"Congratulations, You Have Been Randomized Into the Control Group!(?)": Issues to Consider When Recruiting Schools for Matched-Pair Randomized Control Trials of Prevention Programs

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ABSTRACT

BACKGROUND: Recruiting schools into a matched-pair randomized control trial (MP-RCT) to evaluate the efficacy of a school-level prevention program presents challenges for researchers. We considered which of 2 procedures would be most effective for recruiting schools into the study and assigning them to conditions. In 1 procedure (recruit and match/randomize), we would recruit schools and match them prior to randomization, and in the other (match/randomize and recruitment), we would match schools and randomize them prior to recruitment.

METHOD: We considered how each procedure impacted the randomization process and our ability to recruit schools into the study. After implementing the selected procedure, the equivalence of both treatment and control group schools and the participating and nonparticipating schools on school demographic variables was evaluated.

RESULTS: We decided on the recruit and match/randomize procedure because we thought it would provide the opportunity to build rapport with the schools and prepare them for the randomization process, thereby increasing the likelihood that they would accept their randomly assigned conditions. Neither the treatment and control group schools nor the participating and nonparticipating schools exhibited statistically significant differences from each other on any of the school demographic variables.

CONCLUSIONS: Recruitment of schools prior to matching and randomization in an MP-RCT may facilitate the recruitment of schools and thus enhance both the statistical power and the representativeness of study findings. Future research would benefit from the consideration of a broader range of variables (eg, readiness to implement a comprehensive prevention program) both in matching schools and in evaluating their representativeness to nonparticipating schools.

Keywords: randomization; recruitment; school prevention programs.

Citation: Ji P, DuBois DL, Flay BR, Brechling V. "Congratulations, you have been randomized into the control group!(?)": Issues to consider when recruiting schools for matched-pair randomized control trials of prevention programs. J Sch Health. 2008; 78: 131-139.

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Comprehensive school-based prevention programs that address a broad range of student outcomes, such as academic achievement, prosocial behaviors, and self-esteem, are usually applied school wide.[1] Research designs involving random assignment to conditions are necessary to provide the strongest empirical evidence concerning the effectiveness of school-based prevention programs and have been proposed as 1 criterion for a prevention program to be considered ''evidence based'' or efficacious.[1–10] Randomization reduces the chance that extraneous factors serve as alternate explanations for observed differences in outcomes between treatment and nontreatment schools and strengthens the claim that the intervention itself produced these differences.[11–14] For example, randomization reduces the likelihood that participant selection bias is a factor in the outcome and thus strengthens the internal validity of the results.[14]

If an intervention is applied school wide, it is best to randomize at the school level for evaluation purposes. Randomization at the classroom or student level within a school may contaminate program effects because students typically interact with each other during the school day.[6] Because students are clustered within schools, analyses can then be adjusted to take into account any nonindependence that this introduces into data associated with different students in the same school (as indexed by the intraclass correlation coefficient).[15] Because the number of schools being randomized is typically relatively small in trials of school-wide interventions, randomization alone typically will not ensure equivalence between treatment and control conditions.[6,16] To address this concern, the matched-pair randomized control trial (MP-RCT) may be used. In this design, schools are matched into pairs based on relevant variables, such as standardized academic test scores or number of total students enrolled, and then 1 member of each pair is randomized into a control or treatment group.[11,14] The matched-pair element increases the chance that the conditions of the control and treatment schools are similar on measured and nonmeasured factors and that results will not be attributable to preexisting differences.[6,7,11,13,14]

Recruiting schools for an MP-RCT presents unique challenges for researchers.[17] An assumption that schools will simply acquiesce to their randomly assigned condition has the potential to backfire. Schools may not want to participate in an MP-RCT, for example, because they may prefer to self-select their conditions.[18] In this regard, some schools may prefer to be in the treatment condition because they desire a program that promises to bring immediate benefits to their students.[13,19] Kam et al found that they could not randomly assign schools to test an intervention because the local community required that schools with students who were considered to be most ''at risk'' be given priority in receiving the intervention.[20] Conversely, some schools may prefer to be in the control condition because they are overwhelmed with too many other demands on staff time or they doubt the efficacy of the new program.[13,19]

If schools decline participation, the sample's representativeness, and hence the study's external validity, may be compromised because the participating schools are different from the nonparticipating schools. An overall reduction in the number of participating schools, furthermore, decreases the statistical power of the study to detect the intervention's effectiveness. This issue is an especially important concern for MP-RCTs because in clustered statistical analyses, statistical power will be dependent on the number of schools more so than the number of students sampled at each school.[15] Fewer available schools also may make it more difficult to match schools into pairs, potentially affecting both internal validity and statistical power.

The possibility that a school could refuse participation in an MP-RCT because the school did not want to be randomized into a condition has implications for researchers' recruiting strategies.[21] The randomization element challenges researchers to recruit schools that will accept their assignments notwithstanding their potential preferences to be assigned to 1 group over another. The matched-pair element challenges researchers to find pairs of schools that are sufficiently similar and both willing to accept their random assignments. An important concern in any randomized trial is determining when to conduct the randomization during the recruitment process.[22] In the context of an MP-RCT, this decision may have significant implications for rates of school participation and, in turn, affect issues of internal and external validity and statistical power for reasons described above. Previous researchers have provided accounts of their efforts to recruit schools to participate in program evaluation studies.[17,23] None to our knowledge, however, have discussed how either an MP-RCT design generally or decisions regarding the positioning of randomization in the recruitment process specifically within this type of design affect the process and outcomes of school recruitment. In this article, we describe our experience recruiting schools into an ongoing MP-RCT that is evaluating the effects of a school-based character education and social development prevention program called *Positive Action* (PA).

RESEARCH STUDY BACKGROUND

The PA program is a comprehensive school-based program designed to promote student character and positive behavior, prevent an array of student problem behaviors, and improve student achievement.[24,25] The program includes both a classroom curriculum component as well as school-wide activities to promote staff development and improve climate.

The school-wide nature of the program demands that random assignment be at the level of schools.[6]

MP-RCT RESEARCH DESIGN TO EVALUATE THE PA PROGRAM

The study is part of an ongoing multisite study funded by the Institute for Education Science of the US Department of Education. Seven sites across the nation are participating, and our site's study is taking place in Chicago, IL. Each site is using an MP-RCT to evaluate the effects of a different school-based social and character development program (ie, after schools were matched into pairs at each site, 1 school in each pair was randomized into the treatment group and the other school was assigned to the control group). At the Chicago site, we randomly assigned schools within 7 matched pairs to either an ''Early Start'' group (ie, the treatment group, which immediately received all program materials, training and consulting support to implement the program, and a stipend to offset costs associated with program implementation) or a ''Late Start'' group (ie, the control group, which received a stipend in unrestricted funds for their participation in data collection efforts and an assurance that they would receive the program 3 years later after the evaluation was complete).

The target cohort group at each site consisted of third-grade students enrolled in the 2004-2005 school year. This cohort is being followed until they reach the fifth grade. For each school, data are being collected on students' academic behavior, socioemotional development, and reports of problem behaviors from 3 sources—students, teachers, and parents. The time frame for the study is 4 years. In year 1, schools were recruited and randomized into conditions in preparation for the start of the study in the fall of year 2. For years 2 and 3, data were collected at the beginning and at the end of the school year. For year 4, data will be collected only at the end of the school year.

DETERMINING WHEN TO RANDOMIZE SCHOOLS DURING RECRUITMENT

We were concerned about the implications of positioning the random assignment of the schools at different points in the recruitment process. The order in which we recruited, paired, randomized, and notified the schools of their assigned conditions could impact a school's decision to participate in the study. This, in turn, could affect the participation of the other school in the pair given that a school could participate only if the school with which it was paired accepted its random assignment. After considering potential recruitment and randomization scenarios, we decided among 2 that each had potential merit. In the first procedure, henceforth designated as procedure A (Table 1), we would select schools from the list of Chicago public schools according to our study criteria. We then would approach these schools and inform each school that if they elected to participate, 2 conditions must be met. The school must be paired with another school, and both schools must be willing to be randomized and accept their assignments. In an alternate procedure, designated as procedure B (Table 1), we would select schools, pair them, randomize them into control and treatment conditions, and then recruit each school and inform the school that it has been preassigned to a control or treatment condition.

From the perspective of randomization, procedure A is no different than procedure B. Both schools within a pair received their assignments through randomization. In both approaches, if a control or treatment school decided not to accept its randomly assigned condition and withdrew, then both schools in the pair would have to be dropped to preserve the integrity of the randomization process. Dropping both schools within a pair was necessary to ensure that systematic bias was not introduced when assigning schools to conditions. If a school withdrew, we would not pair the remaining school with an alternate school. Simply inserting another alternate control or treatment school as a replacement in order to complete the pair could still result in systematic bias. The alternate school would not receive its assignment by randomization but by default. Another option that we judged unacceptable was pairing the remaining school

Table 1. **Two Procedures for Recruiting and Randomizing Schools for an MP-RCT**

Procedure A	Procedure B
1. Select initial pool of schools that meet study criteria.	1. Select initial pool of schools that meet study criteria.
2. Secure agreement of schools that they are willing to be randomized and participate in the study.	2. Pair schools and randomize them into conditions.
3. Pair schools and randomize them into conditions.	3. Within each pair, notify the control school first of its assigned condition and recruit into study. After securing the control school's participation, notify and recruit accompanying treatment school.
4. Within each pair, notify the control school first of its assigned condition. After securing the control school's participation, notify the accompanying treatment school.	4. If either school within a pair does not wish to participate, drop both schools in the pair.
5. If either school within a pair withdraws following assignment to condition, drop both schools in the pair.	

with an alternate school and repeating the randomization. This option would result in an imbalance in the number of randomization trials across the schools. True randomization means that each participant in a pool has an equal chance to be assigned to a condition.[14] This would not be possible under this approach because the remaining school would have 2 randomized trials and the alternate school would have 1. Rerandomization also posed ethical concerns. If a school received a second randomization trial, ethically, the other school in the pair should receive a second randomization trial. This situation would place researchers in an awkward recruitment situation. Suppose that, in our case, we had approached a treatment school and it accepted its condition and then approached its matched-pair control school and it did not accept its condition. If we had re-paired the treatment school with an alternate school and rerandomized, the original treatment school might then have been assigned to a control condition. It clearly would pose ethical problems to have returned to the treatment school and changed its originally assigned condition just so we could retain the school pairing.

We considered the advantages and disadvantages of procedures A and B from the perspective of maximizing the likelihood of retaining schools in the study. One concern with procedure A was that a school might initially agree to participate, but if it was randomly assigned to an undesired condition, the school would then withdraw and we would have to drop the pair. To maximize the likelihood that schools would participate in the study despite not receiving their desired condition, we wanted the opportunity to use school recruitment sessions to not only highlight the potential benefits of the intervention but also emphasize the importance of an MP-RCT and how this design was the best for producing convincing evidence about the intervention's intended effects. To assure schools that randomization was conducted with integrity, we wanted to inform all schools simultaneously about randomization during this session so that they would know that everyone would be randomized at the same time and everyone would have an equal chance of receiving the program. We also wanted to use the recruiting session to respond to potential concerns that schools had about being randomized so that we could encourage them to participate regardless of their randomly assigned condition. We hoped that this procedure would increase our chances of securing the schools' agreement to accept their assigned conditions despite their preferences to be in a control or treatment condition.

With procedure B, we would know sooner (ie, without needing to wait for the school's later response to randomization) if a school would or would not participate, thereby potentially saving effort and time during the recruitment process. In this procedure, we could still explain the merits of the MP-RCT design in an effort to encourage schools to accept their preassigned conditions. But we anticipated that we might receive more initial refusals because of the potential for schools to respond negatively to not being informed of the randomization process prior to it taking place. We also considered that with procedure B, we would not be able to determine if it was better to approach the treatment or control schools first. The decision to notify which group first, the control or the treatment schools, cannot be made arbitrarily. Flay and Collins note that a decade ago, it was control schools that were most reluctant to participate in randomized control trials because they would not get the program until the end of the study.[6] However, when policies such as No Child Left Behind were implemented, treatment schools were more reluctant to participate because they did not feel that they had time for programs that did not directly address academic achievement.[6] Now, many schools are recognizing the need for social and character development, so control schools again are more reluctant to participate in randomized control trials because they would prefer to have the program immediately.[6]

With procedure A, during the initial recruitment session, we could use the schools' feedback about the intervention and the study to help ascertain the condition to which they would prefer to be assigned. This information could then be used to determine which school within a pair would be best to approach and secure its participation first. In doing so, we could potentially avoid having to revisit a school and inform it that it could not be in the study if the school that it was paired with decided to withdraw from the study.

In summary, in order to adhere to randomization principles in an MP-RCT, both schools in a pair must be dropped if 1 school within a pair decides to withdraw. Although both recruitment procedures A and B have their merits, we decided to use approach A because we thought it would give us the best chance to prepare schools for being randomized, thereby increasing the likelihood that they would accept their randomly assigned conditions.

THE RECRUITMENT PROCESS

We determined that 14 schools matched into 7 pairs would be the maximum number of schools feasible to recruit and manage in the study and that this sample size would provide statistical power for detecting moderate to large effects of the intervention. Our funding source initially provided support for recruiting only 5 pairs of schools, but we successfully convinced them to provide funds to support recruiting 2 additional pairs of schools. We would have liked to have included an even larger number of schools to ensure power for detecting smaller effects, but

this was not feasible given budget and resource constraints.

Our initial pool of schools was the 483 elementary schools (grades pre-K-6 or pre-K-8) in the CPS. We obtained student demographic data for these schools from the CPS and Illinois State Board of Education Web sites.[26,27] These data were collected so that we could find schools that (a) had a student enrollment that was large enough to constitute a sufficient sample size for our data analysis, allowing for likely attrition due to student mobility, but not so large as to pose prohibitive costs in terms of intervention materials that needed to be provided to all students and school staff and (b) had students who were considered to be at high risk and who could benefit from the intervention.[24,25] Guided by these considerations, schools were selected based on the following criteria: enrollment was between 50 and 140 students per grade, student mobility was below 40%, more than 50% of students were from low-income backgrounds, and less than 50% of students passed the state achievement test. Schools also had to be community based, that is, enroll students from the community in which the school was located. Based on these criteria, we narrowed the pool of eligible schools to 68.

We extended invitations (Figure 1) to these 68 schools. Of them, 32 schools either could not be contacted or declined to attend an information session about the PA program and study. The remaining 36 schools attended the PA information session or elected to have someone from the research team visit them. Two informational sessions were held along with the visits of project staff to some schools. Following the information sessions, project staff made follow-up visits to schools on an as-needed basis to answer any questions that school administrators or staff had about the PA program or the study. Of the 36 schools with which we were able to share information about the study, 18 agreed to participate with the understanding that they would have

Figure 1. **School Recruitment Letter**

Dear Principal:

We are writing to invite you to a special presentation about a program that we think you will feel will be beneficial for your school. The program, *Positive Action*, developed by Dr. Carol G. Allred, is a nationally recognized, science-based, comprehensive school enhancement program. Dr. Brian Flay of UIC recently received a research grant from the U.S. Department of Education to study the effects of the *Positive Action* program on Chicago schools. *Positive Action* was selected because even the most promising programs such as *Positive Action* need to be studied using controlled, rigorous research designs to firmly establish their effectiveness. **This project has been approved by the Chicago Public Schools.** We understand that your school as community-based, of moderate size and mobility, with relatively low performance scores -- that we believe could be helped by *Positive Action*. We hope that you will agree to participate in this project.

This opportunity is also a chance for you and your school to contribute to the ongoing national effort to inform the field of school-based social and character development programs. Should you decide to participate in this project, schools will be selected **randomly** to receive the program either immediately or in three years time. The *Positive Action* program and training will be provided at **no cost** to all participating schools. Schools that receive the program immediately also will receive $4,000 per year for the next 3 years to support a part-time Positive Action Coordinator and $1000 per year to support a Positive Action Committee. Schools that receive the program in three years time will receive $1,000 per year in unrestricted funds for the next 3 years – and then get all program materials and training. All participating schools will be asked to allow researchers to survey their teachers/staff, and to follow one cohort of their students (those who are in grade 3 next year) and their parents for a period of 3 years. The project will provide incentives to parents and teachers to complete surveys (about $4,000 per school each year).

Only 14 schools can participate in this project and we intend to finalize the list of participating schools by the end of May 2004. Should you wish to be part of this select group, we would like to invite you to a presentation by Drs. Allred and Flay about the *Positive Action* program, about the research project and about the logistics of participating in the study and implementing the *Positive Action* program in your school. The presentation will take place on **date**. It will be held at the Health Research and Policy Centers, University of Illinois at Chicago, 1747 W. Roosevelt Road, Chicago, Il 60608. We will meet at the 5th floor conference room. The presentation will last 1 hour with a question/answer session following. **Dinner will be served afterwards.** We hope that one or two CPS representatives will be in attendance. You are welcome to invite other key school personnel who you feel should hear this presentation to attend with you.

Please RSVP to -- please include how many persons from your school will attend and how many will stay for dinner. If you are unable to attend the presentation, we can arrange an individual visit to present the program and address any of your questions. If you wish more information about the *Positive Action* program, please visit their website at www.positiveaction.net.

We look forward to meeting with you.

Sincerely,

to be successfully matched with another school and that each school in the pair would need to accept its randomly assigned condition. Letters of agreement that outlined the conditions of participation were signed by both school officials (the principal and the president of the Local School Council) and principal investigator (contact B.R.F. to obtain a copy of the agreement).

We next used an SAS program (SAS Institute Inc., Cary, NC) provided by Mathematica Policy Research, Inc (MPR) to match the 18 schools into pairs based on school demographic variables.[28] The variables used for matching were selected on the basis of being known predictors of student achievement and problem behaviors, primary outcomes of interest for the PA program.[24,25] The following school demographic variables from the 2003-2004 CPS data were used as matching variables: percentage of white, black, Hispanic, and Asian students; percentage of students who met or exceeded criteria for passing the state achievement test; attendance rate; truancy rate; percentage of students who received a free or reduced-price lunch; percentage of students who enrolled or left school during the school year; number of students per grade; percentage of parents who were involved with school activities; and percentage of teachers employed by the school who met minimal teaching standards. We also used information about the crime rate in the school's neighborhood.[29] Using these variables as input into the MPR program, and with the additional criterion that each school in a pair be located in the same region of the city, we successfully paired the 18 schools into 9 pairs. We selected 7 of the 9 pairs of schools to recruit. In selecting these pairs, we prioritized those that provided the best representation of the ethnic diversity of the CPS students. We then used a random number generation function in the Microsoft Excel program (Microsoft Corporation, Redmond, WA) to randomize the schools within each pair to the control and treatment conditions. The remaining 2 pairs of schools were kept as alternates in case 1 of the original 7 pairs of schools had to be dropped.

We conducted a series of independent groups *t* tests for each school demographic variable to determine if (a) the 7 treatment and 7 control group schools were different from each other and (b) the 14 participating schools were different from the 54 schools from the original pool of 68 that were not participating. As shown in Table 2, neither the treatment and control group schools nor the participating and nonparticipating schools exhibited statistically significant differences from each other on any of the matching variables.

NOTIFYING SCHOOLS OF THEIR ASSIGNMENTS

We next notified schools in each of the 7 selected pairs of their assignments. During our information sessions, we noted that many schools provided positive feedback about the PA program and expressed their desire to be in the treatment condition. Based on these observations, we anticipated that control schools would be more likely than treatment schools to withdraw from the study. We therefore decided to secure the participation of these schools first before

Table 2. **Comparison Between Control and Treatment Schools and Between Schools Participating and Not Participating in the Study***

	Control Schools (n = 7)		Treatment Schools (n = 7)			Participating in Study (n = 14)		Not Participating in Study (n = 54)		
School Matching Variable	**Mean**	**SD**	**Mean**	**SD**	***t***	**Mean**	**SD**	**Mean**	**SD**	***t***
Total school enrollment	67.81	20.41	71.36	21.48	−0.31	589.10	16.40	610.5	145.20	0.48
Percentage of white students	11.73	18.42	10.23	14.06	0.17	11.00	15.80	7.90	14.70	−0.69
Percentage of black students	55.35	43.85	52.48	47.96	0.12	53.90	44.20	57.00	43.80	0.23
Percentage of Hispanic students	28.62	33.26	32.24	34.86	−0.20	30.40	32.80	32.60	36.70	0.21
Percentage of Asian students	4.14	6.40	4.67	7.18	−0.14	44.10	65.40	2.40	5.20	−1.23
Percentage of students meeting minimal state achievement test criteria	34.02	12.20	33.91	9.60	0.11	34.00	10.60	34.10	12.30	0.04
Percentage of students receiving a free lunch	90.88	5.40	89.26	5.47	−0.96	90.10	52.90	90.90	9.90	0.31
School attendance rate	93.05	1.21	94.05	0.99	−0.96	93.60	1.20	92.90	1.90	−1.17
School truancy rate	2.35	1.85	0.85	0.67	0.77	1.60	1.60	3.40	4.60	1.4
School mobility rate	28.54	11.92	27.72	7.11	−0.29	28.10	9.40	30.90	15.60	0.64
Percentage of parent involvement	98.09	1.89	95.10	8.69	0.89	96.60	6.24	94.18	6.51	−1.24
Percentage of nonqualified teachers	7.44	3.78	12.13	7.11	−1.54	9.79	5.97	11.84	5.60	0.84
Neighborhood crime rate	46.26	35.05	50.12	48.68	−0.18	48.19	39.70	50.61	34.8	0.22

*Percentage of students receiving a free lunch indicates low-income students who are eligible to receive free or reduced-price lunches. School attendance rate is defined as the number of days a student attended school divided by the total number of days a student attended school and was absent from school. School truancy rate is defined as the number of students who are truant divided by the average daily student enrollment. School mobility rate reflects any enrollment change between the first school day in September and the last day of the school year, and it is the sum of the students who transferred out and the students who transferred in, divided by the average daily enrollment.

notifying the treatment schools. As previously noted, if we had notified a treatment school first, but then its companion control school withdrew because it did not like its assigned condition, we would have had to revisit the treatment school and inform the school that it could no longer participate in the study—a scenario we wanted to avoid in view of the apparent interest of most schools in having the opportunity to implement the PA program.

Two research staff members who had extensive experience working with CPS delivered a letter to the administrators of control schools that congratulated them for being selected for the study along with a gift (box of candy). These administrators were aware that their schools could be randomized into the control condition. However, when they were actually notified of their assignment, they all expressed significant disappointment and several openly considered not participating in the study. Some administrators expressed the view that they should have had a better chance to be in the treatment group based on such considerations as having agreed to participate early in the recruitment process or being especially enthusiastic about the PA program. Others stated that they did not want their schools to serve as controls in the study because they were under pressure to improve students' test scores and behavior and they needed interventions immediately to help their students. Although it was understood that control schools would receive the intervention after 3 years, the administrators noted that in 3 years, they might no longer be at the school and that a future school administration might decide not to implement the intervention.

To secure the participation of the control schools, research staff reiterated the benefits of the research agreement—that they would receive the PA intervention materials plus training in 3 years, that participating as a control school did not preclude them from using other methods to address their needs as long as those methods were not similar in approach to the PA program, that the obligations of being a control school involved data collection, and that the school would receive an annual stipend of unrestricted funds plus additional monies to offset costs associated with data collection. For ethical reasons, we did not want to attempt to compel schools to comply with the research agreement they had entered into previously. Rather, throughout this process, research staff became familiar with the needs of the school and helped them see how their needs could be met with their current resources.

In all schools, it was important for our research staff to recognize the communication and leadership styles of school administrators so that they could feel at ease candidly discussing their concerns about being selected as a control school. Administrators at 2 of the schools forcefully expressed the desire to pull out of the study if the research team did not change the schools' status. They expected that decisions about who would receive the intervention would be based on considerations other than by random assignment, such as expressed enthusiasm for the intervention, merit, and need. They appeared to believe that they could change their assigned condition if they threatened to withdraw. The research team spent several hours with administrators in these 2 schools to build trust and assure them that the team would follow through on its commitment. In both cases, the assistant principal was an influential factor in convincing the principal to ultimately agree to participate. The resulting relationship between the research team and these schools is now stronger than it is in many of the other control schools. We believe this is due to the time, patience, and effective communication style that were provided to them by the research staff representatives. All control schools have chosen to remain in the study throughout the first 4 time points of data collection.

DISCUSSION

The use of an MP-RCT design in evaluations of school-wide intervention programs poses special issues and challenges. As we have discussed, in the event that either school within a matched pair declines to participate in the study following random assignment, it is necessary to drop both schools in the pair from the study in order to adhere to randomization principles. Although the decision that is made regarding when to randomize schools during the selection and recruitment process does not preclude adherence to this guideline, it may have important implications for whether schools are successfully recruited into the study. A considerable amount of time was spent discussing this issue within our research team. We felt that recruiting schools prior to pairing and randomizing them would give us the best opportunity to introduce schools to the research and prepare them for the randomization process, thereby increasing the likelihood of securing and retaining their participation.

In reality, our experience suggests that preparing the schools to be randomized did little to influence their participation in the study once they had been randomized into the control condition. Rather, the key factor in securing and retaining the participation of control schools appears to have been our efforts to communicate and establish relationships with them after they were assigned the control condition. The research staff approached each of the schools differently based on the communication styles of the administrators and the characteristics of the school. We detected some common characteristics that may have influenced the behavior of the 2 administrators who

showed the most resistance. These administrators were unsure if they would still be at their schools in 3 years time and thus were concerned that the intervention program would not be received on their watch. Both schools were located in the most disadvantaged communities in the district where the need for intervention is very high, and the administrators were under heavy pressure from the district to find new and creative methods for addressing the pressing needs of their students. Finally, both schools were not making adequate yearly progress on their academic achievement tests relative to standards established in the No Child Left Behind legislation. Not surprisingly, administrators at schools that were most in need of an intervention were thus most resistant to accepting the control condition.

It took several months to extend invitations to eligible schools, to host information sessions and make multiple visits and phone calls to reach school staff and describe the intervention and the research study, and to pair schools who agreed to participate. Additional time and resources were needed to support the research staff's extensive and ongoing efforts to address the schools' threats of withdrawing after they were informed that they were assigned to the control condition. The retention of schools proved to be difficult and taxing on research staff. We were fortunate to have ample time and resources and to have research staff who were familiar with the school's needs and who had excellent communication and rapport-building skills so that we could secure the control schools' participation.

From our experience, we are able to provide some suggestions for those who are attempting to ensure the participation of control schools in an MP-RCT:

1. Provide a sufficient incentive for the control schools to want to participate—in terms of both current and future payoffs for the school.
2. Enlist research staff who are familiar with the school district and the methods of communication within the district.
3. Notify control schools of their status in person—preferably notification should be provided by a pair of staff who work well together and are capable of handling a variety of communication styles.
4. Understand the needs of the school community and help them to see how their needs may be able to be met with alternative strategies and resources.
5. Continue to communicate and establish a relationship with administrators even if they insist that they do not want to participate.
6. Look for an advocate within the school's administration team and enlist this person's support.
7. Allow time for the administrators to accept their condition while maintaining regular contact with them.

Our experience does not indicate which procedure, recruiting then randomizing schools or randomizing then recruiting schools, is likely to be more successful in recruiting schools for an MP-RCT. Both approaches would maintain randomization principles as long as both schools within a pair were dropped if either school elected not to participate. However, the approach we used is more likely to result in treatment and control groups who are equally motivated to implement the intervention with fidelity. A comparison of school recruitment procedures and participation rates across different MP-RCT studies would be helpful in clarifying which procedure is more likely to recruit the highest proportion of schools. The approaches have subtle differences that may influence the participation decisions of selected schools. Our experience, however, supports the idea that ample resources must be devoted to the recruitment and the retention of schools after they have been notified of their randomly assigned condition.

It is important to consider how the use of an MP-RCT design may affect the generalizability of study results (external validity). In our study, participation was limited to only those schools that agreed to be involved in the research, to submit to random assignment, and to use the PA program if assigned to the treatment condition. Of the schools we recruited, nearly half either could not be contacted or declined to attend the information sessions about the study that we held. Furthermore, of the schools with which we were able to present and discuss study information, only half agreed to participate. Nonparticipation was potentially nonrandom and might not have been as frequent had we used a different study design. Our examination of school demographic variables indicated that the schools participating in the study were similar to the nonparticipating schools. The analysis did not indicate, however, if the schools participating in the study were different from the larger population of schools with regard to other more subtle and less readily observable factors. These include factors that may be influential in shaping the effectiveness of a school-wide intervention program, such as a school's motivation and readiness to implement the intervention with a high degree of fidelity.[30-33] We would expect that schools that agreed to participate in the study would be more highly motivated to implement the intervention with fidelity than schools that did not agree to participate. An assessment of these types of factors among the participating and nonparticipating schools would allow researchers to make more informed judgments regarding the extent to which study results can be generalized to a larger population of schools.

Our experiences are consistent with and extend previous reports of the challenges and issues that may be encountered when recruiting schools into an MP-RCT.

There clearly is a need to review the methodological, ethical, and practical implications of using different approaches for recruiting schools into an MP-RCT. It is clear that ample time, resources, and research staff with excellent rapport with schools are essential not just for the recruitment of schools but also for retaining their participation after they have been randomly assigned to conditions. More reports of researchers' experiences would aid in further determining best practices for recruiting schools when using an MP-RCT.

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