The Impacts of School Management Reforms in Madagascar: Do the Impacts Vary by Teacher Type?

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Abstract

This paper exploits a recently implemented randomised control trial in Madagascar that focused on management reforms. It investigates whether the impact of the reforms varies by the type of teacher. This is an important issue because Madagascar, like many other developing countries, has recently hired a large number of contract or temporary teachers, who have less training but may be motivated to work harder in order to have their contracts renewed. The management reforms did not have any impact on student test scores. This lack of an impact holds for all types of teachers. It may be that two years is not enough time for the program to have had a measurable impact, but it is also possible that the program is ineffective, at least in the context of Madagascar's educational system.

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I. Introduction

Most economists agree that an educated population is necessary for sustained economic growth. While economists and other researchers have had some success in understanding what policies encourage parents to send their children to school, they have had less success in understanding what policies and programs increase learning among students who are in school (Glewwe and Kremer, 2006). In recent years, many economists have used randomised control trials to try to estimate the impact of specific policies or programs on student learning. This paper adds to that literature by examining the impact of a school management program in Madagascar, and how those impacts vary by the type of teacher.

There is ample evidence that students' academic performance is very low in many developing countries (Glewwe and Kremer, 2006; Hanushek and Woessmann, 2008). While earlier research focused on school and teacher characteristics, such as availability of textbooks and teachers' levels of education, more recent research examines school organization and management. This paper exploits a recently implemented randomised control trial in Madagascar that focused on management reforms. While others have recently estimated the impact of these reforms (Lassibille et. al, 2010), this paper goes further by investigating whether the impact of the reforms varies by the type of teacher. This is an important issue because Madagascar, like many other developing countries, has recently hired a large number of contract or temporary teachers, who have less training but may be motivated to work harder in order to have their contracts renewed. More specifically, this paper examines whether there is heterogeneity in treatment effects by estimating separate effects for different types of teachers: regular (civil service) teachers, contract teachers, and student teachers.

Summarizing the results, of the three variants of the management reforms – district level, sub-district level and school level -- none had any impact on student test scores. Note, however, that many of the estimates are not very precise. Turning to the relative performance of the three interventions, no significant differences were found between the district level and sub-district level interventions. The school level intervention seems to have had larger impacts on test scores in the first year of the program, relative to the district level intervention. Similar results hold for the school level intervention relative to the sub-district level intervention.

The remainder of this paper is organised as follows. The next section briefly reviews the literature on the impact of school management reforms in developing countries. Section III describes Madagascar's education system, and the recently enacted school management reforms. The following section presents the data and the methodology used. The estimation results are shown in Section V, and a final section concludes and provides suggestions for future research.

II. Literature Review

In recent years, policymakers in poor countries, with the help of their development partners (international and bilateral development agencies), have improved educational opportunities for millions of children in those countries. Primary school enrolment rates have increased dramatically in most developing countries since the Education for All initiative was launched in 1990. For example, in Sub-Saharan Africa the average primary net enrolment rate increased from 54% to 70% between 1999 and 2006, and in South and West Asia it rose from 75% to 86% (UNESCO, 2009). Yet the *quality* of education remains a serious concern. Some of the reasons put forward to explain the meagre outcomes are poor school management, poor infrastructure, low teacher quality and lack of teachers (the recent increases in enrolment have raised pupil-teacher ratios in many countries).

To reduce the recent shortage of teachers, many developing countries have hired contract teachers. Another recent policy concerns school management; local accountability has been proposed to improve education quality (World Bank, 2004). This paper focuses on both of these issues for one country: Madagascar.

In recent years, many developing countries, including Madagascar, have increased enrolment rates and in some cases student performance by hiring contract teachers, who typically have less education and training than regular, civil service teachers (Duflo et al., 2007). These teachers are paid much lower salaries than civil service teachers, for example 40% of regular teachers' salaries in Togo (De Laat and Vegas, 2005), and have little or no "security" that assures them a permanent job. Another common characteristic of contract teachers is that they are often hired from the local community.

There is much debate about the quality of contract teachers; opponents argue that they are unqualified, but advocates claim that they exert greater effort because of their tenuous job status (their contracts are usually for only one year, and they have no employment protections). A relatively new literature on contract teachers has investigated the impact of hiring such teachers on schooling outcomes. De Laat and Vegas (2005) found that, controlling for teacher education and experience, students of regular teachers in Togo scored higher on tests than students of contract teachers. Relative to civil service teachers of any experience level, experienced contract teachers' students' normalised test scores are lower by 0.423, and for inexperienced contract teachers they are lower by 0.087. When teacher education and experience are not controlled for, there is no significant difference in performance between the two types of teachers. Since contract teachers in Togo are more educated than regular teachers, and given the strong positive correlation between student performance and teacher's education level, this suggests that more

educated contract teachers exert lower effort, but due to their higher teaching skills their students do as well as the students of regular teachers. Duflo et. al. (2007) found that hiring of contract teachers combined with class size reductions significantly increased students' test scores in Kenya. In India, contract teachers are hired from the local community and are paid only a small fraction of regular teacher salaries; Muralidharan and Sundararaman (2010) find that contract teachers are at least as effective as regular civil service teachers in raising student learning. Finally, Bettinger and Long (2006) examined the impact of the type of instructor on U.S. students' interest in college classes. They found that adjunct professors and graduate assistants negatively affect students' interest in the humanities but positively affect them in some technical and professional fields. Although their paper focuses on college students, as opposed to primary school students, their finding of the importance of the type of instructor highlights teacher type as a key variable in predicting students' performance.

With regard to school management, centralised school systems are often thought to be of high cost and low quality. To remedy that, decentralised (locally controlled) systems have been implemented in many countries, such as El Salvador and Venezuela. Jimenez and Sawada (1999) analyzed the effectiveness of El Salvador's community-managed (EDUCO) schools. The community participates in hiring and firing teachers, purchasing supplies, and maintenance in EDUCO schools. Community and parental involvement appeared to reduce student absenteeism and increase their language test scores. Allcott and Ortega (2009) analyzed the performance of decentralised (*Fe y Alegría*) schools in Venezuela. They found that students in *Fe y Alegría* schools, which are private Catholic schools, perform better on tests than public school students, and they argue that the better performance of *Fe y Alegría* schools is due to their decentralised nature.

In September 2005, with the assistance of the World Bank, Madagascar introduced the AGEMAD (*Amélioration de la Gestion de l'Education àMadagascar*) program, which was designed to improve primary school management.[1] To facilitate evaluation of the program's impacts on education outcomes, it was implemented as a randomised control trial. Thus far two evaluations have been conducted of the AGEMAD program. First, a World Bank report (2008) on all aspects of the program found no significant impact on teacher absenteeism. It also found that, for schools in groups that received the district and sub-district level interventions, no significant impacts were found in either the proportion of teachers that use all of the "essential tools"[2] or the proportion of schools that are considered well managed. However, comparisons of the *school level* intervention to the control group revealed significant differences in the proportion of teachers that use all of the "essential tools" (63% versus 42%) and the proportion of schools that are considered "well managed" (37 % versus 15%).[3] Similar results were found for student dropout and repetition rates. Statistically significant differences were also found between the school level intervention and the district and sub-district level interventions for Mathematics, Malagasy, and overall test scores.

In a second study, Lassibille et al. (2010) examined the impact of the program (described in more detail in Section III) on primary education management and outcomes. They found little evidence that district and sub-district level interventions improved school management. In contrast, they found that combining district and sub-district interventions with school level interventions raised outcomes; teachers' behaviour improved (in terms of teachers' lesson planning and frequency of student evaluation), and students' mathematics and Malagasy test scores increased by 0.1 standard deviations.

Neither of these studies examined whether the AGEMAD program's impacts on students' test scores varied by the type of teacher. Yet the management reforms recently enacted in Madagascar could have different effects for different types of teachers. For example, the reforms could increase the incentives and/or motivation of regular teachers, who currently face few incentives. On the other hand, the incentives already in place for contract teachers may motivate them to take advantage of the opportunities provided by the management reforms since doing so may increase their performance and thus their job security.

III. Education in Madagascar, and the AGEMAD Program

This paper evaluates the impact of the AGEMAD program on student learning in Madagascar, and whether that impact varies by the type of teacher. The AGEMAD program is a management system designed to improve the management and performance of primary education in Madagascar. The long-term plan is to implement this program nationwide. Before doing so, AGEMAD was implemented in a representative sample of school districts (called CISCOs, of which there are 111 in Madagascar). To obtain unbiased estimates of the impacts of AGEMAD on various educational outcomes, a randomised control trial was implemented.

A. Madagascar's Education System. This subsection describes Madagascar's system of education. That system consists of 5 grades in primary school, 4 grades in lower secondary school, 3 grades in upper secondary school, and several types of post-secondary education. Because AGEMAD is implemented only in primary schools, the focus is on those schools.

In 2005, the gross primary enrolment rate in Madagascar was 123.4%. The primary gross enrolment rate is defined as the total number of pupils enrolled in primary school, regardless of age, divided by the population in the theoretical age group for primary education. In recent years,

Madagascar has attempted to increase primary enrolment rapidly by hiring large numbers of new teachers. Almost all of the newly hired teachers were contract teachers. Table 1 shows trends in the numbers of primary schools and teachers in Madagascar from 1997-98 to 2005-06. FRAM is the acronym for *Fikambanan'ny Ray Amandrenin'ny Mpianatra*, which are parent-teacher associations that hire contract teachers, called FRAM teachers. The percentage of teachers who are FRAM contract teachers rose from 5% in 1997-98 to 48.5% in 2005-06.

The rest of this subsection describes the different types of teachers, and explains why almost all recently hired teachers were contract teachers. There are seven types of primary school teachers in Madagascar: regular civil servants (*fonctionnaire*), regular contract teachers (*contractuel*), contract teachers hired and paid by the community (*FRAM non subventionné par l'Etat*), contract teachers hired by the community but paid by the government (*FRAM subventionné par l'Etat*), teachers that have just graduated from teacher training (*sortant CRINFP*), student teachers (*Elève-maître*), and other (*autres*, which includes teachers paid by NGOs and teachers in private schools). Their characteristics are summarised in Table 2.

Regular civil servant teachers ("*fonctionnaires*") and are paid on a 12-month basis by the Ministry of Education. Their employment is stable and they have many years of experience; in the AGEMAD data (described below) *fonctionnaires* have, on average, more than 20 years of experience. They constitute 52.6% of the teachers in the sample.

Contract teachers are about 44.7% of all primary school teachers in Madagascar in the data. There are two groups of contract teachers: "regular" contract teachers ("*contractuels*") and FRAM contract teachers (which are further divided into those paid and those not paid by the Ministry or Education). Regular contract teachers (*contractuels*) are directly hired by the Ministry of Education; they are about 8% of the teachers in the AGEMAD data. In contrast,

FRAM contract teachers were initially hired and paid by local parent-teacher associations, but since 2003 most have been paid by the Ministry of Education (though they are still hired by the FRAMs). They represent about one third of the teachers in the data. For the school year 2006-07, their monthly salary was about 60,000-70,000 Ariary Mg ($31-36), versus an average of 189,000 Ariary Mg for regular civil service teachers. Before 2006, FRAM teachers were paid only for 9 months per year, while regular teachers were paid for all 12 months. Yet starting in the school year 2006-2007 FRAM teachers have also been paid for 12 months. Although they are paid less, they are often more qualified than regular teachers; for example, about 21% have a high school diploma, versus 8% for regular teachers. Indeed, they are even slightly more educated than the sub-district heads (18% of whom have a high school diploma) who are supposed to train and guide them (World Bank, 2008).

The "sortant CRINFP" teachers are recent graduates from the pedagogical training institute (*Centre Régional de l'Institut National de Formation Pédagogique*). They are only 0.8% of the teachers in the data. Student teachers *(Elève-maître*) are teachers who are still in training; they constitute only 1.8% of teachers in the data. The '*Autres*" category is made of teachers paid by NGOs and other types of teachers; they are only 0.3 % of teachers in the data.

B. Description of the AGEMAD Program. The AGEMAD program was first implemented in Madagascar's primary schools in September of 2005. It is designed to streamline the administration of the system (direct objectives) and improve student learning (indirect objectives). More specifically, the AGEMAD program has three (direct) objectives: (1) Explain to district heads, sub-district heads, school principals, and teachers their responsibilities, and provide them with materials and procedures to accomplish their tasks; (2) Promote supervision and follow-up at key positions in the administrative hierarchy; and (3) Facilitate school-

community interactions and promote accountability for results through the use of school report cards.

The AGEMAD program consists of three distinct interventions. Each intervention provides "tools", that is methods and materials, that were designed to help various employees of the Ministry of Education (district heads, sub-district heads, school principals, and teachers) do their work and, ultimately, improve educational outcomes. The first intervention consisted of providing CISCO (district) heads with tools to accomplish their tasks, and training on the use of these tools. Other CISCO level administrative staff (financial officers, pedagogical officers, and programming officers) also received training on the AGEMAD program. Tools for CISCO heads included: a teacher transfer master sheet, a pedagogical supplies form, and a class observation grid. This intervention will be referred to as the CISCO level intervention.

The second intervention was implemented at the sub-district (ZAP) level. (A typical CISCO has about 10 to 15 ZAPs.) It consisted of providing operational tools to ZAP (sub-district) heads that were designed to help them monitor and support their schools. In contrast to the CISCO level intervention, only the ZAP heads received the AGEMAD tools and training; no other ZAP level staff participated. Tools provided at the ZAP level included: pedagogical supervision and support forms, and report cards on the performance of, and the resources for, each school in the ZAP. This intervention will be referred to as the ZAP level intervention. Note that this second intervention was implemented only in ZAPs that belonged to CISCOs in which the CISCO intervention was implemented. Thus any ZAP that has the ZAP level intervention also has the CISCO level intervention.

The third AGEMAD intervention was at the school level. It consisted of providing educational and administrative tools to teachers (e.g. lesson planning forms, records of student

attendance and learning, and reports to parents and school directors) and to school principals (directors), who were provided with tools to better manage their schools (e.g. attendance registers to monitor teachers' absence, summaries of student test scores, and community meeting forms). In addition, school meetings were held to discuss school report cards, in order to encourage parental and community involvement in monitoring school quality. This intervention will be called the school level intervention. Note that it was implemented only for schools that belonged to ZAPs that were selected to implement the ZAP level intervention (and thus only for schools in CISCOs that implemented the CISCO-level intervention). Thus any school that implemented the school level intervention is always in a ZAP that implemented the ZAP level intervention and in a CISCO that implemented the CISCO level intervention.

C. Design of the Experiment. The three interventions described above were randomly assigned to different schools as follows. First, 30 CISCOs (school districts) were randomly assigned so that 15 received the CISCO-level intervention while the other 15 did not receive it. Note that these 30 CISCOs were not randomly selected from Madagascar's 111 CISCOs; rather they were selected based on their similarity and ease of access. Second, within each of the 15 treated CISCOs all ZAPs were randomly assigned to receive (or not receive) the ZAP-level intervention. More specifically, from the 259 ZAPs in the 15 treated CISCOs, 89 were randomly selected to receive the ZAP-level intervention, and the remaining 170 did not receive it (of these 170, data were collected from only 84). Within the 89 ZAPs that received both the CISCO- and ZAP-level interventions there are 739 schools. From these schools, 303 were randomly selected to receive a school-level intervention, while the other 436 did not receive it (of these, data were collected from only 303). This randomization scheme is shown in Figure 1.

Overall, this randomization scheme created one control group and three treatment groups:

- *Control group* - 1,721 schools (of which data were collected from 303) in the 15 non-treated CISCOs.
- *Treatment 1* - 1,314 schools (of which data were collected from 303) in the 15 treated CISCOs that received only the CISCO-level intervention. Henceforth, these schools will be referred to as treatment 1 schools, or CISCO-AGEMAD schools.
- *Treatment 2* - 436 schools (of which data were collected from 303) in the 15 treated CISCOs and the 89 treated ZAPs that received both the CISCO- and ZAP-level interventions, but not the school-level intervention because they were not selected for that intervention. Henceforth, these schools are referred to as the treatment 2 schools, or the ZAP-AGEMAD schools.
- *Treatment 3* - 303 schools in the 15 treated CISCOs and the 89 treated ZAPs that received all three levels of treatment (CISCO, ZAP and school) because they were selected to receive the school-level interventions. Henceforth, these schools will be referred to as the treatment 3 schools, or the FULL-AGEMAD schools.

Comparing these four types of schools allows one to answer several different questions about the AGEMAD program. Comparison of Treatment 1 schools with the control group schools estimates the effect of a "cascade" or "training-of-trainers" model for implementing the program. Comparing Treatment 2 schools to the control group schools gives the effect of a more intensive "cascade" model (both district and sub-district interventions). Comparing Treatment 3 schools to the control group schools allows one to identify the maximum possible impact of the full AGEMAD program, with CISCO, ZAP, and school-level interventions.

In addition, one can estimate the relative effectiveness of different interventions. First, comparing Treatment 3 schools to Treatment 2 schools reveals whether adding the school level

intervention to schools that already have the district (CISCO) and sub-district (ZAP) interventions leads to further improvements in student outcomes. Second, comparison of Treatment 2 schools to Treatment 1 schools shows whether the more intensive "cascade" or "training-of-trainers" intervention, the one implemented at the CISCO and the ZAP levels, has a significantly stronger impact than implementing only the CISCO level intervention.

To ensure that the two groups of 15 CISCOs are comparable, the assignment of the 30 CISCOs to the treatment and control groups was done after stratifying by region, repetition rate, whether or not the *Approche par les Compétences* (APC, a pedagogic innovation project) was implemented, and the presence of Non-Governmental Organizations (NGOs). There are 573 ZAPs and 6,488 schools in these 30 CISCOs. Some ZAPs and schools that are located in remote areas were excluded (107 ZAPs and 2,714 schools) from the experiment because of difficulty of access. As a result, the 30 CISCOs have 466 participating ZAPs and 3,774 participating schools, of which 2,053 schools are in the 15 treated CISCOs and 1,721 are in the 15 control CISCOs.

ZAPs and schools in the treated CISCOs were randomly assigned to treatments 2 and 3 after stratifying by school size and repetition rate. This ensures the similarity of treated groups at the beginning of the experiment and reduces the variance of the estimated impact of the program. The final sample for data analysis was obtained by randomly selecting 303 schools from each of the four intervention groups, which yields a total of 1,212 schools. Program implementation and data collection were conducted from September 2005 to June 2007.

IV. Data and Methodology

This section describes the data used in the analysis, and the empirical methods to estimate the impacts of the AGEMAD program.

A. Data. The main dependent variables in this study are test scores in three subjects, French, Malagasy, and Mathematics, that were given to third grade students in 2005-2006 and fourth grade students in 2006-2007. Including all three subjects, there were 97 questions for the first year test and 98 for the second year test. The pupils tested during the first year were given the fourth grade test during the second year regardless of repetition status. Some pupils were not present in class on the day of the second year's test (due to absence on that day, switching schools, or dropping out). Among the absentees, five pupils per school were chosen at random and tracked down for testing. However, the second year's test was not given to 3,431 pupils who could not be found. The same tests were administered to teachers in 2005-06 and 2006-07 (see table 2.3 of World Bank, 2008) to verify whether those who are supposed to grade the students' tests know enough to do so (a check of teacher quality/knowledge). The final sample sizes were 24,579 third graders in 2005-06 and 22,038 fourth graders (or third graders who had repeated) in 2006-07.

Table 3 provides some basic information on the different types of teachers, using the 2006-07 data. Regular civil servant teachers ("*fonctionnaires*") comprise 27.8% of teachers. They are relatively old (average age of 48.7) and have about 25 years of experience. Their age and experience reflect the fact that almost all teachers hired in the past decade have been FRAM teachers, as explained above. While few (8.1%) have a high school diploma, the vast majority (85.7%) have pedagogical training. Finally, their salaries are relatively high. *Contractuel* teachers are relative rare (7.9% of all teachers) but are very similar to regular civil service teachers. In contrast, the two types of FRAM teachers, which constitute 52% of all teachers, are much younger (in their early 30s) and so have much less experience (4-6 years), more likely to have a high school diploma (21%), but much less likely to have formal pedagogical training.

Higher education levels in the general population, relative to 20 years ago, may explain why FRAM teachers are more likely to have a high school diploma. Finally, their salaries are less than half of those of regular civil service and contract teachers.

For purposes of analysis, these seven types of teachers can be combined into four basic groups. First, the *sortant CRINF*' and *Elève-maître* categories can be combined to form a new category, '*sortant_élève-maître*'; the first category represents new graduates from the teacher's training centres and the second represents teachers still in training, so their level of experience is similar. Second, the two FRAM categories can be combined into a single category because they differ only by who pays their salaries. Finally, the *autres* (others) category was dropped from the sample because it combines many different, often not clearly defined, types of teachers that constitute only 0.3% of the total sample.

B. Methodology. This subsection explains the procedure used to estimate the impact of the three AGEMAD interventions on students' test scores in Madagascar. Given the randomised assignment of CISCOs, ZAPs and schools to treatment and control groups, many econometric problems that arise in non-experimental data are avoided. Yet there are some issues that require discussion, and knowledge of the estimation procedure is necessary to interpret the results.

The AGEMAD data have scores for grade 3 students on tests taken in February, 2006, which is in the middle of the 2005-06 school year. These tests were taken five months *after* the program was launched in September of 2005, so analysis of the February 2006 data estimates the impact of the three interventions after they had been in place for five months. The data also have scores for grade 4 students on tests given in June of 2007, near the end of the 2006-07 school year, at which time the program had been operating for two school years. For simplicity, the 2005-06 and 2006-07 school years are called "year 1" and "year 2" of the program, respectively.

Recall that the three "treatments" were the CISCO level, the CISCO and ZAP level, and the CISCO, ZAP and school level interventions. Given random assignment to the treatment and control groups, the following regression in either year 1 (t=1) or year 2 (t=2) provides the simplest method to estimate the impacts of the three separate programs (denoted by P_1, P_2 and P_3) on the test scores of student i at time t (denoted by T_{it}):

$$T_{it} = \kappa_t + \alpha_t P_{1i} + \beta_t P_{2i} + \gamma_t P_{3i} + u_{it}, \quad t= 1, 2 \qquad (1)$$

where P_{1i} is a dummy variable indicating whether student i is in a school that received the CISCO level treatment (Treatment 1), and P_{2i} and P_{3i} are similarly defined for schools that received the ZAP (Treatment 2) and school (Treatment 3) level interventions, respectively.

In this simple regression, κ_t is the average test scores of students in the control schools at time t (imposing the usual normalisation that $E[u_{it}] = 0$), and α_t, β_t and γ_t are the average impacts of the three programs on students' test scores. If t = 1, this regression estimates these three impacts after only five months (from September 2005 to February 2006), while if t = 2 it estimates these impacts after two full school years (September 2005 to June 2007).

It is likely that most of the impact of the program by time period 2 (i.e., by June 2007) would have taken place in the 16 months between time periods 1 and 2 (between February 2006 and June 2007), rather than during the first 5 months of the intervention (from September 2005 to February 2006). For example, there may have been little impact in the first few months because it probably took some time for education officials to implement and adjust to the interventions. In this case, it is useful to estimate the impact of the program on "learning gains" between time periods 1 and 2, because such estimates of program effects may be more precise

than those of either year 1 or year 2. There are two ways to estimate learning gains over time. First, one could estimate the impact of the programs on the "change" in test scores over time:

$$T_{i2} - T_{i1} = (\kappa_2 - \kappa_1) + (\alpha_2 - \alpha_1) P_{1i} + (\beta_2 - \beta_1) P_{2i} + (\gamma_2 - \gamma_1) P_{3i} + (u_{i2} - u_{i1}) \qquad (2)$$

In this expression, the constant term $(\kappa_2 - \kappa_1)$ is the average change in test scores for students in the schools that did not participate in any intervention. For each intervention program variable (P_{1i}, P_{2i} and P_{3i}), the associated coefficient (e.g. $(\alpha_2 - \alpha_1)$ for the variable P_{1i}) shows how that intervention added to (or perhaps subtracted from) the average change in test scores experienced by the control school students. Note that if any intervention's impact at time period 1, that is after the program has been in place for five months, is close to zero, then equation (2) provides an alternative estimate of α_2, β_2 and γ_2 (since α_1, β_1 and γ_1 are close to zero).

The second way to estimate learning gains moves T_{i1} in (2) to the right of that equation:

$$T_{i2} = (\kappa_2 - \kappa_1) + (\alpha_2 - \alpha_1) P_{1i} + (\beta_2 - \beta_1) P_{2i} + (\gamma_2 - \gamma_1) P_{3i} + \tau_1 T_{i1} + (u_{i2} - u_{i1}) \qquad (3)$$

This is more flexible than equation (2) because the τ coefficient may be different from one. Yet this estimate of τ_1 is almost certainly biased since T_{i1} is correlated with u_{i1}*, but this does not cause bias in estimates of $(\alpha_2 - \alpha_1)$, $(\beta_2 - \beta_1)$ or $(\gamma_2 - \gamma_1)$ because all three P variables are, by random assignment, uncorrelated with all other variables (T_{i1}, u_{i2} and u_{i1}).[4]

Another possible way to increase statistical efficiency in the estimates of equations (1), (2) and (3) is to add other explanatory variables to the above regressions. Denoting those variables by the vector **x**, we have the following modifications of those regressions:

$$T_{it} = \kappa_t + \alpha_t P_{1i} + \beta_t P_{2i} + \gamma_t P_{3i} + \boldsymbol{\delta}_t' \mathbf{x}_{it} + u_{it}', \quad t = 1, 2 \qquad (1')$$

$$T_{i2} - T_{i1} = (\kappa_2 - \kappa_1) + (\alpha_2 - \alpha_1) P_{1i} + (\beta_2 - \beta_1) P_{2i} + (\gamma_2 - \gamma_1) P_{3i} + (\boldsymbol{\delta}_2' \mathbf{x}_{i2} - \boldsymbol{\delta}_1' \mathbf{x}_{i1}) + (u_{i2}' - u_{i1}') \qquad (2')$$

$$T_{i2} = (\kappa_2 - \kappa_1) + (\alpha_2 - \alpha_1)\, P_{1i} + (\beta_2 - \beta_1)\, P_{2i} + (\gamma_2 - \gamma_1)\, P_{3i} + (\boldsymbol{\delta}_2' \mathbf{x}_{i2} - \boldsymbol{\delta}_1' \mathbf{x}_{i1}) + \tau_1 T_{i1} + (u_{i2}' - u_{i1}') \quad (3')$$

The "prime" on the error terms in these regressions indicates that they differ from the error terms in equations (1), (2) and (3). For example, in equation (1′) they are related as follows: $u_{it} = \boldsymbol{\delta}_t' \mathbf{x}_{it} + u_{it}'$. The **x** variables used in the next section are: a dummy variable for male students; child age; mothers' and fathers' education; and wealth. Whether adding these variables increases efficiency is an empirical issue that can be settled only by estimating these regressions.

Finally, given that the goal of this paper is to investigate whether the impacts vary by the type of teacher, all three equations require variables that interact the three program variables with dummy variables for different types of teachers. As explained above, there are four main types: regular (civil service) teachers; FRAM contract teachers; other contract teachers, and student teachers. Using regular teachers as the omitted category, this implies the following regressions to estimate differential impacts by type of teacher:

$$T_{it} = \kappa_t + \alpha_t P_{1i} + \beta_t P_{2i} + \gamma_t P_{3i} + \boldsymbol{\delta}_t' \mathbf{x}_{it} + \pi_{1t} FT_i + \pi_{2t} CT_i + \pi_{3t} ST_i$$
$$+ \theta_{F1t} FT_i \times P_{1i} + \theta_{C1t} CT_i \times P_{1i} + \theta_{S1t} ST_i \times P_{1i} + \theta_{F2t} FT_i \times P_{2i} + \theta_{C2t} CT_i \times P_{2i} + \theta_{S2t} ST_i \times P_{2i}$$
$$+ \theta_{F3t} FT_i \times P_{3i} + \theta_{C3t} CT_i \times P_{3i} + \theta_{S3t} ST_i \times P_{3i} + u_{it}'', \quad t = 1, 2 \qquad (1'')$$

$$T_{i2} - T_{i1} = (\kappa_2 - \kappa_1) + (\alpha_2 - \alpha_1)\, P_{1i} + (\beta_2 - \beta_1)\, P_{2i} + (\gamma_2 - \gamma_1)\, P_{3i} + (\boldsymbol{\delta}_2' \mathbf{x}_{i2} - \boldsymbol{\delta}_1' \mathbf{x}_{i1})$$
$$+ (\pi_{12} - \pi_{11})\, FT_i + (\pi_{22} - \pi_{21})\, CT_i + (\pi_{32} - \pi_{31})\, ST_i$$
$$+ (\theta_{F12} - \theta_{F11})\, FT_i \times P_{1i} + (\theta_{C12} - \theta_{C11})\, CT_i \times P_{1i} + (\theta_{S12} - \theta_{S11})\, ST_i \times P_{1i}$$
$$+ (\theta_{F22} - \theta_{F21})\, FT_i \times P_{2i} + (\theta_{C22} - \theta_{C21})\, CT_i \times P_{2i} + (\theta_{S22} - \theta_{S21})\, ST_i \times P_{2i}$$
$$+ (\theta_{F32} - \theta_{F31})\, FT_i \times P_{3i} + (\theta_{C32} - \theta_{C31})\, CT_i \times P_{3i} + (\theta_{S32} - \theta_{S31})\, ST_i \times P_{3i} + (u_{i2}'' - u_{i1}'') \qquad (2'')$$

$$T_{i2} = (\kappa_2 - \kappa_1) + (\alpha_2 - \alpha_1)\, P_{1i} + (\beta_2 - \beta_1)\, P_{2i} + (\gamma_2 - \gamma_1)\, P_{3i} + (\boldsymbol{\delta}_2'\mathbf{x}_{i2} - \boldsymbol{\delta}_1'\mathbf{x}_{i1}) + \tau_1 T_{i1}$$

$$+ (\pi_{12} - \pi_{11})\, FT_i + (\pi_{22} - \pi_{21})\, CT_i + (\pi_{32} - \pi_{31})\, ST_i$$

$$+ (\theta_{F12} - \theta_{F11})\, FT_i \times P_{1i} + (\theta_{C12} - \theta_{C11})\, CT_i \times P_{1i} + (\theta_{S12} - \theta_{S11})\, ST_i \times P_{1i}$$

$$+ (\theta_{F22} - \theta_{F21})\, FT_i \times P_{2i} + (\theta_{C22} - \theta_{C21})\, CT_i \times P_{2i} + (\theta_{S22} - \theta_{S21})\, ST_i \times P_{2i}$$

$$+ (\theta_{F32} - \theta_{F31})\, FT_i \times P_{3i} + (\theta_{C32} - \theta_{C31})\, CT_i \times P_{3i} + (\theta_{S32} - \theta_{S31})\, ST_i \times P_{3i} + (u_{i2}'' - u_{i1}'') \qquad (3'')$$

where the θ parameters measure the additional impacts for FRAM (FT), contract (CT) and student teachers (ST) for all three interventions relative to the program impacts on regular (civil service) teachers.

Note that these equations assume that teacher types do not change over time (FT_i, CT_i and ST_i do not have time subscripts). This is generally true, but there are some exceptions. More specifically, in both grade 3 year 1 (2005-06) and grade 4 in year 2 (2006-07) over 90% of schools had the same type of teacher for all students in those grades (mainly because most schools had only one teacher per grade). For these schools, 95% had the same type of teacher in grade 3 in year 1 and grade 4 in year 2. For the other schools (less than 10%), which had multiple types of teachers in grade 3 in year 1 and grade 4 in year 2, we cannot determine whether a given child had the same type of teacher in both years because we do not have data that match individual students to specific teachers in year 1.

A final important point regarding estimation of all of the equations presented in this section is that random assignment of schools to the three interventions assures that the error terms (u_{it}, u_{it}' and u_{it}'') are uncorrelated with the program variables (P_{1i}, P_{2i} and P_{3i}). Thus OLS yields consistent estimates of the α, β and γ parameters in equations (1), (2) and (3), though not of τ in equation (3). Random assignment also assures that OLS estimates of equations (1′), (2′) and (3′) consistently estimate α, β and γ; even though the **x** variables could be correlated with the

error terms, random assignment assures that the program variables are not correlated with the **x** variables, so OLS consistently estimates α, β and γ (though not necessarily the **δ** parameters).

Random assignment also assures that OLS estimates of α, β and γ, *and* the θ parameters (coefficients on the interaction terms), in equations (1″), (2″) and (3″) are consistent even though estimates of the **δ** terms may be inconsistent. To see why, consider (without loss of generality) a simpler version of equation (1″), with only one intervention and only one type of teacher (say, FRAM teachers) other than general civil service teachers:

$$T_{it} = \kappa_t + \alpha_t P_{1i} + \boldsymbol{\delta}_t' \mathbf{x}_{it} + \pi_{1t} FT_i + \theta_{F1t} FT_i \times P_{1i} + u_{it}''', \quad t = 1, 2 \qquad (1''')$$

Random assignment of schools to the program assures that Cov (P_{1i}, u_{it}''') = 0. However, u_{it}''' could be correlated both with FT_i and some of the **x** variables. This can be expressed as:

$$u_{it}''' = \beta_0 + \beta_1 FT_i + \boldsymbol{\beta}_{\mathbf{x}}' \mathbf{x}_{it} + \varepsilon_{it}$$

where ε_{it} is uncorrelated with all variables in equation (1‴). Inserting this expression for u_{it}''' into equation (1‴) yields:

$$T_{it} = \kappa_t + \alpha_t P_{1i} + \boldsymbol{\delta}_t' \mathbf{x}_{it} + \pi_{1t} FT_i + \theta_{F1t} FT_i \times P_{1i} + \beta_0 + \beta_1 FT_i + \boldsymbol{\beta}_{\mathbf{x}}' \mathbf{x}_{it} + \varepsilon_{it}$$

$$= (\kappa_t + \beta_0) + \alpha_t P_{1i} + (\boldsymbol{\delta}_t + \boldsymbol{\beta}_{\mathbf{x}})' \mathbf{x}_{it} + (\pi_{1t} + \beta_1) FT_i + \theta_{F1t} FT_i \times P_{1i} + \varepsilon_{it} \quad t = 1,2 \qquad (1'''')$$

Since ε_{it} is, by definition, uncorrelated with all variables in equation (1‴), OLS estimation will consistently estimate all of the parameters in this expression. That is, consistent estimates can be obtained for α_t and θ_{F1t}, but not for κ_t, $\boldsymbol{\delta}_t$ or π_{1t}.

V. Results

The section presents estimates of the impact of the three AGEMAD interventions on the test scores in Madagascar of students who were in grade 3 in 2005-06 (year 1) and in grade 4 (or in grade 3 if the student repeated) in 2006-07 (year 2), using the regression methods discussed in Section IV. To ease interpretation, all test score variables are transformed to have a mean of 0 and a standard deviation of 1. Finally, to reduce clutter the test score variable in all tables is an average of the three tests (Malagasy, French and Mathematics).[5]

A. Estimates of General Program Impacts. The first set of estimates, presented in Table 4, shows estimates of equations (1), (1′) and (1″) for the test scores in year 1, when the students were in grade 3 and had been exposed to the program for five months. Column 1 presents estimates of equation (1), the estimated impacts for all three interventions after 5 months, without covariates. The estimated impacts are quite small, ranging from -0.05 to 0.11 standard deviations (of the distribution of averaged test scores), and all are statistically insignificant. Note that the size of the standard errors, between 0.11 and 0.12, are somewhat large, and so can detect only program impacts that are 0.2 standard deviations or larger.[6]

Adding student and teacher covariates may increase the precision of the estimated impacts for year 1. This is examined in columns 2 (student covariates only) and 3 (student and teacher covariates) of Table 4.[7] Adding these covariates did not increase precision; instead, the standard errors of the estimated impacts of the three interventions increased slightly in five out of six cases. Note that the (possibly biased) estimates of the student characteristics are plausible; students who are older, have more educated parents, or are from wealthier households all have higher test scores.

The estimates in the last column of Table 4 examine whether the program impacts vary by the type of teacher, the central issue addressed by this paper. The three interventions still have small (between -0.10 and 0.01) and statistically insignificant impacts. These estimates apply to regular (civil service) teachers, the omitted teacher group. Thus they indicate that, after the first five months, none of the three interventions had any discernable impact on the test scores of students of regular teachers.

The remaining rows of Table 4 measure the impacts of the three interventions among students who had the other types of teachers.[8] With one exception, the estimated impacts for other teachers are statistically insignificant; although the impacts (relative to those for regular teachers) of some of the interventions appear to be large (the estimated parameters that are not significant range from -0.17 to 0.30), they are all imprecisely estimated, with standard errors ranging from 0.12 to 0.34. (The most precise estimates are for FRAM teachers, who are much more common than contract teachers or student teachers, but the range of these estimates is also smaller, from -0.17 to 0.14.) The one exception is student teachers (which include very new teachers); the CISCO level treatment appears to have had a strong negative impact on students who were taught by student teachers, reducing their test scores by 0.54 standard deviations of the distribution of test scores, which is significant at the 5% level.

To summarise the results in Table 4, after the first five months there appears to have been little impact of any of the three interventions on students' test scores. The only exception is that the CISCO level intervention appears to have had a negative impact on students taught by student teachers. Yet this result should be interpreted with caution, since 12 parameters are being estimated, and even if the true values of all were zero each has a one in twenty chance of being significant at the 5% level. Also, recall that the year 1 test scores measure the programs'

impacts after only five months, which may not be enough time for any of the programs to have had a large impact.

Next, turn to estimates of the impacts of all three interventions after almost two years. Table 5 presents estimates of equations (1), (1′) and (1″) for the test scores in year 2, when the students were in grade 4 and had been exposed to the programs for nearly two school years. Column 1 presents estimates of equation (1). The estimated impacts are quite small, ranging from -0.01 to 0.11 standard deviations (of the distribution of averaged test scores), and are all statistically insignificant. As in Table 4, the standard errors are somewhat large, between 0.10 and 0.12, and thus can detect only program impacts that are 0.2 standard deviations or larger.

Adding student and teacher covariates does not increase the precision of the estimated impacts in year 2 (see columns 2 and 3 of Table 5). It slightly reduces the point estimates of the impacts of the three interventions (they now range from -0.02 to 0.09), and they are still far from statistically significant. Again, the (possibly biased) estimates of student characteristics are plausible; students with more educated or wealthier parents have higher test scores.

The last column in Table 5 presents estimates that examine whether program impacts vary by the type of teacher. The estimated parameters for all three programs are very small, ranging from -0.02 to 0.07, and all are statistically insignificant. Recall that these estimates apply to regular (civil service) teachers. Thus they indicate that, after 2 years, the impact of all three interventions was very small for students whose main teacher was a regular teacher. The last nine rows of Table 5 assess whether the program impacts were different for the three other types of teachers. As in Table 4, these impacts are imprecisely estimated, with the standard errors ranging from 0.09 to 0.50. The estimates range from -0.29 to 0.43, yet none of them is statistically significant. Note also that, in contrast with the significantly negative result after five

months, students taught by student teachers who received the CISCO level intervention did not have significantly lower test scores (indeed, the point estimate is positive, but not significant).

Overall, the results in Table 5 indicate that two years after the three interventions were implemented, none had any impact on student test scores (combined for all three subjects), and this result does not vary over different types of teachers. These results are similar to those of Lassibille et. al. (2010).

Estimation of equations (2) and (3), and their variants, may provide more precise estimates of program effects than estimation of equation (1) and its variants. Table 6 presents estimates of equations (2), (2′) and (2″). The first column includes no covariates. Comparing the standard errors in that column with the standard errors in the first column of Table 5, there is no general tendency toward more precise estimates, and so the estimated impacts of gains in students test scores from year 1 to year 2 are all insignificant. Adding student and teacher characteristics did not improve statistical precision. Finally, the estimates in the last column are, on average, no more precise than those in the last column of Table 5. While two of the nine coefficients are statistically significant at the 10% level, this is not much more than one would expect from random variation. Also, it is hard to imagine why the ZAP-level program had a negative effect for contract teachers, relative to regular teachers, while the same was not true of the "full" intervention, which included all the (presumably negative) interventions implemented at the ZAP level. Similarly, the large positive (though very imprecisely estimated) impact of the CISCO level treatment for student teachers primarily reflects the unusually negative impact for that combination in year 1, which (as discussed above) is difficult to interpret and may be due to random chance.

Finally, estimates of equations (3), (3′) and (3″) are shown in Table 7. In this case, there is some tendency for more efficient estimation relative to the Table 5 results. For example, in column 1 of Table 5 the standard errors of the estimates of the impact of the interventions on the Year 2 test scores ranged from 0.10 to 0.12, while in column 1 of Table 7 the standard errors of the estimates of the program impacts on the Year 2 test scores, conditional on year 1 test scores, range from 0.09 to 0.10. Yet even with these smaller standard errors the estimated impacts of the three interventions on test scores in year 2, conditional on scores in year 1, are all small (ranging from 0.00 to 0.06) and statistically insignificant. Adding other covariates (columns 2 and 3) does not improve the precision. Regarding the possibility of differential impacts for different types of teachers, only one of the nine interaction terms is statistically significant, and only at the 10% level, which is what one could expect even if the true (interaction) effects were all equal to zero. Overall, the modest gain in precision does not change the general result that none of the interventions has an impact after two years, and that there appear to be no differences across different types of teachers (relative to civil service teachers).

B. Comparing the Relative Effectiveness of Different Interventions. Although none of the interventions appears to have a sizeable impact on test scores, relative to no intervention at all, it is possible that one intervention may outperform another. That is, the impact of one intervention may be slightly negative (e.g. $\alpha_t < 0$ in Equation (1)) while that of another is slightly positive (e.g. $\gamma_t > 0$ in Equation (1)), and although neither is significantly different from zero the *difference* of the two impacts could be statistically significant (e.g. $\gamma_t - \alpha_t$ could be significantly greater than 0). This subsection examines this possibility.

First, consider the relative impacts of the CISCO and ZAP level interventions. Recall that, in the 15 CISCOs randomly assigned to get the CISCO treatment, some of the ZAPs (sub-

districts) were randomly selected to get the ZAP level treatment as well, while the others had only the CISCO level treatment. Thus regressions can be estimated only for these 15 CISCOs (that is, dropping the 15 "control" CISCOs), which is somewhat more flexible but, in theory, may suffer some loss of precision. The results are shown in Table 8. Note that, unlike the results in the previous subsection, in these regressions only half of the sample is used (i.e. only two of the three types of treatment schools, and none of the control schools; cf. Figure 1).

The first and third columns of Table 8 show no significant difference in the impact of the ZAP level intervention, relative to the CISCO level intervention, for both Year 1 and Year 2 scores. Indeed, the two point estimates are very small (-0.03 and 0.02, respectively). Columns (2) and (4) examine whether these relative differences vary by the type of teacher. Although there is more variation in the point estimates (which are all relative to regular teachers), indeed they vary from -0.31 to 0.29, only one of the six (that for FRAM teachers in Year 1) is statistically significant, and only at the 10% level. The three interaction terms in column 2 are not jointly significant (F=1.72 and p=0.16). Overall, there is little evidence of a significant difference between the impacts of the ZAP level and the CISCO level interventions.

Table 9 presents evidence on whether there is a difference between the impacts of the CISCO and ZAP level interventions in *changes* in test scores between Year 1 and Year 2. Columns (1) and (2) show results where the change in test scores is the dependent variable, while columns (3) and (4) present findings when the Year 2 test score is the dependent variable and the Year 1 test score is an explanatory variable. The simplest specifications, with no covariates or interaction terms, show no evidence of a differential impact for the CISCO and ZAP level interventions. Yet when interactions are added there is weak evidence (significant at 10% level) that the impact of the ZAP level intervention is smaller than that of the CISCO level intervention

for student teachers, and these effects are large (-0.63 in column 2 and -0.44 for column 4). While this is consistent with the result in the fourth column of Table 6, where the interaction between the CISCO level intervention and student teachers was large and significant at the 10% level, the effects in both Tables 6 and 9 are very imprecisely estimated. Moreover, the three interaction terms in columns 2 and 4 are not jointly significant ($F = 1.16$ and $p = 0.33$).

Turn next to the relative impacts of the CISCO and school level interventions (Treatment 1 vs. Treatment 3). Recall that the school level intervention combined the CISCO, ZAP and school level interventions. Intuitively, if the ZAP and school level interventions are valuable one would expect a larger impact on student test scores from the school level intervention than from the CISCO level intervention. The results are shown in Tables 10 and 11.

The first column of Table 10 shows that, after 5 months, the school level intervention has a larger impact than the CISCO level intervention on Year 1 test scores. More specifically, the Year 1 test score is 0.13 standard deviations higher in schools exposed to the school level intervention, and this impact is significant at the 5% level. However, this differential effect falls and becomes statistically insignificant after the program has been in place for two years; the point estimate drops somewhat, to 0.11 standard deviations, and is no longer significant even at the 10% level. When covariates and interactions are added (columns 2 and 4) the evidence is somewhat contradictory; in Year 1 students taught by student teachers in schools with the school level intervention seem to do much better than those taught by student teachers in schools with the CISCO level intervention, a large effect (0.63) that is significant at the 5% level, but in Year 2 this effect drops to 0.15 and is insignificant, and instead there is a large (0.52) and statistically significant impact for contract teachers. The three interaction terms in column 2 are not jointly significant ($F=1.93$ and $p=0.13$) but those in column 4 are jointly significant ($F=3.47$and $p=0.02$).

Table 11 presents evidence on whether there is a difference between the impacts of the CISCO and school level interventions in changes in test scores from Year 1 to Year 2. The simplest specifications, in columns 1 and 3, which have no covariates or interaction terms, show no evidence that the school level intervention has a larger impact than the CISCO level intervention on changes in test scores. Indeed, the two point estimates are quite small (-0.02 and 0.05, respectively). The second and fourth columns examine whether these relative differences vary substantially by the type of teacher. As in Table 9, there is more variation in the point estimates (which are all relative to regular teachers) – they vary from -0.47 to 0.40 – but these impacts relative to regular teachers are quite imprecisely estimated, with standard errors ranging from 0.13 to 0.25. Two of six estimated parameters (those for student teachers in the change specification and for contract teachers in the "conditional on Year 1 scores" specification) are statistically significant, but only at the 10% level. The three interaction terms in column 2 are not jointly significant (F=1.76 and p=0.16) while those in column 4 are weakly jointly significant (F=2.44 and p=0.07). These weakly significant effects, while large (-0.47 and 0.40, respectively), are difficult to interpret: why would adding the ZAP and school level interventions to the CISCO level intervention favour contract teachers in one specification but not the other? The same point applies to student teachers, column 2 has a large (-0.47) and significantly negative impact, while column 4 has a much smaller (-0.12) impact that is not at all significant. Overall, the evidence from Tables 10 and 11 does not show a significant difference between the impacts of the CISCO level intervention and the school level intervention.

Finally, consider the relative impacts of the ZAP level and the school level interventions (Treatments 2 and 3). Both provided "tools" at the CISCO and ZAP levels, but only the latter provided assistance at the school level. These results are shown in Tables 12 and 13.

The first column of Table 12 shows that, after 5 months of the program, the school level intervention has a larger impact than the ZAP level intervention on Year 1 test scores; the Year 1 test score is 0.16 standard deviations higher in schools exposed to the school level intervention, and this difference is significant at the 5% level. Yet this differential effect becomes smaller (0.10) and statistically insignificant after the interventions have been in place for two years. It is possible that the ZAP level intervention had some initial start-up problems that had a small negative effect on test scores, which were later resolved, but we do not have any evidence to support this conjecture.

When covariates and interactions are added (columns 2 and 4) there are no statistically significant interaction effects in Year 1 (column 2). Yet there is one significant interaction in Year 2; students in schools with contract teachers and the school level intervention had much higher test scores (0.45 standard deviations) than students in schools with contract teachers and the ZAP level intervention. This impact is difficult to interpret. The three interaction terms in column 2 are not jointly significant (F=0.71 and p=0.55) while those of column 4 are marginally jointly significant (F=2.77 and p=0.05).

Table 13 examines whether there is a difference between the impacts of the ZAP and school level interventions in changes in test scores between Years 1 and 2. The specifications in columns 1 and 3, which have no covariates or interaction terms, show no evidence that the school level intervention has a larger impact than the ZAP level intervention on changes in test scores; indeed, the two point estimates are fairly small (-0.06 and 0.03, respectively). The second and fourth columns examine whether there are differences by the type of teacher. Only one of the six estimated parameters (that for contract teachers in column 4) is statistically significant, and only at the 10% level. This is consistent with the result in column 4 of Table 12,

and the same caveats apply. The three interaction terms in column 2 are not jointly significant (F=0.90 and p=0.45) and the same holds for the three interaction terms in column 4 (F=1.90 and p=0.14).

VI. Conclusion

This paper has examined the impact of the AGEMAD program on student learning in Madagascar, focusing on whether the impacts vary by type of teacher. Overall, after two years the program appears to have had little or no impact in terms of either general or teacher specific results. Test scores on three subjects, Mathematics, Malagasy, and French, were averaged into a single score. The results show no direct impact of any of the three interventions on students' average test scores for either year of the program, nor on the change in test scores between Years 1 and 2. There seemed to be a negative impact of the CISCO (district) level intervention on students who were taught by student teachers in Year 1, and a negative impact of the ZAP (sub-district) level intervention on those taught by (regular) contract teachers. A large positive but weakly significant impact of the CISCO level intervention on the change in test scores of those taught by student teachers was found, but the estimates are not very precise. Turning to the relative performance of the three interventions, no significant difference between the district level and sub-district level intervention was found. The school level intervention seemed to have had larger impacts on Year 1 test scores, but not Year 2 test scores, relative to the CISCO level intervention. Similar results are found for the school level intervention relative to the ZAP level intervention.

There are several possible explanations of the lack of any program impacts. First, maybe two years is not enough time for the type of interventions implemented by this program to have

effects on student performance (which is an indirect effect of the program) compared to school management (which is a direct effect). Second, the fact that there are no true baseline data to compare to the post implementation data to may explain the insignificant results on changes in test scores. Third, the sample size may be too small to obtain precise estimates, as the large standard errors on the program coefficient suggest. Finally, it may simply be that the AGEMAD reforms are inherently ineffective, at least in the context of Madagascar's educational system, and thus that other policies will be needed to improve students' academic performance.

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[1] More details on the AGEMAD program are given below in Section III.
[2] Tools that are necessary for a teacher to properly manage his/her classroom and teach more effectively.
[3] Well managed schools are defined as schools where "the school director and all the teachers perform all their essential tasks" (Lassibile et. al. 2010). See Table A.1 in Appendix for a list of the essential tasks.
[4] See Angrist and Pischke (2009, pp.22-24) on this point. They also point out that adding controls could increase precision, as explained in the next paragraph, but that this is not necessarily the case.
[5] Separate results for each subject (available from the authors on request) are similar to the overall numbers. For example, when the regressions in Table 4 are estimated for each subject, all the intervention effects are insignificant.
[6] The standard errors in Table 4 allow for heteroskedasticity of unknown form, including allowing for arbitrary correlation in the error terms of the estimated equations for any schools in the same CISCO. Clustering at the ZAP or the school level reduces the standard errors somewhat, but not enough to lead to statistically significant results.
[7] Adding covariates and interaction terms reduces the sample size by about 10-15%. This raises the possibility that any changes in results due to the introduction of covariates and interaction terms is due to a change in the sample, not the change in the specification. To check this, we re-estimated all regressions including only observations that had all the covariates and interaction terms; the results (available from the authors) were essentially unchanged.
[8] A related issue is whether student characteristics varied by type of teacher. In fact, there is very little variation of this type. For example, the distribution of high (low) income students by type of teacher was: 55.7% (51.8%) had regular civil service teachers; 7.3% (8.3%) had regular contract teachers; 33.7% (37.6%) had FRAM contract teachers; and 3.2% (2.3%) had student teachers.

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Table 1: Supply of schools and teachers in Madagascar 1997-2005

	1997-98	2000-01	2001-02	2002-03	2003-04	2004-05	2005-06
Public							
Number of schools	10,610	12,730	14,436	14,637	15,420	15,690	16,917
Pupils per school	178	181	167	195	218	229	219
Number of teachers	28,537	33,868	36,181	38,509	47,315	48,871	57,024
% FRAM	5.0	17.9	16.2	18.5	27.5	33.2	48.5
Pupils per teacher	51	53	52	59	57	60	52
Private							
Number of schools	3,022	3,532	3,859	4,340	4,740	4,946	5,301
Pupils per school	141	141	134	134	137	138	135
Number of teachers	12,111	15,543	14,555	16,800	16,950	18,266	19,807
Pupils per teacher	35	32	35	35	38	37	36
Total							
Number of schools	13,632	16,262	18,295	18,977	20,160	20,636	22,218
Number of teachers	40,648	49,411	50,736	55,309	64,265	67,137	76,831
Pupils per teacher	47	47	47	52	52	54	48

Source: World Bank (2008)

Table 2: Teacher Characteristics by Type of Teacher

Teacher type	Definition	Hired by	Paid by	Where they teach	Temporary or permanent
Fonctionnaire	Regular civil service teachers	Ministry of Education	Ministry of Education	Public schools	Permanent
Contractuel	Regular contract teachers	Ministry of Education	Ministry of Education	Public or private schools	Temporary
FRAM subventionné par l'Etat	FRAM Contract teachers	Parents-teachers association	Ministry of Education	Public schools	Temporary
FRAM non subventionné par l'Etat	FRAM Contract teachers	Parents-teachers association	Parents-teachers association	Public schools	Temporary
Sortant CRINFP	Recent graduate from teacher training center	Ministry of Education	Ministry of Education	Public or private schools	Temporary
Elève-maître	Student teachers	Ministry of Education	Ministry of Education	Public or private schools	Temporary
Autres	Other types of teachers (e.g. private teachers)	Varies by type of teacher	Varies by type of teacher	Public or private schools	Temporary

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Table 3: Selected Teacher Characteristics from Year Two School Survey Data

Teacher type	Percent		Age	Years of Experience	Percent with High school diploma or higher	Percent with any pedagogical diploma	Salary (in ariary)
	Current Job	First Job					
Fonctionnaire	52.8	27.8	48.7	25.5	8.1	85.7	188,919
Contractuel	8.1	7.9	47.4	24.2	15.0	76.1	185,539
FRAM subventionné par l'Etat	25.2	20.7	31.5	3.7	21.2	5.3	70,266
FRAM non subventionné par l'Etat	11.4	31.3	32.5	5.7	21.0	10.6	60,035
Elève-maître or Sortant CRINFP	2.5	--	--	--	--	--	--
Maître privé	--	12.3	41.0	17.4	19.8	42.8	112,228

All columns are calculated directly from the 2006-07 data. The teacher type in the second column refers to the kind of position the teachers held in their first job (civil servant, private teacher, teacher FRAM, etc.); since almost all *fonctionnaires* were initially student teachers (*elève-maîtres*) those two categories are combined into *fonctionnaire*. These data were collected for 5 teachers per school and have only 6 categories for teacher type (S*ortant CRINFP* and *autres* are not among the categories).

Table 4: Impact of All Three Interventions on Year 1 (2005-06) Test Scores

VARIABLES	No controls Year 1 score	Student controls Year 1 score	Teacher controls Year 1 score	With interactions Year 1 score
CISCO level intervention	-0.034	-0.050	-0.035	-0.098
	(0.115)	(0.114)	(0.116)	(0.140)
ZAP level intervention	-0.052	-0.048	-0.062	-0.018
	(0.110)	(0.111)	(0.114)	(0.130)
School level intervention	0.106	0.108	0.094	0.009
	(0.117)	(0.119)	(0.119)	(0.151)
Male		-0.007	-0.011	-0.013
		(0.026)	(0.026)	(0.026)
Age (years)		0.042***	0.043***	0.043***
		(0.013)	(0.012)	(0.012)
Mother's education		0.229***	0.214***	0.215***
		(0.025)	(0.027)	(0.027)
Father's education		0.141***	0.133***	0.135***
		(0.023)	(0.021)	(0.020)
Wealth index		0.071**	0.070**	0.068**
		(0.026)	(0.026)	(0.025)
Teacher contractual			-0.103	-0.206**
			(0.108)	(0.098)
Teacher FRAM			-0.149***	-0.182**
			(0.048)	(0.084)
Teacher sortant-elevemaitre			0.122	0.107
			(0.143)	(0.180)
Contractuel*School				0.284
				(0.261)
Contractuel*ZAP				0.127
				(0.177)
Contractuel*CISCO				0.031
				(0.203)
FRAM*School				0.133
				(0.116)
FRAM*ZAP				-0.166
				(0.118)
FRAM*CISCO				0.141
				(0.137)
Sortant-elevemaitre*School				0.299
				(0.260)
Sortant-elevemaitre*ZAP				-0.119
				(0.336)
Sortant*elevemaitre*CISCO				-0.538**
				(0.213)
Constant	0.006	-0.710***	-0.620***	-0.604***
	(0.077)	(0.149)	(0.160)	(0.160)
Observations	21,057	18,453	17,767	17,767
R-squared	0.004	0.021	0.025	0.031

Standard errors in parentheses, clustered at the CISCO level. The teacher types "Sortant CRINFP" and "Eleve-maitre " were combined into "Sortant_elevemaitre". *** p<0.01, ** p<0.05, * p<0.1

Table 5: Impact of All Three Interventions on Year 2 (2006-07) Test Scores

VARIABLES	No controls Year 2 score	Student controls Year 2 score	Teacher controls Year 2 score	With interactions Year 2 score
CISCO level intervention	-0.014	-0.020	-0.013	-0.019
	(0.115)	(0.114)	(0.114)	(0.135)
ZAP level intervention	0.010	0.011	0.012	0.061
	(0.104)	(0.104)	(0.107)	(0.121)
School level intervention	0.105	0.090	0.079	0.068
	(0.116)	(0.115)	(0.115)	(0.134)
Male		-0.042*	-0.047**	-0.046**
		(0.021)	(0.021)	(0.021)
Age		0.014	0.021	0.021
		(0.013)	(0.012)	(0.012)
Mother's education		0.169***	0.161***	0.159***
		(0.043)	(0.043)	(0.041)
Father's education		0.118***	0.120***	0.121***
		(0.026)	(0.027)	(0.027)
Wealth index		0.043*	0.044*	0.043*
		(0.022)	(0.022)	(0.021)
Teacher contractuel			-0.064	-0.046
			(0.068)	(0.113)
Teacher FRAM			-0.062	-0.029
			(0.050)	(0.070)
Teacher sortant-elevemaitre			0.152	0.002
			(0.159)	(0.422)
Contractuel*School				0.232
				(0.226)
Contractuel*ZAP				-0.089
				(0.142)
Contractuel*CISCO				-0.287
				(0.173)
FRAM*School				-0.062
				(0.101)
FRAM*ZAP				-0.118
				(0.092)
FRAM*CISCO				0.042
				(0.133)
Sortant-elevemaitre*School				0.433
				(0.504)
Sortant-elevemaitre*ZAP				-0.098
				(0.464)
Sortant-elevemaitre*CISCO				0.213
				(0.432)
Constant	-0.023	-0.392**	-0.424**	-0.436**
	(0.090)	(0.167)	(0.175)	(0.175)
Observations	21,057	19,987	19,281	19,281
R-squared	0.002	0.010	0.012	0.017

Standard errors in parentheses, clustered at the CISCO level. The teacher types "Sortant CRINFP" and "Eleve-maitre "were combined into "Sortant_elevemaitre". *** p<0.01, ** p<0.05, * p<0.1

Table 6: Impact of All Three Interventions on Change in Test Scores

VARIABLES	No controls Change in score	Student controls Change in score	Teacher controls Change in score	With interactions Change in score
CISCO level intervention	0.020	0.015	0.007	0.053
	(0.123)	(0.123)	(0.127)	(0.116)
ZAP level intervention	0.062	0.064	0.073	0.079
	(0.105)	(0.109)	(0.113)	(0.104)
School level intervention	-0.001	0.004	0.005	0.073
	(0.108)	(0.108)	(0.114)	(0.119)
Male		-0.044***	-0.039**	-0.039**
		(0.015)	(0.016)	(0.016)
Age		-0.045***	-0.043***	-0.043***
		(0.010)	(0.011)	(0.010)
Mother's education		-0.046	-0.038	-0.041
		(0.048)	(0.047)	(0.046)
Father's education		-0.005	-0.004	-0.005
		(0.032)	(0.035)	(0.036)
Wealth index		-0.030*	-0.029	-0.029
		(0.017)	(0.017)	(0.018)
Teacher contractual			0.033	0.183
			(0.105)	(0.116)
Teacher FRAM			0.091**	0.135
			(0.038)	(0.097)
Teacher sortant-elevemaitre			0.028	-0.114
			(0.187)	(0.368)
Contractuel*School				-0.095
				(0.240)
Contractuel*ZAP				-0.274*
				(0.157)
Contractuel*CISCO				-0.359
				(0.222)
FRAM*School				-0.166
				(0.120)
FRAM*ZAP				0.060
				(0.121)
FRAM*CISCO				-0.062
				(0.125)
Sortant-elevemaitre*School				0.166
				(0.431)
Sortant-elevemaitre*ZAP				0.012
				(0.514)
Sortant-elevemaitre*CISCO				0.752*
				(0.381)
Constant	-0.028	0.566***	0.500**	0.473**
	(0.080)	(0.182)	(0.199)	(0.189)
Observations	21,057	19,987	19,281	19,281
R-squared	0.001	0.007	0.008	0.013

Standard errors in parentheses, clustered at the CISCO level. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Impact of All Three Interventions on Year 2 Scores (Conditional on Year 1 Scores)

VARIABLES	No controls Year 2 score	Student controls Year 2 score	Teacher controls Year 2 score	With interactions Year 2 score
CISCO level intervention	0.001	-0.005	-0.004	0.012
	(0.104)	(0.104)	(0.105)	(0.107)
ZAP level intervention	0.033	0.034	0.039	0.069
	(0.089)	(0.091)	(0.094)	(0.095)
School level intervention	0.059	0.053	0.047	0.071
	(0.096)	(0.096)	(0.099)	(0.105)
Year 1 score	0.439***	0.436***	0.437***	0.436***
	(0.024)	(0.026)	(0.027)	(0.026)
Male		-0.043***	-0.043***	-0.043***
		(0.013)	(0.013)	(0.013)
Age		-0.012	-0.007	-0.007
		(0.010)	(0.010)	(0.010)
Mother's education		0.075*	0.074*	0.072*
		(0.041)	(0.039)	(0.038)
Father's education		0.064**	0.066**	0.066**
		(0.024)	(0.026)	(0.026)
Wealth index		0.011	0.012	0.011
		(0.015)	(0.015)	(0.015)
Teacher contractuel			-0.022	0.054
			(0.066)	(0.100)
Teacher FRAM			0.005	0.043
			(0.039)	(0.073)
Teacher sortant-elevemaitre			0.098	-0.049
			(0.158)	(0.390)
Contractuel*School				0.089
				(0.189)
Contractuel*ZAP				-0.170
				(0.114)
Contractuel*CISCO				-0.319*
				(0.166)
FRAM*School				-0.108
				(0.095)
FRAM*ZAP				-0.040
				(0.087)
FRAM*CISCO				-0.003
				(0.113)
Sortant-elevemaitre*School				0.317
				(0.458)
Sortant-elevemaitre*ZAP				-0.050
				(0.453)
Sortant-elevemaitre*CISCO				0.448
				(0.396)
Constant	-0.025	0.025	-0.021	-0.040
	(0.077)	(0.156)	(0.164)	(0.160)
Observations	21,057	19,987	19,281	19,281
R-squared	0.227	0.228	0.231	0.235

Standard errors in parentheses, clustered at the CISCO level. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Relative Performance of CISCO Level and ZAP Level Interventions

VARIABLES	No controls Year 1 score	All controls with interactions Year 1 score	No controls Year 2 score	All controls with interactions Year 2 score
ZAP level intervention	-0.025	0.054	0.019	0.088
	(0.080)	(0.097)	(0.070)	(0.090)
Male		-0.022		-0.056*
		(0.025)		(0.031)
Age		0.052***		0.021**
		(0.010)		(0.009)
Mother's education		0.141**		0.017
		(0.056)		(0.051)
Father's education		0.151***		0.139***
		(0.049)		(0.043)
Wealth index		0.016		0.025
		(0.018)		(0.016)
Teacher contractuel		0.019		-0.248*
		(0.191)		(0.148)
Teacher FRAM		-0.010		0.005
		(0.092)		(0.081)
Teacher sortant-elevemaitre		-0.069		0.317***
		(0.172)		(0.119)
Contractuel*ZAP		0.046		0.101
		(0.242)		(0.198)
FRAM*ZAP		-0.235*		-0.179
		(0.123)		(0.117)
Sortant-elevemaitre*ZAP		0.288		-0.309
		(0.298)		(0.261)
Constant	-0.418***	-1.253***	-0.308***	-0.564***
	(0.109)	(0.157)	(0.117)	(0.181)
Observations	10,430	8,770	10,430	9,491
R-squared	0.076	0.103	0.055	0.064

Standard errors in parentheses, with CISCO fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Relative Performance of CISCO Level and ZAP Level Interventions: Change in Test Scores and Year 2 Score Conditional on Year 1 Score

VARIABLES	No controls Change in score	All controls with interactions Change in score	No controls Year 2 score	All controls with interactions Year 2 score
ZAP level intervention	0.044	0.065	0.029	0.079
	(0.079)	(0.102)	(0.063)	(0.083)
Year 1 score			0.412***	0.408***
			(0.021)	(0.021)
Male		-0.031		-0.046
		(0.035)		(0.030)
Age		-0.055***		-0.010
		(0.010)		(0.009)
Mother's education		-0.011		0.005
		(0.064)		(0.049)
Father's education		0.019		0.090**
		(0.050)		(0.040)
Wealth index		-0.013		0.009
		(0.017)		(0.014)
Teacher contractual		-0.224		-0.238
		(0.217)		(0.154)
Teacher FRAM		0.045		0.022
		(0.091)		(0.073)
Teacher sortant-elevemaitre		0.417***		0.358***
		(0.133)		(0.102)
Contractuel*ZAP		-0.005		0.057
		(0.297)		(0.207)
FRAM*ZAP		0.035		-0.092
		(0.130)		(0.105)
Sortant-elevemaitre*ZAP		-0.631*		-0.441*
		(0.351)		(0.262)
Constant	0.111	0.783***	-0.135	-0.014
	(0.129)	(0.198)	(0.110)	(0.172)
Observations	10,430	9,491	10,430	9,491
R-squared	0.062	0.075	0.252	0.254

Standard errors in parentheses, with CISCO fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Relative Performance of CISCO Level and School Level ("Full") Interventions

VARIABLES	No controls Year 1 score	All controls with interactions Year 1 score	No controls Year 2 score	All controls with interactions Year 2 score
School level intervention	0.130**	0.089	0.108	0.089
	(0.059)	(0.084)	(0.066)	(0.095)
Male		-0.018		-0.043
		(0.034)		(0.030)
Age		0.049***		0.007
		(0.007)		(0.008)
Mother's education		0.115*		0.103**
		(0.055)		(0.045)
Father's education		0.126***		0.073**
		(0.030)		(0.032)
Wealth index		0.031		0.020
		(0.021)		(0.017)
Teacher contractuel		0.055		-0.179
		(0.168)		(0.179)
Teacher FRAM		0.011		0.028
		(0.093)		(0.111)
Teacher sortant-elevemaitre		-0.139		0.267
		(0.193)		(0.158)
Contractuel*School		0.242		0.521**
		(0.224)		(0.239)
FRAM*School		-0.003		-0.129
		(0.128)		(0.141)
Sortant-elevemaitre*School		0.625**		0.150
		(0.281)		(0.285)
Constant	-0.380***	-1.085***	-0.232***	-0.385**
	(0.025)	(0.118)	(0.027)	(0.131)
Observations	10,416	8,846	10,416	9,607
R-squared	0.095	0.116	0.071	0.086

Standard errors in parentheses, with CISCO fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Relative Performance of CISCO Level and School Level ("Full") Interventions: Change in Test Scores and Year 2 Score Conditional on Year 1 Score

VARIABLES	No controls Change in score	All controls with interactions Change in score	No controls Year 2 score	All controls with interactions Year 2 score
School level intervention	-0.022	0.041	0.053	0.069
	(0.073)	(0.069)	(0.050)	(0.073)
Year 1 score			0.427***	0.429***
			(0.029)	(0.034)
Male		-0.020		-0.033
		(0.027)		(0.024)
Age		-0.066***		-0.024**
		(0.011)		(0.009)
Mother's education		-0.002		0.058
		(0.069)		(0.049)
Father's education		0.025		0.052
		(0.067)		(0.042)
Wealth index		-0.028		-0.001
		(0.020)		(0.016)
Teacher contractuel		-0.207		-0.191
		(0.184)		(0.163)
Teacher FRAM		0.053		0.039
		(0.066)		(0.084)
Teacher sortant-elevemaitre		0.426***		0.335***
		(0.066)		(0.088)
Contractuel*School		0.234		0.398*
		(0.200)		(0.192)
FRAM*School		-0.145		-0.136
		(0.140)		(0.128)
Sortant-elevemaitre*School		-0.467*		-0.115
		(0.245)		(0.238)
Constant	0.148	0.897***	-0.069**	0.164
	(0.121)	(0.216)	(0.024)	(0.151)
Observations	10,416	9,607	10,416	9,607
R-squared	0.065	0.082	0.260	0.276

Standard errors in parentheses, with CISCO fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Relative Performance of ZAP Level and School Level ("Full") Interventions

VARIABLES	No controls Year 1 score	All controls with interactions Year 1 score	No controls Year 2 score	All controls with interactions Year 2 score
School level intervention	0.159**	0.064	0.095	0.031
	(0.069)	(0.090)	(0.059)	(0.080)
Male		-0.014		-0.035
		(0.031)		(0.037)
Age		0.056***		0.025**
		(0.013)		(0.010)
Mother's education		0.173***		0.097**
		(0.055)		(0.044)
Father's education		0.197***		0.089**
		(0.042)		(0.035)
Wealth index		0.015		0.017
		(0.022)		(0.017)
Teacher contractuel		0.079		-0.164
		(0.135)		(0.134)
Teacher FRAM		-0.209*		-0.129
		(0.105)		(0.083)
Teacher sortant-elevemaitre		0.259		0.027
		(0.280)		(0.254)
Contractuel*School		0.194		0.450**
		(0.217)		(0.188)
FRAM*School		0.190		-0.011
		(0.152)		(0.110)
Sortant-elevemaitre*School		0.141		0.289
		(0.340)		(0.361)
Constant	-0.495***	-1.358***	-0.378***	-0.691***
	(0.056)	(0.171)	(0.075)	(0.173)
Observations	10,472	8,822	10,472	9,668
R-squared	0.080	0.112	0.054	0.068

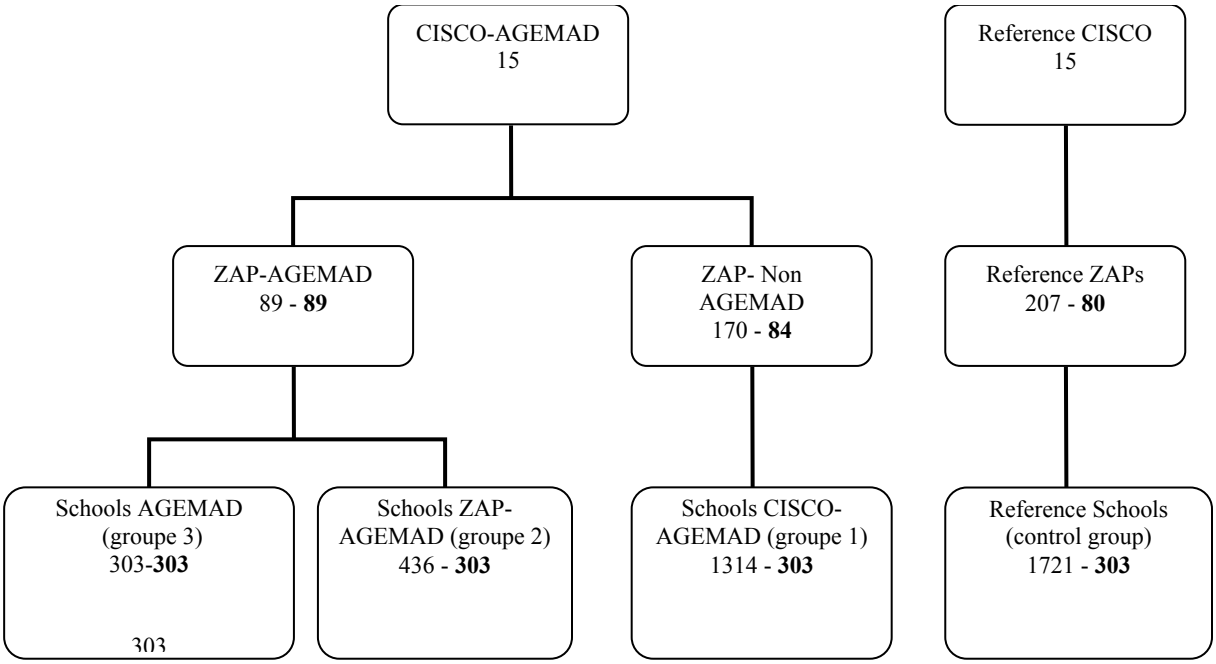
Standard errors in parentheses, with CISCO fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Table 13: Relative Performance of ZAP Level and School Level ("Full") Interventions: Change in Test Scores and Year 2 Score Conditional on Year 1 Score

VARIABLES	No controls Change in score	All controls with interactions Change in score	No controls Year 2 score	All controls with interactions Year 2 score
School level intervention	-0.063	-0.022	0.028	0.008
	(0.069)	(0.075)	(0.054)	(0.066)
Year 1 score			0.426***	0.423***
			(0.024)	(0.024)
Male		-0.038		-0.036
		(0.032)		(0.031)
Age		-0.043***		-0.004
		(0.009)		(0.008)
Mother's education		-0.062		0.030
		(0.078)		(0.053)
Father's education		-0.054		0.029
		(0.055)		(0.038)
Wealth index		-0.024		-0.000
		(0.020)		(0.015)
Teacher contractual		-0.285		-0.215
		(0.212)		(0.149)
Teacher FRAM		0.075		-0.043
		(0.100)		(0.073)
Teacher sortant-elevemaitre		-0.240		-0.086
		(0.335)		(0.251)
Contractuel*School		0.266		0.372*
		(0.288)		(0.203)
FRAM*School		-0.187		-0.085
		(0.154)		(0.108)
Sortant-elevemaitre*School		0.180		0.243
		(0.383)		(0.328)
Constant	0.117	0.780***	-0.167*	-0.068
	(0.103)	(0.194)	(0.085)	(0.170)
Observations	10,472	9,668	10,472	9,668
R-squared	0.042	0.056	0.260	0.266

Standard errors in parentheses, with CISCO fixed effects.
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Distribution of ZAP and schools in initial sample and final sample by treatment group[a]



a/ Sample sizes for in final sample in bold, final sample is sample for which data was collected on both years of the experiment

Source: De Nouveaux Modes de Gestion pour Accroitre les Performances de l'Enseignement Primaire Malgache : Evaluation d'Impact de l'Initiative d'Amélioration de la Gestion de l'Education a Madagascar (AGEMAD) Octobre 2008, World Bank

Appendix

Table A.1: Essential Tasks for Teachers and School Directors

Teachers	School Directors
Takes daily roll call Prepares daily lesson plan Prepared bi-monthly lesson plans Monitors student learning Has tested the pupils during the past two months Helps lagging students Discusses student learning issues with the director	Keeps a register of enrollments Signs off on daily roll call Analyzes student absences on a monthly or bi-monthly basis Reviews students' test results Takes stock of teacher absences Informs sub-district or district officer about teacher absences Follows up with teachers on lesson planning

Source: Lassibille et. al., 2010

Table A.2: AGEMAD Tools for Use by Teachers

Objective	Activities	AGEPA Tool			
		Code	Description	Theme	Periodicity
Provide overview of the year's curriculum and promote systematic lesson planning	Plan time use and calendar of lessons Prepare lessons Track pupils' reception of the lessons	E1A/PED	Bimonthly lesson plan for the entire school year	Pedagogy	At start of school year
		E1B/PED	Weekly lesson plan for bimonthly periods	Pedagogy	At start of each bimonthly period
		E2/PED	Individual lesson plan	Pedagogy	Daily
Put focus on student learning	Prepare and mark tests Record test scores Identify and help lagging pupils	E3/EVA	Record of bimonthly test scores *(Cahier de notes de class)*	Evaluation	After each bimonthly test
Mobilize parental support for academic excellence	Inform parents about pupils' progress in school	E4/EVA	Individual pupil report card *(Bulletin individuel de notes*)	Evaluation	End of each bimonthly period
Reduce student absenteeism	Monitor pupils' attendance Detect possible attendance problems and their causes Take remedial action	E5/APP	Class attendance register	Time for learning	Daily
Reduce teacher absenteeism	Account for teacher absences Detect possible attendance problems and their causes Take remedial action	E6/APP	Teacher's personal leave/travel record card	Time for learning	Each occasion of absence

Table A.3: AGEMAD Tools for Use by School Principals (Directors)

Objective	Activities	AGEPA tool			
		Code	**Description**	**Theme**	**Periodicity**
Ensure proper registration of pupils	Keep up-to-date register of pupils at the school	D1/ADM	*Registre matricule* (National Printer document)	Administration	Each time a new pupil registers
Reduce teacher absenteeism	Monitor presence of teachers	D2/APP	Attendance register	Time for learning	Daily
		D3/APP	Summary table of teacher absences	Time for learning	At end of each month
Reduce pupil absenteeism	Record summary of pupil attendance records Review attendance record and assess possible problems and their causes Take remedial action	D4/APP	Summary table of monthly record of pupil absenteeism	Time for learning	At end of each month
Focus on progress of student learning at the school	Call periodic staff meetings (*Conseil des Maîtres*) Track student learning, note strengths and weaknesses, and plan to implement remedial action as needed	D5/EVA	Summary of student test scores	Evaluation	At end of each bimonthly period
Improve school's internal efficiency	Use school's data on student flow to identify and address possible problems Ensure application of grade to grade transition criteria (i.e., *système de cours)* [a/]	D6/EVA	Student flow table	Evaluation	End of each school year
Strengthen school's-partnership with the local community	Organize meetings with parents and teachers, FAF and FRAM Sensitize parents of pupils at the school Work with parents to prepare a *contrat programme de réussite scolaire* (CPRS)	D7/PART	Community meeting form	Community relations	At each meeting with the community
Improve conditions for learning at the school	Assign teaching duties among staff, and allocate classrooms and teaching materials; and select teachers for in-service training.	D8/PED	Organization of pedagogical arrangements	Pedagogy	At start of school year
Improve performance of the school	Study and display in public area the school report card Update school performance indicators Discuss the school report card with community; use it to develop a school improvement plan (*contract programme*) for implementation	D9/TDB	School report card (*Tableau de bord de l'école*)	Focus on results	At end of school year

a/ Automatic promotion between grades 1 and 2 and between grades 4 and 5.

Table A.4: AGEMAD Tools for Use by ZAP (Sub-District) Heads

Objective	Activities	AGEPA Tool			
		Code	Description	Theme	Periodicity
Improve management of schools in ZAP	Keep up-to-date inventory of schools in the ZAP	Z1/ADM	List of all schools in the ZAP	Administration	At start of school year
Improve the education information system	Ensure timely and complete collection of school data on the census forms	Z2/STA	Checklist of data collection	Statistics	At start and end of school year
Improve pupils' access to pedagogical materials	Determine number of pupils in each functioning school to receive a school kit	Z3/STA	Enrollments by school and distribution of the school kits	Statistics	At start of school year
Strengthen teacher competencies	Take stock of in-service teacher training (*journées pédagogiques*)	Z4/FOR	*Aide mémoire* of in-service teacher training event	Training	After each training event
	Supervise and provide pedagogical support to teachers in schools Provide feedback to school director Track assimilation of training provided Identify potential areas of additional training to help teachers become more effective	Z5/PED	Pedagogical supervision and support form	Pedagogy	After each school visit
Improve management at school finances	Track expenditure against grants provided through the *Caisse Ecole* and *Caisse Cantine*	Z6/ADM	*Caisse Ecole* form	Administration	On each school visit
	Ensure public posting of the expenditures	Z7/ADM	*Caisse Cantine* form	Administration	On each school visit
Strengthen school-community partnership	Maintain relations with the community Visit/meet community partners for education	Z8/PART	Community meeting form	Community relations	On each school visit
Improve performance of schools in the ZAP	Take note of each school's progress from year to year Compare schools in the ZAP, identify lagging schools, and plan extra support and attention for such schools	Z9/TDB	ZAP report card (*Tableau de bord de la ZAP*)	Focus on results	At start of school year

Table A.5: AGEMAD Tools for Use by CISCO (District) Heads

Objective	Activities	AGEPA Tool			
		Code	Description	Theme	Periodicity
Manage teacher transfers	Take stock of existing deployment of teachers Summarize and process transfer requests	C1/ADM	Teacher transfer master sheet	Administration	End of school year
Improve schooling conditions	Inform the Ministry about requirements for school kits and other school supplies Distribute school kits, textbooks and other supplies	C2/ADM	Pedagogical supplies form	Administration	Before start of school year
Improve management of school finances	Distribute the *Caisse Ecole* and *Caisse Cantine* grants Ensure follow-up on utilization of the grants	C3A/ADM	*Caisse Ecole* follow-up sheet	Administration	End of second bimonthly period
		C3B/ADM	*Caisse Cantine* follow-up sheet	Administration	End of second bimonthly period
		C4A/ADM	*Caisse Ecole* verification form	Administration	On occasion of each visit to ZAP
		C4B/ADM	*Caisse Cantine* verification form	Administration	On occasion of each visit to ZAP
Strengthen teacher competencies	Identify teacher training needs and organize training events Implement post-training follow-up	C5/FOR	Planning sheet for in-service teacher training (*journées pédagogiques*)	Training	Before each training event
		C6/FOR	Follow-up sheet for in-service teacher training	Training	Before and after each training event
	Plan school visits to provide in-service support, giving priority to the schools in difficulty Provide feedback to school personnel based on systematic on-site recording of classroom practices	C7/PED	Class observation grid (*Grille d'observation de classe*)	Pedagogy	On each school visit
Improve performance of schools in CISCO	Evaluate outcomes across ZAPs and schools and identify lagging units for extra support and attention Analyze possible sources of difficulty in lagging units and plan remedial action for implementation	C8/TDB	CISCO report card *(Tableau de bord de la CISCO)*	Focus on results	At start of school year