45	0.55

Source: PATH 2005 Survey, PATH 2004 Survey, MIS data

We also found no evidence that the impacts of PATH were higher for children with low levels of attendance at baseline than for children with high levels of attendance. Children who were attending school fewer than 12 days in the 20-day reference period (which corresponds to the 10^{th} percentile) and those who were attending fewer than 16 days (which corresponds to the 25^{th} percentile) exhibited PATH impacts that were statistically indistinguishable from children who were attending more than 16 days in the 20-day reference period. Similar findings arose from the analogous analysis for school attendance in the 20-day typical period.

4. Additional Analysis of the Education Outcomes

Participants' responses about their interactions with both PATH and the schools are a potential source of insight into what underlies the observed effects of PATH as reported above. Here we present tabulations of a number of relevant variables.

a. Sanctions

It is likely that the incentive effects of PATH depend, in part, on how often sanctions were exercised in cases of noncompliance with PATH requirements (Table V.10). As indicated in the table, approximately 26 percent of the participant household children ages 7 to 17 in the sample were reported to have had their benefits reduced at some point while they were participating in PATH. According to the survey, the most common rationale for sanctions in the form of reduced benefits was failure to meet school attendance requirements. However, interestingly enough, a significant number of respondents said they did not know why their benefits were reduced. This could reflect confusion on the part of respondents, but it could also reflect administrative errors. The latter would be consistent with findings from the qualitative assessments (Chapter III) that some participants believed such errors sometimes occurred.

TABLE V.10

HAS PATH PAYMENT EVER BEEN REDUCED
(Households with Children 7-17 years old)

Payment Ever Been Reduced	
Yes	25.8
No	71.9
Can't recall/Did not respond	2.4
Reasons Why Payment Was Reduced[a]	
Household member did not meet school attendance requirement	37.9
Household member did not meet health attendance requirement	23.1
Change in household composition	2.6
Other	10.8
Don't know	30.4
Sample Size	**1,493**

Source: PATH 2005 Survey

[a]More than one answer was possible. Sample restricted to those who reported reduction in the previous question.

Overall, the findings on sanctions provide significant evidence that they were probably viewed by PATH participants as a credible threat. Similar data on sanctions for not meeting preventive health care requirements are presented in the next chapter.

b. Perceived Frequency of Attendance Relative to Previous Year

Survey respondents with children in school were asked about their perception of whether their children were attending school more or less frequently, or at about the same frequency than they had in the previous year (Table V.11).

TABLE V.11

FREQUENCY OF SCHOOL ATTENDANCE RELATIVE TO PREVIOUS YEAR
(Percent unless otherwise noted)

	Participant Group	Comparison Group
Frequency of attendance relative to previous year[a]		
Less frequently	5.2	12.6
About the same frequency	64.2	77.0
More frequent	30.6	10.4
Reasons for increase in school attendance[b]		
Change in health status	5.0	7.7
Change in economic conditions	21.7	42.3
Change in distance to nearest school	1.9	4.0
Trying to comply with PATH requirement	73.5	18.0
Other	5.1	24.9
Reasons for *decrease* in school attendance[c]		
Change in health status	14.6	9.0
Change in economic conditions	69.4	80.1
Change in distance to nearest school	3.5	2.9
Trying to comply with PATH requirement	2.0	0.0
Other	14.1	12.3
Sample Size	**3,788**	**3,589**

Source: PATH 2005 Survey

[a]Based on survey question "Relative to previous year, did child X attend school with..?" Possible answers indicated in above table.

[b]Restricted to those who answered "More frequent" in the previous question. Based on Survey question "The change in frequency of attending school was due to?" MARK ALL THAT APPLY. Possible answers indicated in above table.

[c]Restricted to those who answered "Less frequent" in the previous question. Based on Survey question "The change in frequency of attending school was due to?" MARK ALL THAT APPLY. Possible answers indicated in above table.

Approximately 31 percent of respondents in the participant group indicated that their child was attending school more often, compared to 10 percent in the comparison group. This finding is consistent both with the findings on rising attendance rates reported earlier and with findings from the participant focus groups as reported in Chapter III.

As shown in the second panel of Table V.11, a very substantial majority of the participant group who reported an increase in attendance said that PATH was the reason.[26] The second most common reason given by that group was "change in economic conditions" (22 percent). That response was also the most common reason given by comparison group members for an increase in their children's attendance. Those who indicated a drop in school attendance relative to the previous year attributed it to a change in economic conditions. This is true in both groups, but particularly in the comparison group.

c. Reasons for School Absences

Table V.12 summarizes the reasons given by respondents in the participant and in the comparison group for school absences. Respondents were first asked if their children were absent from school in the reference period, and if so, they were asked why. More than one answer was allowed.

By and large, the pattern of responses is quite similar for both groups. However, the participants were much less likely than the comparison group to cite "money problems" as a reason for absence (12 percent versus 22 percent) but were slightly more likely to cite illness. Comparison group members were relatively more likely to give such reasons as "truancy", "working outside the home," and similar responses, though the differences are quite small.

[26] Interestingly, some comparison group households also indicated that PATH had led to increased attendance by their children at school—though a far lower percentage than for participants. The comparison group answers may reflect the fact that, as noted earlier, a small number of comparison households ended up entering PATH, or it could reflect confusion about the question by some respondents.

TABLE V.12

REASONS FOR SCHOOL ABSENCE
(Percent unless otherwise noted)

	Participant Group	Comparison Group
Reason for Absence in Reference Period[a]		
Illness	5.28	4.66
Truancy	0.23	0.24
Working outside the home	0.02	0.02
Needed at home	0.09	0.05
Market day	0.02	0.04
Transport problem	0.21	0.49
School closed	1.61	1.58
Shoes/uniform missing/dirty/wet	0.30	0.36
Rain	1.74	2.06
Money problems	12.20	22.11
Had to run errand	0.03	0.02
Not safe at school	0.14	0.02
Not safe in community	0.21	0.07
Other	1.49	0.84
Missing	79.27	71.36
Has Child Ever Been Absent from School for These Reasons[b]		
Household could not provide lunch	81.81	89.52
School fees not paid	2.98	6.73
Household could not afford transportation costs	39.65	45.58
Household could not afford uniform/shoes	7.11	8.38
Other	19.20	11.78

[a]Up to two answers were allowed. Question immediately follows question "During the 4 week period May 2-27, how many days was X sent to school?" Exact wording of this question is: "What were the two main reasons for X's absence from school?" Sample sizes: 5,761 (participant group) and 5,496 (comparison group).

[a]Up to two answers were allowed. Question immediately follows question "During the 4 week period May 2-27 how many days was X sent to school?" Exact wording of this question is: "What were the two main reasons for X's absence from school?" Sample sizes: 5,761 (participant group) and 5,496 (comparison group).

Interestingly enough, in the baseline survey, which was administered about a year before the follow-up, the percentage of participant group respondents who connected absences with "money problems" was very similar to that of the comparison group. The fact that, under PATH, the percentage of participants giving this reason dropped substantially below the percentage of comparison group members giving this reason suggests that one of the ways in which PATH encouraged attendance was to provide the resources (such as transportation and lunch money) families needed to send their children to school. This finding is consistent with the responses in the second panel of Table V.12, which are based on a different question in the follow-up survey that specifically listed a set of possible reasons for attendance problems and, for each possible reason, asked if it was applicable to the household. In general, respondents in the participant group were less likely to cite reasons having to do with lack of money to pay school-related costs.

d. Effects on School Achievement

Overall, the findings on PATH's effect on school achievement appear to be similar in the treatment and in the comparison groups (Table V.13). Respondents were asked whether their child had graduated to the next highest education level at the end of the previous year and how their child's recent grades related to grades received in the previous year. Children in the participant group were slightly more likely than children in the comparison group to advance to the next grade (by about two percentage points), but this difference is small and not statistically significant. On the other hand, the participant group children were slightly more likely to report "much worse" grades, but again, this difference is small and not even close to being statistically significant.

We also used regression models to assess PATH's impacts on school advancement and grades. These models are analogous to the ones used to estimate PATH's impact on school

attendance and preventive health visits. In this case, we used linear probability models with the same explanatory variables as our original models (see Appendix 6 for details). The results are consistent with the tabular versions presented above; they show no statistically significant differences between the participant and comparison groups.

TABLE V.13

ACADEMIC ACHIEVEMENT
(Percent unless otherwise noted)

	Participant Group	Comparison Group
Did Child Advance to Next Grade?[a]		
Yes	87.5	85.2
No	2.2	2.9
Can't recall	1.3	1.2
Don't know yet: final grades have not been received	6.7	6.8
NA	2.3	3.9
Grades Relative to Previous Year		
Much worse	4.4	3.4
Worse	1.9	2.1
About the same	4.4	6.2
Better	29.6	31.5
Much better	37.8	36.8
Not applicable	18.3	15.4
Did not respond	4.4	3.4
Sample Size	**3,783**	**3,956**

[a]Based on the survey question "At the end of the past academic years (2004-2005), did [NAME] advance to next grade or graduate?" Possible answers indicated in above table.

In reviewing these findings, readers should note that school achievement was not one of the major, focal outcome variables specified when the analysis was planned. As a result, and given constraints on evaluation resources, the resources devoted to obtaining data on these outcomes were quite limited. It is therefore possible that a relatively lower level of statistical power in the analysis in this area is responsible for the apparent absence of effects for these variables. It could also be that impacts on academic performance may take some time to emerge. The participant group households had received PATH benefits for about a year (on average) by the time of the follow-up survey.

e. Child Employment

Another potential benefit of PATH is the possibility that by encouraging school attendance, the programme could reduce child labor. However, at least as reported by the children's parents, rates of child labor are extremely low in the sample, and there are no significant differences between the participant and the comparison group in this outcome (Table V.14). In each group, the reported incidence of child labor is less than two percent. Regression analysis of this outcome also revealed no statistically significant differences between the two groups (see Appendix 6 for details).

TABLE V.14

DOES CHILD WORK/ ACTIVITY TOWARDS HOUSEHOLD MAINTENANCE?
(Percent unless otherwise noted)

	Participant Group	Comparison Group
Yes	1.75	1.75
No	88.75	89.40
Not stated or missing	9.49	8.85
Sample Size	4,169	3,989

Note: Based on survey question "Does [NAME] carry out any kind of work or other activities that contribute towards the maintenance of the household or towards himself / herself?" Possible answers indicated in above table.

f. Summary of Analysis on Education Outcomes

For the most part, the evidence reviewed in Section 4 is highly consistent with the basic regression findings reviewed earlier. Sanctions appear to have been quite common in the sample, so they presumably provided a credible incentive for complying with PATH requirements. Also, parents reported that they believed their children were going to school more often, and members of the participant group tended to report that lack of money post-PATH was less a factor in causing their children to be absent from school.

There is no significant evidence, however, that other outcomes such as advancement to next grade, grades, and child employment have been affected by PATH. This implies that while the programme may have achieved its main education-related objective of increasing the frequency of school attendance, these increases may have not translated into improvements in deeper outcomes such as grades. As mentioned above, however, it is important to remember that effects on these variables may emerge only with more exposure to the programme.

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VI. PATH'S IMPACTS ON HEALTH-RELATED OUTCOMES

The basic approach to examining health-related outcomes in the evaluation is essentially the same as that described above for the education variables. As a result, our discussion in this area can be somewhat more concise, since the discussion of methodology in Chapter V applies here as well.

Below, we first present key regression results applicable to health outcomes (Section A). We then provide an interpretive discussion of the regression findings, as they relate to policy issues (Section B). Finally, we provide additional insights based on analysis of other health-related components of the survey data (Section C).

A. REGRESSION RESULTS

For the regressions of health outcomes, we have included essentially the same set of explanatory variables as were used with the education outcomes (see Section V.C). In addition, we have repeated the same examinations of multiple equation specifications as those used for the education variables.

Two age groups, children and the elderly, are included in our analysis of health care utilization. However, both the PATH requirements and the underlying determinants of health care utilization are different for these two groups. Therefore, the regression analysis of outcomes was conducted separately on the two samples defined by age. Table VI.1 below displays the results obtained in running the alternative regressions for observations on children age 0-6, while Table VI.2 presents analogous results for the four equations run on elderly households (defined as households age 60 or older). The dependent variable for the analysis is the number of visits to a health practitioner in the last six months for preventive reasons.

TABLE VI.1

IMPACT ESTIMATES ON PREVENTIVE HEALTH VISITS FOR CHILDREN 0-6 YEARS OLD
Dependent Variable: Number of visits to a health centre for preventive reasons in the past 6 months

	Specification			
	(1)	(2)	(3)	(4)
Participant group	0.170***	0.262***	0.307***	0.278***
	(0.048)	(0.075)	(0.080)	(0.085)
Eligibility score		0.010**	0.011*	0.009*
		(0.006)	(0.007)	(0.007)
Attendance at a health centre in 6 months prior to baseline survey			0.205***	0.199***
			(0.025)	(0.025)
Controls for household characteristics	No	No	No	Yes
Number of observations	**3,756**	**3,756**	**2,759**	**2,745**

Note: Regressions were run at the individual level. Huber-White standard errors (in parenthesis) were used to account for within-family correlations.

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level.

TABLE VI.2

IMPACT ESTIMATES ON PREVENTIVE HEALTH VISITS FOR THE ELDERLY
(Dependent variable: number of visits to a health centre for preventive reasons in the past 6 months)

	Specification			
	(1)	(2)	(3)	(4)
Participant group	0.048	0.002	-0.014	0.009
	(0.111)	(0.178)	(0.200)	(0.216)
Eligibility score		-0.005	-0.011	-0.011
		(0.014)	(0.017)	(0.020)
Attendance at a health centre in 6 months prior to baseline survey			0.271***	0.250***
			(0.059)	(0.058)
Controls for household characteristics	No	No	No	Yes
Number of observations	**1,379**	**1,379**	**1,112**	**1,107**

Note: Regressions were run at the individual level. Huber-White standard errors (in parenthesis) were used to account for within-family correlations.

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level.

The regression results for children (Table VI.1) are broadly consistent with the corresponding findings on school attendance. In particular, in all of the regression specifications, there are positive and statistically significant coefficients on the variable of policy interest, the participant group indicator. Similarly, the baseline version of the outcome enters the regressions as highly significant. In these regressions specifications, however, the eligibility score is statistically significant. These results suggest that PATH had a positive and statistically significant impact on preventive care health clinic visits by children in the programme.

However, the results for the elderly, presented in Table VI.2, show no statistically significant impacts of PATH. The estimated coefficients on the participant group variable for this group are small and are not consistent in sign across the different equation specifications. As with the education regressions, the eligibility score variable is statistically insignificant. As expected, the lagged value of the dependent variable remains significant.

Overall, then, our findings are very different for the two sets of regressions. The children's use of preventive health care has been affected by PATH, while there is no evidence that the elderly's has.

B. POLICY INTERPRETATION OF HEALTH CARE RESULTS

As with the education findings, it is important to translate the regression numbers into policy-relevant metrics. Following the pattern of the education analysis, we have chosen to focus our discussion on the estimated coefficients presented in the fourth column of Tables VI.1 and VI.2. However, the substantive policy-related findings would not be substantially different if a different specification were used as the focus.

1. Basic Policy-Related Findings

Table VI.3 summarizes the policy-relevant implications of the regression results reviewed above. For children age 0 to 6, the estimated usage of health care in the absence of PATH is

0.73 visits per six-month period. The impact of PATH is estimated to be 0.28 visits per six-month period, an impact of approximately 38 percent of the baseline value. The elderly have a higher average number of visits per six-month period. However, the estimated impact which is implicit in the regression findings is only one one-hundredth (0.01) of a visit, and it is not statistically significant.

TABLE VI.3

IMPACT ESTIMATES ON PREVENTIVE HEALTH VISITS
(Summary Table)

	Participant Group	Comparison Group	Impact Estimate
Number of visits to health centre for preventive reasons in past 6 months for ***children 0-6 years old***	1.01	0.73	0.28***
Number of visits to health centre for preventive reasons in past 6 months for ***elderly*** (60 or older)	1.20	1.19	0.01

*/**/*** Impact estimate statistically significant at the 10%/5%/1% significance level.
Elderly defined as 60 years or older

2. Subgroup Estimates

Table VI.4 provides evidence on whether various subgroups of the population are affected in different ways by PATH. Entries in the table are the estimated impacts of PATH in number of preventive visits, defined the same way as the impacts in the third column of Table VI.3 above. As shown in Table VI.4, estimated impacts are very similar for boys and girls, and are quite similar for different age groups, although there is some tendency for the age 0 to 1 group to have higher effects. However, because of the smaller sample sizes available for subgroup estimation, neither this difference nor the other differences within groups in the table are statistically significant.

TABLE VI.4

IMPACT ESTIMATES ON PREVENTIVE HEALTH VISITS FOR SELECTED SUBGROUPS
(Summary Table)

	Impact Estimate	
Subgroup	Children 0–6 years	Elderly
Boys	0.16	-0.18
Girls	0.45	-0.05
Kingston metropolitan area	-0.04	1.37
Other	-0.27	-0.68
Rural	0.29	-0.04
Age 0-1	0.84	NA
Age 2-6	0.28	NA

C. RELATED ANALYSIS OF SURVEY DATA

If we further examine the underlying factors associated with the above findings, we arrive at some additional insights of considerable interest.

1. Sanctions

As with the analysis of education outcomes, it is important to assess whether or not households had reason to view sanctions for not meeting the health care requirement as a credible threat. Relevant evidence on this issue is available in a tabulation presented in the previous chapter, in connection with the interpretation of the school attendance findings (see Table V.9).

While that earlier table indicated that the single largest reason for sanctions among families who had children 7-17 years old was failure to meet school attendance requirements (true for 38 percent of households with a payment reduction), it also indicates that substantial numbers of respondents, 23 percent of households with a payment reduction, reported having their payments reduced for failure to meet the *health visit* requirement. Furthermore, fewer households were at risk of the health sanction than were at risk of the education sanction. Among children, the health visit requirement applies only to age 0-6, while the school attendance requirement

extended to the much larger 6-17 age group. When the analysis is restricted to families with children 0-6 years old, 36 percent of households report failure to meet school attendance requirement and 28 percent report failure to meet health attendance requirement (see Table VI.5).[27] Finally, while the programme's initial design involved sanctions for elderly who did not meet the health requirement, this feature of the design was changed to eliminate sanctions for the elderly, which may help explain why there were no impacts of PATH for the elderly.

TABLE VI.5

HAS PATH PAYMENT EVER BEEN REDUCED?
Households with children 0-6 years old

	Percent
Payment Ever Been Reduced	
Yes	20.0
No	76.5
Can't recall/Did not respond	3.5
Reasons why payment was reduced[a]	
Household member did not meet school attendance requirement	35.9
Household member did not meet health attendance requirement	28.0
Change in household composition	2.8
Other	11.3
Don't know	27.6
Sample Size	1,121

Source: PATH 2005 Survey

[a]More than one answer was possible. Sample restricted to those who reported reduction in the previous question.

Together, these observations suggest that sanctioning because of failure to meet the health visit requirement was quite common among children, and we conclude that the threat of being sanctioned was credible and may have had considerable incentive effects.

[27] It is important to note that the questions related to payment reduction were asked at the household level, and some households have children in both age groups (0-6 and 7-17).

2. Services Received

It is also of interest to assess whether the health centre visits that took place were largely for preventive services, as was intended by the reform initiative. Table VI.6 below provides information on which services children in PATH households received when they went to a health centre, as well as similar information for the comparison group. It suggests that the participant group did indeed receive services that are generally regarded as constituting preventive care. Sixty percent reported receiving immunizations while 49 percent received health-related advice. Eighteen percent reported height measurement, and 31 percent weight measurement. Only about 2 percent received blood or urine tests, which are more likely to be diagnostic than preventive.

TABLE VI.6

SERVICES RECEIVED DURING PREVENTIVE CARE VISITS
(percent unless otherwise noted)

CHILDREN 0-6 ONLY

	Participant Group	Comparison Group
Immunization	60.23	66.14
Blood Test	1.43	2.81
Urine Test	0.89	1.91
Height Measurement	18.48	19.46
Weight Measurement	31.34	33.41
Health-Related Advice	49.46	44.21
Other	8.13	7.20
Sample Size	1,120	889

Based on survey question: "What service(s) did [NAME] receive during the visits for preventive care?" ENTER ALL THAT APPLY. Possible answers indicated in above table.

In general, these patterns are roughly similar for the comparison group. However, the percentage reporting that they got health-related advice is slightly lower, and the percentage getting immunizations was somewhat higher.

The corresponding patterns for the elderly are quite different (Table VI.7). Not surprisingly, these individuals were much less likely to receive immunizations, and the incidence of diagnostic

blood or urine tests was higher. The elderly were also somewhat more likely to receive health-related advice. But the differences between the participant and comparison groups were not statistically significant.

TABLE VI.7

SERVICES RECEIVED DURING PREVENTIVE CARE VISITS
(percent unless otherwise noted)

ELDERLY ONLY

	Participant Group	Comparison Group
Immunization	1.50	1.57
Blood Test	30.33	33.96
Urine Test	35.14	34.59
Height Measurement	7.21	4.72
Weight Measurement	19.52	20.75
Health-Related Advice	63.06	61.01
Other	22.82	26.42
Sample Size	**333**	**318**

Based on survey question: "What service(s) did [NAME] receive during the visits for preventive care?" ENTER ALL THAT APPLY. Possible answers indicated in above table.

3. Reasons Preventive Care Visits Were Not Made

Respondents were asked whether preventive care visits had been made by household members in the previous six months. If the answer was "no," they were asked why not. Table VI.8 summarizes the responses as they apply to children. Two answers predominate: the household did not think the visit was needed (51 percent of the participant group), or they didn't have the required money (33 percent), meaning either money for the visit itself or for associated costs like transportation. In general, the response pattern is very similar for the participant and the comparison groups, although parents of children in the participant group are slightly less likely than their counterparts in the comparison group to report as a reason for not making preventive visits that they don't consider them necessary. Similar patterns are observed for the elderly (Table VI.9).

TABLE VI.8

REASONS PREVENTIVE CARE VISITS WERE NOT MADE
(Percent unless otherwise noted)

CHILDREN 0-6 ONLY

	Participant Group	Comparison Group
Don't think it's necessary	51.23	53.33
Don't have a place to go	0.39	0.00
Health centre is too far from where I live	1.23	0.00
Don't have money	33.20	34.12
I am not well treated	2.87	0.78
I have to wait too long to get care	2.05	2.35
I don't have time	1.23	5.49
I don't trust the physicians/nurse	0.00	0.00
I went but didn't receive care	0.82	0.39
Other	5.74	5.49
Don't know/ Did not respond	6.56	8.63
Sample size	244	255

Based on survey question: "Why did you not make any visits for preventive care in the past 6 months?" ENTER ALL THAT APPLY. Possible answers indicated in above table.

TABLE VI.9

REASONS PREVENTIVE CARE VISITS WERE NOT MADE
(Percent unless otherwise noted)

ELDERLY ONLY

	Participant Group	Comparison Group
Don't think it's necessary	55.42	53.29
Don't have a place to go	0.66	0.00
Health centre is too far from where I live	1.81	2.63
Don't have money	29.52	31.58
I am not well treated	0.60	1.32
I have to wait too long to get care	5.42	4.61
I don't have time	3.01	1.32
I don't trust the physicians/nurse	0.66	0.00
I went but didn't receive care	0.60	0.00
Other	1.81	4.61
Don't know/ Did not respond	10.84	9.21
Sample size	166	152

Based on survey question: "Why did you not make any visits for preventive care in the past 6 months?" ENTER ALL THAT APPLY. Possible answers indicated in above table.

4. Health Care Visits Relative to Previous Years

Table VI.10 summarizes respondent perceptions about how the frequency of preventive care visits had changed relative to previous years. There is a slight tendency for the participant group to be more likely to report an increase in usage as compared to the comparison group (8 percent versus 5 percent), but the difference is quite small. Given the earlier regression-based finding that PATH had a positive impact on the number of preventive visits by children, it is not clear why this does not reveal itself more clearly in reported perceptions.

TABLE VI.10

FREQUENCY OF PREVENTIVE CARE VISITS RELATIVE TO PREVIOUS YEARS
(Percent unless otherwise noted)

CHILDREN 0-6 ONLY

	Participant Group	Comparison Group
Less Frequency	15.78	17.50
About the Same Frequency	69.53	69.04
More Frequency	8.34	5.40
Missing	6.36	8.07
Sample Size	2,015	1,909

Source: PATH 2005 Survey

Based on survey question "Relative to previous years, has person X attended health centre for preventive reasons with...?" Possible answers indicated in above table.

We also explored this issue using the same regression models we used to explore PATH's impacts on school attendance and preventive health visits. In this case, we used a linear probability model in which the dependent variable is whether a household reported more frequent visits this year than in previous year. The results also show no statistically significant differences between the participant and comparison groups. See Appendix 6 for details.

Participants who said that their children's usage of health care *had* gone up were asked the reason for the increase. Forty-six percent (not shown) indicated that the increase was due to the PATH requirement. Most of the rest indicated a change in health circumstances as the reason.

5. Immunization Rates

There is no evidence that PATH was associated with higher immunization rates. Children 0-5 years old in the participant group tend to have similar immunization rates than children in the comparison group (Table VI.11), though the immunization rates of both groups tended to be very high to begin with.

TABLE VI.11

IMMUNIZATION RECORDS (2005)
(Percent unless otherwise noted)

CHILDREN 0-5 YEARS OLD

	Participant Group	Comparison Group
O. P. V.	95.78	94.49
D. P. T.	98.46	97.87
B. C. G.	98.23	98.43
Measles	87.34	86.04
Sample Size	1,655	1,544

Based on Survey question: "Records immunization status of the child."

6. Reported Health Status

The data provide no evidence that PATH led to increases in actual health status of children 0-6 years old relative to previous years (Table VI.12). The differences between the participant and comparison groups in how health status had changed were all about one percentage point and not statistically significant. We again explored this issue using regression models to estimate impacts we have used elsewhere in the report, and find no evidence that PATH affected health status. See Appendix 6 for details.

TABLE VI.12

HEALTH STATUS RELATIVE TO LAST YEAR
(percent unless otherwise noted)

CHILDREN 0-6 ONLY

	Participant Group	Comparison Group
Much worse	0.79	0.84
Worse	3.52	3.77
About the same	70.42	69.57
Better	14.04	13.99
Much better	5.21	4.14
Missing	6.01	7.70
Sample Size	2,015	1,909

Based on survey question: "Relative to last year is [NAME's] current health?" Possible answers indicated in above table.

f. Summary of Analysis of Health Outcomes

Generally speaking, the findings of this section for children are broadly consistent with PATH's impacts on school attendance outcomes (see Section V.D, in particular subsection f). On the one hand, sanctions appear to have been quite common in the sample, so they presumably provided a credible incentive for complying with the PATH requirements. But on the other hand, participant group households do not seem to report significant increases in preventive visits that were different from those reported by comparison group households.

Finally, analogous to our education findings, there is no significant evidence that PATH has affected health care status. This implies that while the programme may have achieved its main health-related objective of increasing the frequency of preventive health visits for children 0-6 years old, these increases have not translated into improvements in deeper outcomes. Two important caveats to keep in mind are that influencing these variables may take longer than the amount of time that the participant group was exposed to the programme, and that our measure of health status was a very crude one (since this was not one of the main outcomes to be studied in the evaluation).

VII. COMPARISON WITH RESEARCH FINDINGS IN OTHER COUNTRIES

An additional perspective on the PATH findings can be gained by examining them in the light of findings from research on similar CCT programmes in other countries. To be sure, cross-country comparisons inevitably involve potential ambiguities, partly because of the effects of different national contexts, and partly because of limitations in the documentation for some programmes and differences in the programmes themselves. Differences in evaluation methods must also be taken into account. Nevertheless, despite these caveats, cross-country comparisons offer some insight into the PATH findings presented earlier in this report.

This chapter examines the PATH results in this broader, international context. It draws heavily on a very useful synthesis of the relevant research assembled by Dr. Norbert R. Schady of The World Bank and presented at a World Bank-sponsored conference held in Istanbul, Turkey, in June 2006. (We are indebted to Dr. Schady for making his work available to us in preparing this report.)

Three major sets of findings are examined below:

- Targeting;
- Impacts on school-related outcomes; and
- Impacts on health-related outcomes.

A. TARGETING

Summary data relating to targeting are available for four countries in addition to Jamaica. Data that support more-detailed comparisons are available for one country, Mexico. Both lines of comparison are discussed below.

1. Percentage of Benefits Going to Poorest Quintile

As noted in Chapter IV, a useful measure of benefit targeting success is the percentage of the benefits in a transfer programme which are received by the poorest 20 percent (poorest quintile) of the population. Because of various constraints in directing benefits precisely to those most in need, it would be unrealistic to expect that all of the benefits would accrue to this quintile. But, since the objective is to help the poor, it is reasonable for welfare systems to attempt to maximize this figure.

The analysis contained in Chapter IV indicates that for PATH, the targeting rate as measured in terms of the percentage of benefits accruing to the poorest quintile is 58 percent. Table VII.1 compares this figure to figures for four other countries (which are identified in the table by letters, since their names are not included in the material available to us).

TABLE VII.1

TARGETING: PATH RELATIVE TO OTHER COUNTRIES
(percent unless otherwise noted)

Programme	Percent of Benefits Going to Poorest Quintile of the Population
PATH	58
Comparison Countries	
A	62
B	58
C	40
D	36

Source: Schady (2006)

Note: Comparison countries are identified only by letters in this table because there were not identified by name in the source document.

One of the countries in the comparison achieves a percentage of benefits going to the bottom quintile which is slightly higher than that for PATH—approximately 62 percent versus 58 percent—and one other country has essentially the same targeting rate as PATH. The remaining two countries have substantially lower rates that PATH, 40 percent and 36 percent.

In reviewing these data, it is important to keep in mind that the policies themselves, their geographic coverage, and the techniques used for estimating the distribution of benefits may all be quite different for the different programmes. Nevertheless, the available data suggest that PATH has achieved a reasonable level of targeting.

2. Detailed Targeting Comparisons with PROGRESA

The most fully studied of the CCT programmes is the PROGRESA programme in Mexico (now known as Oportunidades), and the literature on PROGRESA makes it possible to conduct a somewhat more detailed comparison between PATH and this programme. PROGRESA, which was implemented in the 1990s, is very similar in structure to PATH. Thus, PROGRESA's experience may shed some light on what is reasonable to expect in the Jamaican context. (The targeting estimates for PROGRESA discussed below are as of 1998 and are presented in Skoufias et al. 1999.)

Estimates from PATH for this section (presented in Table VIII.2) are based on tabulations presented in Chapter IV. Note that there are a number of technical difficulties in making comparisons between the two programmes. We address some of these as we present the results. We also address additional complications in a subsequent subsection, after providing an overview of the basic results.

Table VII.2 shows the percentages of programme beneficiaries who would be considered poor for PATH and PROGRESA, under alternative definitions of "poor." We believe there are two useful ways to assess these comparisons, each of which has both advantages and disadvantages. One approach is to compare tabulations of data from the two programmes using a common percentile-based definition of poverty. Because of limitations in the data available for PROGRESA in published reports, there are two percentile poverty level cutoffs that can be compared across the two programmes, one at the 25th consumption percentile and one at the

52nd. As shown in the table, if living in poverty is defined as being below the 25th consumption percentile, then 39 percent of the PROGRESA households are poor, compared with 63 percent of PATH people.[28] If, on the other hand, a broader definition is used under which living in poverty is defined as being below the 52nd percentile, then the numbers of programme participants in poverty are 74 percent of households for PROGRESA and 89 percent for PATH people. These comparisons, based on the first of our two approaches to the assessment, suggest that PATH has been more successful than PROGRESA at targeting its benefits to the poor.

A second way of making comparisons is to consider how each programme has done relative to the definition of poverty in the country where the programme is operating. According to the PROGRESA evaluation source referenced earlier, Mexico's definition of poverty in planning the PROGRESA intervention was about the 52nd percentile of consumption, and under this definition 74 percent of PROGRESA's households are poor. By contrast, Jamaica was using a much more rigorous definition of poverty in structuring PATH: about the 20th percentile. As noted earlier, under this standard, 58 percent of people in PATH would be considered poor. Thus, PROGRESA was able to reach a higher proportion of the people it was targeting, but its task was presumably easier because it was targeting a broader segment of the overall population.

3. Some Qualifications

While we believe that the estimates in the table are useful, it is very important to note that several qualifications make the numbers not strictly comparable across programmes, including the following:

[28] PROGRESA's study uses adult-equivalent per capita consumption, whereas the PATH targeting analysis reported here uses per capita consumption (to be consistent with the way official poverty statistics are produced in Jamaica). Furthermore, PROGRESA's tabulations were done at the household level, whereas PATH's tabulations were done at the individual level. When PATH's tabulations are done at the household level, and using adult-equivalent consumption, the corresponding numbers in Table VII.2 are 52, 59, and 84 percent.

TABLE VII.2

ASSESSING PATH'S TARGETING RELATIVE TO PROGRESA
(Entries are percentages of programme participants with consumption *below* the percentile cutoffs shown)

	Percent of Programme Participants that Are "Poor" Under the Definition	
Definition of "Poor"	PROGRESA	PATH
Below 20th percentile of overall population consumption levels	NA	58
Below 25th percentile of overall population consumption levels	39	63
Below 52nd percentile of overall population consumption levels	74	89

Note: PROGRESA estimates are from Tables 4 and 5 in Skoufias et al 1999. PATH estimates are from tabulations presented earlier in this report. The percentiles used in defining the row headings are based on the availability of information from Skoufias et al 1999.

- The PROGRESA and PATH programmes are structured based on different definitions of poverty. As we have already noted, the PATH programme uses the official Jamaican poverty level, which is about the bottom 20 percent of the consumption distribution, while the PROGRESA programme uses a broader targeting standard of roughly the bottom 50 percent of the consumption distribution.

- The PROGRESA programme, at least as of the time covered by the available data, attempted to target only very "marginal" (i.e., very poor) localities in the country, thus essentially excluding poor households in higher-income areas. Focusing only on high-poverty areas may have helped PROGRESA in its targeting. (The PROGRESA data in the table are limited to the marginal localities included in the programme.)

- The comparisons are based on one measure of targeting (percentage of participants who are below some consumption threshold). However, there are other criteria that may be important in making a comprehensive comparison. For example, undercoverage (the percentage of poor households not covered by the programme) and severity of poverty (a measure of how far from the poverty threshold households fall) are two other criteria to consider.

- Note that the available published assessment of targeting in PROGRESA that we have been able to locate focuses on the programme as it was structured in 1998. Since then the programme has changed, but it is not clear whether this has left overall targeting unaffected, improved, or lessened.

- While the methodologies to assess targeting in both studies are similar, there are differences that may make the comparisons between the two programmes difficult. Both studies use a consumption-based measure to assess whether a participant is poor or not. However, the two studies differed in what consumption measure was used and whether the analysis was done at the household or the individual level.

B. IMPACTS ON SCHOOLING

Comparing schooling-related CCT outcomes across countries is complicated by the fact that the outcome variables studied are different for PATH than for most other CCTs. In particular, reflecting the priorities in the countries where the programmes were implemented, most previous programmes and their evaluations have focused on school *enrollment,* whereas the focus of PATH has been on school *attendance* (because school enrollment is already very high in Jamaica, especially when compared with other countries using CCT programmes). Nevertheless, some idea of the size of the effects which might reasonably be expected can be gained by comparing observed *percentage* changes in the measures studied.

Relevant information is available on 11 programmes in all—PATH and 10 other programmes conducted in various countries. For the 10 others, the outcome variable available is the percentage of children *enrolled* in schools, while for the PATH programme it is the percentage of enrolled children *attending* school on an average day. Table VII.3 classifies these 11 programmes based on the starting percentage of the outcome variable (the column headings) and the estimated increase in outcome variable (the row headings). For instance, the entries in the middle column indicate that there were a total of five programmes with baseline values between 50 and 80 percent and that among those programmes, three achieved an outcome improvement of between 5 and 10 percent, while two achieved an outcome improvement of greater that 10 percent.

All of the programmes included in this analysis registered increases in the outcome variables which they were trying to affect. However, the main implication of the table for our current discussion is that if the outcome variable in percentage terms is already quite high, then the achievable change appears to be quite limited. Thus for each of the four CCTs where the relevant outcome variable started as a percentage greater than 80 percent, the increase in the

outcome achieved by the programme was relatively limited—under 5 percent. On the other hand, where the outcome variable expressed in percentage terms was quite low at the outset, the percentage increase tended to be considerably larger. For instance, each of the two programmes which started with a baseline variable below 50 percent was able to achieve an increase of more that 10 percent (as shown in the first column).

These relationships can be further illustrated using additional information provided by the source document cited in the table. Two of the countries considered that had the highest starting value for enrollment rates were Brazil and Honduras, and their CCTs had only relatively modest--under 5 percent—estimated effects on the outcome variables studied. On the other hand, Bengladesh and Cambodia were among the lowest observations in terms of baseline school enrollment, and they achieved estimated impacts with their CCTs of approximately 12 and 26 percent, respectively.

TABLE VII.3

ESTIMATED PERCENTAGE EFFECTS ON ENROLLMENT
OR ATTENDANCE, BY STARTING VALUES
(Entries are numbers of CCTs in each cell)

	Baseline Value of Outcome of Interest, Expressed as a Percentage Enrolled or Percentage Attending on a Given Day		
Estimated Percent Increase in outcome variable	< 50 percent	50-80 Percent	>80 percent
0-5 percent			4 (includes PATH)
5-10 percent		3	
>10 percent	2	2	
Column total	2	5	4

Source: Adapted from slide 11, Schady (2006).

In Chapter V, we estimated that PATH's effect on attendance was relatively modest—of the magnitude of about 3 percent. The findings in Table VII.3 provide a useful perspective on that

finding. Specifically, they suggest that the relatively small size of the PATH effect (in percentage terms) is not surprising, given that the starting attendance rate was quite high.

C. IMPACTS ON HEALTH CARE UTILIZATION

The PATH evaluation results in the area of preventive care utilization also appear to be broadly consistent with findings from other countries, at least for children. Schady (2006) indicates that in Colombia a CCT had an estimated effect on "growth monitoring" of 23-33 percentage points, while the comparable figure in Honduras was an effect of approximately 20 percentage points. An increase in children's preventive care use in Mexico is also noted, although no size of the effect is given.

As presented earlier, in Table VI.3, the estimated increase for Jamaica in the average number of preventive care visits is estimated to be approximately 38% percent. This is very consistent with these other findings.

One area in which the PATH results differ from those of other countries is preventive care for adults. The survey of other countries' experiences (Schady 2006) notes that for PROGRESA, preventive visits went up for adults age 50 and over, which contrasts with our finding for Jamaica of essentially no effects on health care utilization by the elderly. This finding may be explained by the fact that Jamaica did not enforce sanctions for elderly who did not comply with the health requirement.

D. CONCLUSIONS FROM THE CROSS-COUNTRY COMPARISONS

After taking into account differences in starting contexts across countries, the findings for PATH appear to be quite consistent with those from other programmes. This is true with regard to the existence and even the magnitude of effects. Broadly speaking, there seems to be a growing body of evidence that conditional transfers can have significant effects in increasing

school attendance and preventive health visits. The findings for Jamaica are part of this evidence.

But the Jamaica findings are also consistent with the experience of other countries in that the increased use of schools and health centres may not be translating into improvements in "deeper" or longer-terms outcomes such as school advancement, grades, or health status. As Dr. Schady points out in his review of CCT evaluations, this may suggest the importance of interactions between CCTs and (a) other household determinants of education and health status, and (b) quality of service providers. While getting children to schools and health centres is an important achievement, it is not clear that CCTs are contributing to school performance and health status, which are the ultimate outcomes many policymakers are interested in.

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VIII. CONCLUSIONS

Previous chapters have examined PATH from multiple perspectives, including

- the perceptions and experiences of the clients and the public employees involved with the programme;
- the analysis of quantitative data from the programme's management information system; and
- the analysis of information from the client surveys undertaken to support the impact analysis.

The results of these different perspectives are generally consistent with one another and show that PATH has had considerable success in achieving its goals. However, there is also evidence that changes in PATH could be useful in some areas. We summarize the relevant findings below.

A. IMPLEMENTATION

It is clear that the MLSS has been basically successful in setting up operations which implement the PATH concept. Key accomplishments include:

- Developing a functioning formula for determining eligibility for the programme.
- Setting up procedures for publicizing the availability of PATH and enrolling clients into the programme, largely through the functions of the MLSS parish offices.
- Setting up a system for disbursing benefits through the postal system.
- Setting up the MIS for compiling key information about client accounts and for supporting the benefit disbursement process.
- Setting up procedures for obtaining information from schools and clinics about compliance with PATH's requirements, and feeding that information into the system for determining payments.

Together, these accomplishments have created a programme which is widely viewed by stakeholders as having accomplished its basic mission. Survey results indicate that most PATH

participants report having had satisfactory transactions with the programme.[29] Furthermore, the results from the quantitative outcomes analysis also suggest that PATH is having an impact on attendance in schools and health centres. All of these findings point to a programme which has been successfully implemented.

However, the findings from the qualitative analysis suggest a number of areas where improvements may be possible. We note here some problems with implementation:

- There are often delays in making PATH payments available to clients. The resulting uncertainty about the timing of benefit availability imposes substantial costs and sometimes hardships on clients.
- Many stakeholders, including both clients and also workers at schools and health clinics, believe that there are problems in the accuracy of the information used for enforcing the sanctions related to school and health care requirements. There appear to be particular issues in getting medical excuses for school absences reflected in the system and in dealing with situations where clients use a health care facility other than the one they typically use.
- Clients report difficulties and delays in obtaining information from MLSS representatives about their cases and in having changes made in their case records

These and similar issues warrant review by Jamaica officials to ensure that PATH policies are being implemented as completely as possible.

B. BENEFIT TARGETING

The evidence from the PATH management information system, reviewed in Chapter IV, indicates that PATH has been highly successful at targeting its benefits to the Island's poorest households. Specifically, 58 percent of benefits were found to go to the poorest quintile of the population. Comparisons made in Chapter VII to CCT programmes in other countries suggest that this figure compares favorably with results that have been attained elsewhere.

[29] PATH's Targeting Report, Mathematica Policy Research, June, 2004.

The targeting of benefits may still be improved, however. During interviews and focus groups, not only clients but also school and health facility staff indicated that they were aware of households receiving PATH benefits who were probably not deserving of them, while they also knew of certain other households who had been turned down by PATH, even though their need for assistance seemed very great. During these discussions, it was suggested that greater in-person monitoring by MLSS staff—including more visits to clients' homes to verify information the households provided—could be useful in improving the targeting.

C. IMPACTS ON SCHOOL ATTENDANCE

PATH has been effective at accomplishing its objective of encouraging households to send their children to school with greater regularity. Multiple regression analysis results reported in Chapter V show that PATH has increased school attendance by approximately 0.5 days per month. The estimated increase is about 3 percent over the baseline level, and it is statistically significant.

The size of this estimated effect should be viewed in the context of the fact that school attendance was already quite high in Jamaica. Specifically, the starting attendance rates were on the order of 85 percent. Also, as in other countries, illnesses and other legitimate reasons for missing school are not uncommon. Together, these factors have constrained the possible size of the estimated effect of attendance—there was only limited room for improvement.

In general, countries with baseline conditions similar to those in Jamaica have experienced education-related effects similar to those found with PATH. It should be pointed out, however, that comparisons with other countries are somewhat complicated by the fact that most other CCT programmes have tended to focus on increasing enrollment, while PATH focuses on increasing attendance.

The results of discussions with parents and with school staff were consistent with the statistical findings. Parents had a reasonably clear understanding of the programme and the conditionality of the benefits. School staff indicated that in their experience, the parents were highly aware of the requirements of the PATH programme and were making good-faith efforts to increase their children's attendance.

In response to survey questions about why their children were sometimes absent from school, parents indicated that PATH had relieved some of the financial pressures associated with attendance, such as providing lunch money and payments for transportation.

D. IMPACTS ON HEALTH OUTCOMES

PATH has also been successful in meeting its objective of increasing the use of preventive health care for children in PATH families. The results of the statistical analysis suggest that health care visits for children 0-6 years old increased by approximately 38 percent as a result of PATH. As with educational outcomes, the magnitude of this effect appears to be broadly consistent with the corresponding effects in other countries which have enacted CCTs.

Regression analysis of the impact of PATH on health care visits by the elderly shows no evidence of effects on this group. One potential explanation is that early in the programme a decision was made not to enforce the conditionality requirements for this group of PATH recipients.

E. IMPACTS ON OTHER OUTCOMES

While PATH has been successful at increasing school attendance and preventive health visits, there is no evidence that it was able to affect longer-term outcomes such as grades, advancement to next grade, or health care status. There are at least two broad sets of explanations for this finding. The first one is methodological. It is possible that not enough time has elapsed to be able to observe the long term effects of the programme. On average, the

participant group had only received PATH benefits for about a year, and this may simply not be enough time for long term effects to emerge. It is also possible that there may not have been enough statistical power to detect these long-term effects. The lack of statistical power may be due to the fact that these effects are probably small (especially given that they may only exist for the subset of children who actually increased their attendance as a result of the programme) and to the fact that the variables used for measuring long term outcomes were likely to be susceptible to measurement error.

Another possible explanation behind the finding is that increasing attendance to school and health centres is simply not enough by itself to substantially improve longer term outcomes. It is possible that improvements in the infrastructure of schools and health centres or in the quality of services provided in education and health are needed for increased attendance to translate into improved long term outcomes.

Assessing which of these two explanations is more prevalent in this context is necessarily speculative. But if the latter explanation is accurate, it suggests that to improve the human capital of poor households through PATH, policymakers may want to pay particular attention to the interaction between PATH and factors related to the delivery of education and health services in Jamaica.

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APPENDIX 1

SURVEY RESPONSE ANALYSIS

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This appendix presents information on survey nonresponse. As in any other survey, there was some degree of nonresponse because, among other reasons, (1) households refused to respond to the survey, and (2) not all households were located. We present the response rates for the baseline and follow-up surveys (Tables 1.1 and 1.2); compare the respondents and nonrespondents in terms of demographic and socio-economic characteristics for the whole evaluation sample, and then separately for the participant and comparison groups. We also tested whether differences between respondents and nonrespondents are significant; p-values are reported accordingly. While some differences are statistically significant, our assessment is that, practically speaking, they are rarely large.

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TABLE 1.1

BASELINE SURVEY: RESPONSE RATES AND DATES OF INTERVIEW

	Participant Group	Comparison Group
Overall Response Rate (%)	90.4%	90.6%
Households Interviewed by Month (%)		
Jan-04	37.6	39.2
Feb-04	48.5	48.0
Mar-04	12.1	11.1
Apr-04	1.7	1.7
May and June-04	0.1	0.0
No. of Households Interviewed	2,259	2,263

Source: PATH 2004 Survey

TABLE 1.2

FOLLOW-UP SURVEY: RESPONSE RATES AND DATES OF INTERVIEW

	Participant Group	Comparison Group
Overall Response Rate (%)	**81.4%**	**83.3%**
Households Interviewed by Month (%)		
May 2005 or earlier	0.5	0.6
June 2005	4.9	4.9
July 2005	38.4	38.7
August 2005	47.1	46.2
September 2005	8.7	8.6
October 2005 or later	0.5	0.8
Missing	0	0.1
No. of Households Interviewed	**2,036**	**2,083**

Source: PATH 2005 Survey

TABLE 1.3A

PROGRAMME-RELATED CHARACTERISTICS OF PATH 2005 RESPONDENTS AND NONRESPONDENTS: WHOLE EVALUATION SAMPLE
(Percent unless otherwise noted)

Characteristics	Respondents	Non-respondents	P-value
Eligibility Score			
Average	1,031	1,031	0.393
Minimum	1,017	1,017	
Maximum	1,037	1,037	
Distribution (%)			
< 1,000	0	0	--
1,000 –< 1,010	0	0	--
1,010 –< 1,020	6	7	0.672
1,020 –< 1,030	27	28	0.764
1,030 –< 1,040	66	65	0.616
>=1,040	0	0	--
Benefit Type (%)			
Health	25	27	0.140
Education	53	56	0.022
Elderly	5	4	0.033
Disabled	2	2	0.331
Pregnant/Lactating	4	4	0.661
Adult poor	0	0	0.647
None	10	7	0.000
Application Date (%)			
Before April 2002	0	0	--
April 2002-June 2002	77	62	0.000
July 2002-September 2002	7	15	0.000
October 2002-December 2002	4	5	0.174
January 2003-March 2003	10	15	0.000
April 2003-June 2003	2	4	0.004
Sample Size (No. of Households)	**4,119**	**881**	

Source: Management Information System (MIS)

TABLE 1.3B

DEMOGRAPHIC CHARACTERISTICS OF PATH 2005 RESPONDENTS
AND NONRESPONDENTS: WHOLE EVALUATION SAMPLE
(Percent unless otherwise noted)

Characteristics	Respondents	Nonrespondents	P-value
No. of Household Members (%)			
1	0	0	--
2	7	9	0.238
3-5	65	66	0.337
6 and over	28	25	0.084
Age (%)			
< 3 years	9	9	0.455
3-5 years	10	11	0.072
6-17 years	40	42	0.012
18-59 years	34	33	0.024
>59 years	6	5	0.028
Location (%)			
Kingston	9	19	0.000
Other town	16	21	0.000
Rural	75	60	0.000
Sample Size (No. of Households)	**4,119**	**881**	

Source: Management Information System (MIS)

TABLE 1.3C

SOCIOECONOMIC CHARACTERISTICS OF PATH 2005 RESPONDENTS
AND NONRESPONDENTS: WHOLE EVALUATION SAMPLE
(Percent unless otherwise noted)

Characteristics	Respondents	Non-respondents	P-value
Water Source			
Indoor tap/pipe	6	5	0.829
Outside private pipe	27	36	0.000
Public standpipe	31	30	0.715
Other	36	29	0.000
Main Material on the Outer Wall			
Wood	49	55	0.003
Block and steel	37	33	0.037
Other	14	12	0.170
Toilet Facilities			
Exclusive use	78	72	0.000
Shared	22	28	0.000
Weekly Spending (J$)			
Average	361	344	0.098
Median	300	300	
Telephone			
Yes	11	9	0.061
No	53	53	0.947
Cellular	36	38	0.256
Household Items			
Gas stove(s)	60	58	0.1902
Electric stove(s)	0	0	0.3008
Refrigerator(s) or freezer(s)	35	31	0.0186
Fan(s)	24	27	0.0126
Stereo equipment	16	15	0.4284
Video equipment	8	5	0.0032
Washing machine(s)	0	0	0.9770
Television(s)	59	58	0.3569
Motor bike(s)	1	1	0.9022
Car(s) and/or other vehicle(s)	0	0	0.2318
Sample Size (No. of Households)	**4,119**	**881**	

Source: Management Information System (MIS)

TABLE 1.4A

PROGRAMME-RELATED CHARACTERISTICS OF PATH 2005 RESPONDENTS AND NONRESPONDENTS: PARTICIPANT GROUP
(Percent unless otherwise noted)

Characteristics	Respondents	Non-respondents	P-value
Eligibility Score			
Average	1,027	1,027	0.361
Minimum	1,017	1,017	
Maximum	1,035	1,035	
Distribution (%)			
< 1,000	0	0	--
1,000 –< 1,010	0	0	--
1,010 –< 1,020	13	13	0.975
1,020 –< 1,030	55	53	0.338
1,030 –< 1,040	32	34	0.298
>=1,040	0	0	--
Benefit Type (%)			
Health	29	32	0.023
Education	57	57	0.801
Elderly	6	4	0.006
Disabled	2	2	0.455
Pregnant/Lactating	5	5	0.627
Adult poor	0	0	0.631
None	2	1	0.022
Application Date (%)			
Before April 2002	0	0	--
April 2002-June 2002	67	50	0.000
July 2002-September 2002	5	9	0.00
October 2002- December 2002	6	9	0.092
January 2003-March 2003	18	25	0.000
April 2003-June 2003	4	7	0.018
Sample Size (No. of Households)	**2,036**	**464**	

Source: Management Information System (MIS)

TABLE 1.4B

DEMOGRAPHIC CHARACTERISTICS OF PATH 2005 RESPONDENTS
AND NONRESPONDENTS: PARTICIPANT GROUP
(Percent unless otherwise noted)

Characteristics	Respondents	Non-respondents	P-value
No. of household members (%)			
1	0	0	--
2	6	7	0.282
3-5	64	68	0.140
6 and over	30	25	0.036
Age (%)			
< 3 years	9	10	0.242
3-5 years	11	12	0.047
6-17 years	40	42	0.202
18-59 years	33	31	0.039
>59 years	6	5	0.008
Location (%)			
Kingston	12	23	0.000
Other town	15	18	0.140
Rural	73	59	0.000
Sample Size (No. of Households)	**2,036**	**464**	

Source: Management Information System (MIS)

TABLE 1.4C

SOCIOECONOMIC CHARACTERISTICS OF PATH 2005 RESPONDENTS
AND NONRESPONDENTS: PARTICIPANT GROUP
(Percent unless otherwise noted)

Characteristics	Respondents	Nonrespondents	P-value
Water Source			
Indoor tap/pipe	6	5	0.530
Outside private pipe	28	37	0.000
Public standpipe	30	31	0.736
Other	36	27	0.000
Main Material on the Outer Wall			
Wood	53	59	0.016
Block and steel	33	28	0.036
Other	14	13	0.531
Toilet Facilities			
Exclusive use	75	68	0.001
Shared	25	32	0.001
Weekly Spending (J$)			
Average	347	328	0.140
Median	286	300	
Telephone			
Yes	10	7	0.080
No	52	50	0.352
Cellular	38	43	0.046
Household Items			
Gas stove(s)	62	61	0.8994
Electric stove(s)	0	0	0.4994
Refrigerator(s) or freezer(s)	35	29	0.0149
Fan(s)	23	30	0.001
Stereo Equipment	14	15	0.4018
Video Equipment	8	6	0.0773
Washing Machine(s)	0	0	0.4182
Television(s)	62	58	0.1299
Motor bike(s)	0	0	0.9066
Car(s) and/or other vehicle(s)	0	0	0.408
Sample Size (No. of Households)	**2,036**	**464**	

Source: Management Information System (MIS)

TABLE 1.5A

PROGRAMME-RELATED CHARACTERISTICS OF PATH 2005 RESPONDENTS AND NONRESPONDENTS: COMPARISON GROUP
(Percent unless otherwise noted)

Characteristics	Respondents	Nonrespondents	P-value
Eligibility Score			
Average	1,036	1,036	0.637
Minimum	1,035	1,035	
Maximum	1,037	1,037	
Distribution (%)			
< 1,000	0	0	
1,000 –< 1,010	0	0	
1,010 –< 1,020	0	0	
1,020 –< 1,030	0	0	
1,030 –< 1,040	100	100	
>=1,040	0	0	
Benefit Type (%)			
Health	23	22	0.396
Education	50	54	0.002
Elderly	4	4	0.812
Disabled	2	3	0.029
Pregnant/lactating	4	4	0.994
Adult poor	0	0	--
None	17	13	0.000
Application Date (%)			
Before April 2002	0	0	--
April 2002-June 2002	88	76	0.000
July 2002-September 2002	8	20	0.000
October 2002-December 2002	1	1	0.265
January 2003-March 2003	3	2	0.777
April 2003-June 2003	0	1	0.258
Sample Size (No. of Households)	**2,083**	**417**	

Source: Management Information System (MIS)

TABLE 1.5B

DEMOGRAPHIC CHARACTERISTICS OF PATH 2005 RESPONDENTS
AND NONRESPONDENTS: COMPARISON GROUP
(Percent unless otherwise noted)

Characteristics	Respondents	Nonrespondents	P-value
No. of Household Members (%)			
1	0	0	--
2	9	10	0.426
3-5	65	65	0.891
6 and over	26	25	0.704
Age (%)			
< 3 years	9	7	0.012
3-5 years	10	11	0.677
6-17 years	40	43	0.022
18-59 years	35	34	0.333
>59 years	6	5	0.699
Location (%)			
Kingston	6	14	0.000
Other town	17	25	0.000
Rural	77	62	0.000
Sample Size (No. of Households)	**2,083**	**417**	

Source: Management Information System (MIS)

TABLE 1.5C

SOCIOECONOMIC CHARACTERISTICS OF PATH 2005 RESPONDENTS
AND NONRESPONDENTS: COMPARISON GROUP
(Percent unless otherwise noted)

Characteristics	Respondents	Non-respondents	P-value
Water Source			
Indoor tap/pipe	6	6	0.731
Outside private pipe	27	34	0.002
Public standpipe	31	29	0.375
Other	37	31	0.027
Main Material on the Outer Wall			
Wood	46	50	0.114
Block and Steel	41	39	0.514
Other	14	11	0.170
Toilet Facilities			
Exclusive use	80	76	0.041
Shared	20	24	0.041
Weekly Spending (J$)			
Average	374	361	0.421
Median	300	300	
Telephone			
Yes	12	11	0.409
No	54	57	0.251
Cellular	34	32	0.523
Household Items			
Gas stove(s)	59	54	0.0644
Electric stove(s)	0	0	0.4381
Refrigerator(s) or freezer(s)	36	34	0.4149
Fan(s)	24	24	0.8587
Stereo equipment	19	15	0.0802
Video equipment	8	5	0.0169
Washing machine(s)	0	1	0.4384
Television(s)	57	57	0.9014
Motor bike(s)	1	1	0.9145
Car(s) and/or other vehicle(s)	1	0	0.3835
Sample Size (No. of Households)	**2,083**	**417**	

Source: Management Information System (MIS)

APPENDIX 2

IMPLICATIONS OF DELAYS IN THE TARGETING SURVEY

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In principle, the targeting assessment would be based on some measure of the well-being of participants before they received benefits from the programme. Therefore, the PATH 2003 Survey was designed to be administered before participants started receiving benefits from PATH. However, as noted in Chapter I, various delays in getting the survey into the field resulted in about two-thirds of survey respondents having registered for the programme prior to responding to the survey. In this appendix, we examine the implications of this deviation for the targeting analysis results presented in Chapter IV.[1]

The basic approach in Chapter IV involves estimating targeting variables using data that, for some participant households, may reflect PATH payments. We begin by noting that this approach is *conservative* in that it tends to make participants look *less poor* than they would have been had they not already started receiving PATH payments by the time of the survey. Thus, to the extent that we conclude in Chapter IV that PATH is relatively well targeted, the fact that we used data from a survey conducted earlier than it actually was would have reinforced this conclusions. Nevertheless, it is of interest to examine the likely pre-PATH consumption of these participant households.

While we cannot observe the level of consumption by households just prior to receiving PATH benefit payments, we can *approximate* it by estimating the changes in consumption that the payments may have generated. In particular, we estimated what consumption would have been at the time of the survey interview if the household not received the payment.[2]

[1] While two-thirds of households reported they were "currently participating" in PATH, it was not clear whether they had actually received payments from PATH.

[2] For households that did not receive PATH payments by the time of the survey, we did not make any adjustments to the consumption level estimated from the survey.

A. METHODS

To estimate this "adjusted" level of consumption, we would ideally like to know if households modified their labor market activities in response to the payment. In particular, we would need to know if they worked fewer hours and earned less income. Since the cash transfer represents a modest proportion of the consumption levels of these households (about 10 percent of the average per capita consumption of PATH households), and since many of these households are poor or close to being poor,[3] it seems reasonable to assume that households are unlikely to substantially change their labor market activities and earned income in response to the payment.

In light of this, we assumed for this sensitivity analysis that households did not make any labor market adjustments in response to PATH.[4] We also assumed that if households had not received the PATH payment, their consumption would have been lower than their actual consumption by the full payment amount. This assumes that the adjusted annual consumption expenditure is equal to the original consumption expenditure minus the payment amount (annualized). Under this assumption, the difference between the adjusted consumption and unadjusted one is as large as possible; hence, this represents the maximum possible change in the targeting figures presented earlier.

B. RESULTS

The targeting results did not change substantially when we adjusted consumption as described above to reflect the fact that PATH participants received payments before being

[3] As indicated earlier in Chapter IV, 59 percent of PATH beneficiaries have consumption levels below the poverty line, and 94 percent have consumption levels below twice the poverty line. Moreover, these households are likely to face severe liquidity constraints, which may limit their ability to adjust their labor market activities.

[4] While we do not have estimates of labor supply elasticity in Jamaica, studies in other countries suggest that the labor supply income elasticity of primary earners is either zero or extremely small.

interviewed (Table 2.1). When the adjusted consumption measure is used, the percentage of PATH households in the bottom quintile of the distribution is 61 percent, compared with 58 percent when the unadjusted measure. Similarly, the percentage of PATH households that are classified as poor using the adjusted consumption measure is 61 percent, compared with 59 percent using the unadjusted measure.

These results suggest that the timing of the survey did not substantially affect the results reported in Chapter IV.

TABLE 2.1

ASSESSMENT OF TARGETING OF PATH
Percentage of PATH Participants That Fall In Each Category

	No Adjustments	With Adjustments
Quintile		
I	58	61
II	21	21
III	14	13
IV	5	4
V	1	1
Poor	59	61
Extreme Poor	27	29

Source: PATH 2003 Survey, Jamaica Survey of Living Conditions 2002 (SLC-2002). In "with adjustments" column, consumption was adjusted to reflect the fact that some participants had received payment before the interview.

Quintile I corresponds to the % of PATH participants who are in the bottom 20% of Jamaica's per capita consumption distribution. Quintile V corresponds to the % of PATH participants who are in the top 20% of Jamaica's per capita consumption distribution.

Poor: People whose adult-equivalent consumption is below Jamaica's official poverty line.
Extreme Poor: People whose adult-equivalent consumption is below Jamaica's official food poverty line.

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APPENDIX 3

VALIDITY OF SURVEY-BASED OUTCOMES

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The outcome measures used in the impact analysis come from the follow-up survey. As indicated in Chapter V, one possible methodological concern is that households from the participant group may believe that they have an incentive to over-report attendance to school and health centres because of a perceived threat that Statin could report back to the MLSS and generate sanctions for noncompliance with PATH. While this over-reporting concern is an issue in many other similar studies, including the PROGRESA evaluation and many (or perhaps all) other evaluations of conditional cash transfer programs, we attempt in this appendix to assess the likelihood and severity of the issue.

If participant group households are indeed over-reporting their outcomes, and if they are doing so to a greater extent than the comparison group, this would lead us to overstate the effects of PATH on these outcomes. While we cannot fully rule out that this may have happened to some extent, we present several arguments that lead us to believe that this concern is unlikely to explain our results. Furthermore, we note that, because this concern cannot be fully discarded, we tried to compensate for the potential of the issue by making conservative decisions in research choices in other parts of the analysis (by not adjusting for take-up rates and crossover rates, and by choosing conservatively the regression specifications used to discuss the policy implications) so as to compensate for the possible existence of some degree of over-reporting.

Following are the arguments that would suggest that over-reporting did not play a large role in our impact estimations:

- The organization in charge of data collection (Statin) made it very explicit to households that their role was to collect data, and that they were not representing the government or administrators of any program.
- The questionnaire was designed in a way that the outcome questions related to school attendance and health centre visits were asked before any questions about PATH.
- Other parts of the evaluation are generally supportive of the main impact findings. For example, the qualitative analysis suggests that parents were not only aware of the requirements of the program but also reported sending their children more often to

school and health centres. The teachers themselves also reported increased attendance on the part of PATH children. Other questions in the survey also suggest increased attendance at school and health centres. For example, participant group households reported larger increases in school attendance (though not in preventive visits) than the comparison group. When asked about why children did not attend school, participant group households were less likely to cite reasons related to lack of money. Finally, participant households have a clear understanding of the programme's rules and are very aware of the incentives that sanctions provided, so it is not surprising to see that they responded to these incentives by sending their children to school more often and taking them to health centres for preventive visits more often.

APPENDIX 4

PLACEBO TESTS

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The regression discontinuity design involves performing a regression in which the dependent variable is the outcome of interest, the key explanatory variable is an indicator of whether the person is in the participant group or not, and a key control variable is a score (or some function of it) to statistically account for any differences between the participant and comparison groups related to program participation. Two key issues to address are determining what functional form to use in entering the score into the estimation equation and assessing the extent to which the score really statistically accounts for differences between the participant and comparison groups. To address these two issues, we ran "placebo regressions" using data from the baseline survey. The idea was to regress the *baseline* version of the outcome on participant dummy and a function of eligibility score:

$$Outcome_{ij}^{B} = \alpha_0 + \alpha_1 PART_j + \alpha_2 f(Score_j) + \alpha_3 X_{ij} + \alpha_4 APPDATE_j + u_{ij}$$

If the score (or a function of it) indeed statistically accounts for any differences between the two groups, then we should not expect the coefficient on participation dummy (α_1) to be statistically significant. Furthermore, performing this regression could help us assess whether certain functional forms of the variable score add much explanatory power over the simple linear specification and can therefore guide the selection of functional form. In this appendix, we describe the placebo tests and present the findings.

A. TECHNICAL SPECIFICATIONS OF PLACEBO TESTS

The key technical specifications used in conducting the placebo tests are the following:

- For every outcome, we performed the following 4 regressions:
  - Spec 1 – Score enters linearly
    $$Outcome_{ij}^{B} = \beta_0 + \beta_1 PART_j + \beta_2 Score_j + \beta_3 APPDATE_j + u_{ij}$$
  - Spec 2 - Score enters in quadratic form
    $$Outcome_{ij}^{B} = \beta_0 + \beta_1 PART_j + \beta_2 Score_j + \beta_3 Score_j^2 + \beta_4 APPDATE_j + u_{ij}$$
  - Spec 3 - Score enters in cubic form
    $$Outcome_{ij}^{B} = \beta_0 + \beta_1 PART_j + \beta_2 Score_j + \beta_3 Score_j^2 + \beta_4 Score_j^3 + \beta_5 APPDATE_j + u_{ij}$$
  - Spec 4 – Score enters linearly and include household characteristics
    $$Outcome_{ij}^{B} = \beta_0 + \beta_1 PART_j + \beta_2 Score_j + \beta_3 APPDATE_j + \beta_4 X_{ij} + u_{ij}$$
- We used the following outcomes:
  - Attendance at school in reference period [Number of Days]
  - Attendance at school in ”typical” period [Number of Days]
  - Attendance at health centres for preventive reasons for children 0-6 [Number of Visits]
  - Attendance at health centres for preventive reasons for elderly [Number of Visits]
- Household characteristics (in X vector) include demographics (age and education of head of household, family size, number of infants, etc.); housing characteristics (materials of the house, access to electricity and water, etc.); and ownership of assets (gas stove, refrigerator, car, etc.).
- Application date refers to the date on which a household applied to PATH. There is a binary indicator for each date. Hence, regressions are fixed-effects at the application date level.
- Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.

B. RESULTS OF PLACEBO TESTS

TABLE 4.1

PLACEBO TESTS FOR SCHOOL ATTENDANCE IN REFERENCE PERIOD
(Dependent Variable: Number of Days Attending School in 20-Day Reference Period)

	Specification			
	(1)	(2)	(3)	(4)
Participant group	-0.081 (0.208)	-0.113 (0.302)	-0.113 (0.302)	-0.171 (0.211)
Eligibility score	-0.008 (0.016)	0.948 (6.402)	0.000 (0.000)	-0.015 (0.018)
Eligibility score squared		0.000 (0.003)	0.000 (0.003)	
Eligibility score cubed			0.000 (0.000)	
Controls for household characteristics	No	No	No	Yes
No. of observations	**7,145**	**7,145**	**7,145**	**7,112**

Standard errors in parenthesis.

Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level.

TABLE 4.2

PLACEBO TESTS FOR SCHOOL ATTENDANCE IN TYPICAL PERIOD
(Dependent Variable: Number of Days Attending School in 20-day Typical Period)

	Specification			
	(1)	(2)	(3)	(4)
Participant group	0.288 (0.209)	0.260 (0.293)	0.260 (0.293)	0.128 (0.215)
Eligibility score	0.023 (0.017)	0.852 (6.827)	0.000 (0.000)	0.030 (0.019)
Eligibility score squared		0.000 (0.003)	0.000 (0.003)	
Eligibility score cubed			0.000 (0.000)	
Controls for household characteristics	No	No	No	Yes
No. of observations	**7,124**	**7,124**	**7,124**	**7,091**

Standard errors in parenthesis.

Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level.

TABLE 4.3

PLACEBO TESTS FOR PREVENTIVE HEALTH VISITS FOR CHILDREN 0-6 YEARS
(Dependent Variable: Number of Preventive Health Care Visits in Past 6 Months for Children 0-6)

	Specification			
	(1)	(2)	(3)	(4)
Participant group	0.043 (0.081)	0.208* (0.117)	0.208* (0.117)	-0.035 (0.086)
Eligibility score	0.001 (0.007)	-4.930 (2.628)	0.000 (0.000)	0.000 (0.007)
Eligibility score squared		0.002 (0.001)	-0.002 (0.001)	
Eligibility score cubed			0.000 (0.000)	
Controls for household characteristics	No	No	No	Yes
No. of observations	**2,874**	**2,874**	**2,874**	**2,860**

Standard errors in parenthesis.

Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level.

TABLE 4.4

PLACEBO TESTS FOR PREVENTIVE HEALTH VISITS FOR ELDERLY
Dependent Variable: Number of Preventive Health Care Visits in Past 6 Months for Elderly

	Specification			
	(1)	(2)	(3)	(4)
Participant group	-0.041 (0.134)	0.033 (0.191)	0.034 (0.191)	0.018 (0.145)
Eligibility score	0.006 (0.011)	-2.217 (4.007)	0.000 (0.000)	-0.002 (0.011)
Eligibility score squared		0.001 (0.002)	-0.001 (0.002)	
Eligibility score cubed			0.000 (0.000)	
Controls for household characteristics	No	No	No	Yes
No. of observations	**1,205**	**1,205**	**1,205**	**1,199**

Standard errors in parenthesis

Regressions are run at the individual level. Huber-White Standard errors are used to account for within-family correlations.

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level

C. CONCLUSIONS

The results from the placebo tests lend support to using a regression discontinuity design for estimating PATH's impacts. In general, the coefficient in the participant group indicator is not statistically significant. It is not statistically significant for any of the regression specifications involving school attendance (in the reference period and in the typical period) or for health visits for the elderly. It is only statistically significant at the 10 percent level for two regressions in which the dependent variable is health visits for children 0-6 years old (specifications 2 and 3).

Furthermore, these tests suggest that it is reasonable to use regression specification 4 when estimating the impacts of PATH. The coefficients on score squared and score cubed are statistically insignificant in all specifications, which lead us to believe that a linear specification of the score in the impact estimation regressions is a reasonable way to proceed.

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APPENDIX 5

ROBUSTNESS ANALYSIS

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In any empirical study, it is important to verify the extent to which the results are robust to the different specifications of the regression equations and to different features of the research design. This appendix, which verifies the robustness of the impact estimates presented in Chapters V and VI, generally finds that the results are fairly robust.

A. RESTRICTING PARTICIPANT GROUP SCORES

As described in Chapter V, the eligibility score spans a much wider range for the participant group (1,017 to 1,035) than for the comparison group (1,035 to 1,037). This section reports on impact estimates for which both regression specification 4 was used and the participant group was restricted to households with eligibility scores above a certain threshold (1,025 for the Table 5.1 and 1,030 for Table 5.2).[5] The impact estimates continue to be positive and statistically significant for school attendance and for preventive health visits for children, and statistically insignificant for the elderly. The magnitude of the impact estimates tends to be at least as large, and often much larger than, the ones in the regressions in which the participant group was not restricted. Restricting the participant group to households with high eligibility scores this supports, and perhaps even strengthens, the main impact estimates presented in Chapters V and VI.

[5] It is important to note that by imposing this restriction, the statistical power of the design diminishes (since the sample size used in each regression decreases).

TABLE 5.1

IMPACT ESTIMATES RESTRICTING PARTICIPANT GROUP
TO HOUSEHOLDS WITH SCORE GREATER THAN 1,025

	School Attendance		Attendance at Health Centres	
	Reference Period	Typical Period	Children 0-6 years	Elderly
Participant group	0.736*** (0.282)	1.064*** (0.277)	0.309*** (0.111)	-0.134 (0.325)
Eligibility score	0.046 (0.036)	0.090 (0.039)	0.006 (0.015)	-0.020 (0.051)
No. of observations	**5,346**	**5,343**	**2,156**	**882**

Standard errors in parenthesis.

Regressions are run at the individual level. Huber-White standard errors were used to account for within-family correlations.

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level.

TABLE 5.2

IMPACT ESTIMATES RESTRICTING PARTICIPANT GROUP
TO HOUSEHOLDS WITH SCORE GREATER THAN 1,030

	School Attendance		Attendance at Health Centres	
	Reference Period	Typical Period	Children 0-6 years	Elderly
Participant group	1.036 (0.348)	1.313 (0.337)	0.249* (0.145)	-0.122 (0.423)
Eligibility score	0.148 (0.077)	0.175 (0.076)	-0.013 (0.036)	-0.027 (0.094)
No. of observations	**4,382**	**4,373**	**1,707**	**724**

Standard errors in parenthesis.

Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level.

B. RESTRICTING APPLICATION DATES

As described in Chapter V, the participant group tended to have applied later to the program than the comparison group. We report here impact estimates using regression specification 4 and restricting the sample to include only households with application dates earlier than January 1, 2003. In this way, the participant and comparison group members that enter the impact estimations have similar application dates. The impact estimates continue to be consistent with our main impact estimates (i.e. positive and statistically significant impact for school attendance and preventive health visits for children, and statistically insignificant for preventive health visits for the elderly).[6]

TABLE 5.3

IMPACT ESTIMATES RESTRICTING TO APPLICANTS WHO APPLIED IN 2002

	School Attendance		Attendance to Health centres	
	Reference Period	Typical Period	Children 0-6 years	Elderly
Participant group	0.482**	0.717***	0.353***	-0.067
	0.241	0.232	0.093	0.218
Eligibility score	0.002	0.026	0.014	-0.017
	0.020	0.020	0.008	0.021
No. of observations	**5,858**	**5,850**	**2,333**	**975**

Standard errors in parenthesis.

Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level.

[6] We also estimated regression equations without controlling for application date, and continue to obtain results that are consistent with our main impact estimates.

C. QUALITY OF MATCHES

Some of the regression specifications used to estimate the impact estimates presented in Chapters V and VI include the baseline version of the outcome as the explanatory variable. To perform these regressions, we had to match observations in the follow-up data with observations in the baseline data. While it was relatively easy to match household-level information because of the presence of a consistent household ID variable in both data sources, matching at the level of the individual proved much more challenging. We used an algorithm to match at the individual level, and in this section, we explore how sensitive our estimates are to different ways of dealing with the matches that resulted from this algorithm. About 80 percent of the matches were deemed to be "high quality," which means that we were reasonably sure that the person in the follow-up survey was the same as the person being matched to in the baseline survey.

We explored several ways of dealing with the matches at the stage of estimation and concluded that the main impact estimates are robust to different scenarios. In the first scenario, we restricted our sample to high-quality matches. In the second scenario, we used all matches but added in the regressions the binary explanatory variables indicating the quality of match. In the third scenario, we used a household average baseline measure instead of an individual one.

APPENDIX 6

IMPACT ESTIMATES ON OTHER OUTCOMES

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This appendix presents impact estimates on outcomes other than school attendance and preventive health visits; the estimates were derived by using the same regression specifications used to estimate impacts on our key outcomes. The analysis here confirms the general pattern of the results of the tabular analysis presented in Chapters V and VI. While PATH has raised the rate of school attendance and preventive health visits, there is no evidence that it affected longer-term outcomes such as grades, advancement to the next grade, or health care status. This finding is subject to the caveats presented in more detail in Chapter VIII.

Since many of the outcomes examined here are discrete variables that take three or more values, we recoded some of these variables to make them binary. For example, we used a binary variable to indicate whether a household had reported better or much better grades relative to the previous year. We then used a linear probability model in which the dependent variable is the recoded binary variable, and the explanatory variables are the same as those used to estimate impacts on school attendance and preventive health visits.

While models such as the ordered multinomial probit or logit are better for examining regressions using the kind of outcomes examined here, it is complicated to estimate these models in the context of fixed effects and clustered standard errors, and the linear probability model provides a reasonable approximation.

TABLE 6.1

IMPACT ESTIMATES ON OTHER OUTCOMES

Outcomes	Impact Estimate
Education	
Proportion who reported advancing to the next grade	0.004 (0.010)
Proportion who reported grades better or much better than previous year	-0.024 (0.031)
Proportion who reported grades worse or much worse than previous year	-0.023 (0.155)
Proportion of children performing work activities towards household maintenance	-0.001 (0.007)
Health (restricted to children 0-6 years old)	
Proportion who went to preventive visits more frequently than in the previous year	0.032 (0.021)
Proportion reporting worse or much worse health status than that of previous year	0.023 (0.032)
Proportion reporting better or much better health status than that of previous year	-0.002 (0.014)

Standard errors in parenthesis.

Regressions were run at the individual level. Huber-White standard errors were used to account for within-family correlations.

*/**/*** Coefficient statistically significant at the 10%/5%/1% significance level.