Does Encouragement Matter in Improving Gender Imbalances in Technical Fields? Evidence from a Randomized Controlled Trial*

Cait Unkovic† Maya Sen‡ Kevin M. Quinn§

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Abstract

Education policy research looking at gender imbalances in technical fields often relies on observational data or small N experimental studies. Taking a different approach, we present the results of one of the first and largest randomized controlled trials on the topic. Using the 2014 Political Methodology Annual Meeting as our context, half of a pool of 3,945 political science graduate students were randomly assigned to receive two personalized emails encouraging them to apply to the conference ($n = 1,976$), while the other half received nothing ($n = 1,969$). We find a robust, positive effect associated with this simple intervention and suggestive evidence that women responded more strongly than men. However, we find that women's conference acceptance rates are higher within the control group than in the treated group. This is not the case for men. The reason appears to be that female applicants in the treated group solicited supporting letters at lower rates. The contributions from this research are twofold. First, our findings are among the first large-scale randomized controlled interventions in higher education. Second, and less optimistically, our findings suggest that such "low dose" interventions may promote diversity in STEM fields, but that they have the potential to expose underlying disparities when used alone or in a non-targeted way.

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†University of California-Berkeley School of Law (cait.unkovic@berkeley.edu).

‡John F. Kennedy School of Government, Harvard University (maya_sen@hks.harvard.edu).

§University of California-Berkeley School of Law (kevin_quinn@berkeley.edu).

1 Introduction

The scarcity of women in technical fields is well documented. According to the U.S. Department of Commerce, women hold close to half of all jobs in the U.S., but barely a quarter of jobs in science, technology, engineering, and mathematics (STEM). Within engineering, the shortage is even more acute, with women occupying around 14% of all jobs, a figure that has barely budged in ten years (Beede et al., 2011). Despite these patterns, however, very few studies have used modern social science methods to explore possible explanations behind these patterns. Indeed, most studies within education policy, including many studies on gender imbalances in STEM fields, either rely on observational data or, at best, small experimental designs or lab-based studies. These methodologies have produced important findings; however, these methodologies also have substantial weaknesses that make it difficult to secure credible causal inferences.

In this study, we take a different approach by applying techniques from modern experimental social science to the problem of gender in STEM. Specifically, we present the results of one of the first large-scale randomized controlled trials on the topic. We use as our experimental context applications to the Society for Political Methodology Summer Meeting ("PolMeth"). As our subject pool, we take 3,945 graduate students enrolled in the Top 50-ranked political science programs. From this subject pool, half were randomly assigned to receive two personalized emails encouraging them to apply to the conference ($n = 1,976$), while the other half received nothing ($n = 1,969$). Thus, our randomized intervention encourages some students (and not others) to apply as a way to explore whether women (1) receive less information about opportunities within STEM fields or (2) receive less encouragement about STEM activities. To our knowledge, ours is among the first large-N, randomized controlled trials that has ever been conducted to explore either of these questions.

We look at two outcomes: (a) applications to the conference and (b) acceptance into the conference. With respect to applications to the conference, we find a robust, positive effect associated with this simple intervention: those who received this simple treatment are more likely to apply than those that did not. This is the case even though our sample necessarily includes some individuals who would never apply to the conference ("never appliers") under any circumstances. We also find strong suggestive evidence that women responded more strongly than men in terms of applications (although we are not able to rule out that there is no difference between the groups). When it comes to who was admitted to the conference, the picture is more mixed. Although we cannot estimate the average effect of encouragement on acceptance (as the acceptance outcomes are not independent, due to the limited number of slots for graduate students), we find that women's conference acceptance rates are higher within the control group than in the treated group. This is not the case for men. Looking further into applications reveals the likely reason: women applicants in the treated group solicited supporting letter of recommendation at lower rates compared to the other groups.

Our contributions are twofold. First, from a methodological perspective, our findings are among the first large-scale randomized controlled interventions in higher education, illustrating an attractive application of modern causal inference tools to education policy questions. Here, we've addressed the imbalance of women in STEM fields, but large-scaled field experiments such as this one could be used to address broader education policy questions. Second, from a substantive perspective, although our findings suggest that simple, low-cost interventions may help promote gender balance in STEM fields such as political methodology, they also reveal potential problems. In particular, as our findings here elucidate, such interventions have the potential to expose underlying disparities, particularly when they are used alone or in non-targeted way. This raises additional questions for future researchers to examine.

The rest of this paper is organized as follows. In Section 2, we discuss previous education policy research, noting possible explanations that have been considered in explaining the gender imbalance in technical fields as well as some of the methodological gaps in the existing educational policy literature. We describe the research design in Section 3 and then present the core results documenting the baseline treatment effect in Section 4. Additional discussions of the potential explanation behind the result and results from a follow-up survey are presented in Sections 5 and 6. We conclude by discussing the implications of this research in Section 7, highlighting how this kind of research design could be fruitful for education policy researchers. Additional information, including examples of the interventions used, are provided in the Supplemental Information.

2 What Education Policy Research Can, and Cannot, Tell us About Gender Imbalances in STEM

Education policy research on higher education, and also on gender in STEM fields, has mostly relied on either observational work or small-N studies (with important exceptions, which we discuss below). Although we take a different approach by conducting a large-scale randomized controlled field experiment, the existing literature nonetheless highlights several factors believed to play a role in the "leaky pipeline" of women's attrition in STEM fields. Some of these concern women's own attitudes and preferences and others touch upon how women are viewed by others. However, the two are closely related: how women are perceived and treated within technical fields obviously shapes their own attitudes about pursuing math, science, and statistics.

2.1 Research on Gender & STEM

One set of studies has found that a possible explanation for the scarcity of women in technical fields is the *relative scarcity of female role models.* Some of these have found a positive relationship between the presence of female faculty and interest among undergraduates in majoring in STEM fields. For example, Bettinger and Long (2005)'s observational study finds that having female instructors does indeed increase course enrollment and major selection in some disciplines but not in others. This is a finding consistent with other observational studies such as Rask and Bailey (2002) and Ashworth and Evans (2001), who all find mostly positive relationships between the presence of female role models and female interest in technical fields. The finding is not consistent across all studies, however. For example, Canes and Rosen (1995) find no relationship between the share of the department that is female and women's choice of major at several elite universities.

Other mostly survey-based studies have explored whether women's *preferences over family, childbearing, and work-life balance* could also contribute to the shortage. For example, Goulden, Mason and Frasch (2011) survey predoctoral students in STEM fields, finding that female students are more likely to express concern about the demands on current or future family obligations posed by the pursuit of prestigious scientific careers. (They find that, conditional on having neither children nor future plans to have children, attitudes between men and women hardly differ – speaking to the possibility of unmeasured confounders in such survey-based analyses.) This is a finding echoed by Long (2001), who reports that marriage and family concerns are one of the most important factors dictating persistence in the STEM workforce. He notes that "[s]ingle men and single women participate equally in the workforce" but "[m]arriage and children are associated with increased rates of full-time employment for men, but declining rates for women." More recently, Mason, Wolfinger and Goulden (2013)'s survey-based work on academic career advancement suggests that this is an issue for women across all disciplines, not just those in technical areas. That female role models within math and science are more likely to be single or have no children perhaps makes these issues more salient.

Third, and related to the other phenomena, women are less likely to report favorably on *informal structures and networks* that would otherwise promote their research and intellectual development. Again, however, the evidence has been observational. For example, a 2010 report from the National Academy of Sciences notes that "although women reported that they were more likely to have mentors than men, they were less likely to engage in conservations with their colleagues on a wide rage of professional topics, including research, salary, and benefits." The report notes that this "distance may prevent women from accessing important information and may make them feel less included and more marginalized in their professional lives" (National Academy of Science, 2010). More informally, observations about the field we study here, political methodology, reflects a similar theme, according to one account from Achen (2014):

> In a subfield not famous for its practitioners' social skills, male insecurity can lead to clumsy combative behavior that makes the atmosphere even colder. The cumulative effect can be depressingly powerful. One need not spend much time talking to women political scientists who have attended past [Political Methodology] Summer Methods meetings to hear dreadful stories of dismissive or belittling remarks

2.2 Pitfalls with Observational and Small-N Studies

As we noted, these have been mostly observational studies, and do not document fully the potential differences between male and female students. This reflects a broader problem within education policy research. As others have argued (e.g., King and Sen, 2013), very few large-scale experiments have been done in an educational context, particularly within higher education.[1] The problems with conducting large-scale experimental studies are significant. First, randomizing treatments within a classroom makes it nearly impossible to avoid cross-contamination (and thereby SUTVA problems in terms of analyzing causal effects). Second, randomizing a treatment at the classroom level (or department or university level) results in very small N, thus leading to studies that may be underpowered for most treatment effects.

We do note some experimental inroads. First, a set of important studies have examined not differences between male and female students, but, rather, differences in how they are treated – specifically *implicit bias against women* in more technical areas. For example, Moss-Racusin et al. (2012) ask panels of scientists to review identical vitas for a presumptive

[1] For example, as King and Sen (2013) note, only one large-scale experiment on the impact of class size on student learning has been done, Chingos (2013), and that was in the context of primary education.

lab assistant position, one filed under a female name and the other with a male name; they find that CVs associated with the female name would be offered substantially less money than the one with the male name, despite the two CVs being identical in all other respects. In addition, this bias has been estimated using lab-based Implicit Association Tests, which seem to suggest that the degree of bias against women is not only pervasive, but it correlates with substantive differences in science and mathematics achievement (Nosek, 2009). For the most part, however, these are experiments that operate within a highly controlled, laboratory environment.

The second, and perhaps most apropos, arena are existing studies in behavioral economics that have used low-cost "nudge" interventions to surprising effect (Thaler and Sunstein, 2008). Although most of these studies have operated outside of the education context, a growing number have started to look at educational outcomes. For example, randomized trials have found that texting students encouraging messages increases college attendance among low-income students (Castleman and Page, 2015), that mailing personalized informational packets about the college processes increases applications from low-income families (Hoxby and Turner, 2014), and that weekly text messages sent to parents of high school students reduced the share of students who failed courses (Kraft and Rogers, 2015). As we show in the rest of this article, these sorts of experiments could be of great use to education policy research when used in a targeted way. These experiments also offer the rigor of a large-scale field design and the feasibility, from a policy perspective, of easy-to-administer of a "low-dose" interventions. However, these studies have for the most part looked at primary or high school education, rather than secondary education.

3 Experimental Protocol

In light of this existing scholarship, we isolate at least four different mechanisms explored by the existing literature, all of which are intimately intertwined: (1) lack of role models and mentoring, (2) family and life balance concerns, (3) implicit or explicit bias, and (4) exclusion and professional/social ostracism. Our experiment touches upon the two of those mechanisms, focusing specifically on the idea that women systematically receive (i) less information about important professional and intellectual opportunities, and (ii) may also receive less encouragement. Indeed, that women receive less information and encouragement about engaging in STEM areas could flow directly from an unconscious bias by existing members of the community; it could also flow from the notion that women are less well integrated (both professionally and perhaps socially) in valuable professional networks that would open up intellectual and research opportunities.

We use as our experimental background the Political Methodology Summer Conference ("PolMeth"), sponsored in part by the National Science Foundation. Hosted annually by a rotating cast of political science departments, the conference provides significant networking opportunities for graduate students and showcases research on applied statistics, text analysis, causal inference, and machine learning. We note that gender imbalances within th political methodology community reflect broader gender imbalances within STEM. Only around 25% of attendees at the annual PolMeth meeting are women (Shannon, 2014; Achen, 2014) and only around 17% of co-authors or presenters are women. We also note that applied statistics is, like training in other STEM areas, becoming increasing important within the academic job market. For example, in Appendix Table 9, we present data showing that job market candidates who specialize in political methodology face more favorable jobs-to-applicants ratios. PolMeth therefore serves as a good setting to understand the roots of gender imbalances in more technical fields.

Acceptance to PolMeth is competitive. Along with a proposal, all student applicants are requested to provide a faculty letter of support. Proposals are then subject to non-blind review by a committee of nine faculty members from across the community. Each year, the program committee has approximately 70 to 80 slots for student presenters. Table 1 presents statistics on the number and gender of previous attendees and, to the extent

Table 1: Graduate Student Applications and Acceptances to PolMeth, 2010-2014

	Applications			Acceptances		
Year	Male	Female	Gender Unknown	Male	Female	Gender Unknown
2010	–	–	–	39	21	14
2011	–	–	–	56	20	12
2012	–	–	–	44	19	4
2013	85	29	0	64	15	0
2014[2]	92	67	2	45	27	0

available, applicants.

The intervention was conducted in the Spring of 2014. To generate the potential subject pool of applicants, we collected the names and contact information of all graduate students in the Top 50 programs within political science. We did this by first identifying the 50 top-ranked programs, as determined by *U.S. News & World Report*. (Historically, the majority of conference applicants come from these programs.) We then searched each department's webpage to determine the names and email addresses of enrolled graduate students.[3] In addition, we denoted the gender of each student according to commonly used first names and/or online photographs. When gender could not be determined, this was denoted as being uncertain.

This search yielded 4,188 names and email addresses, including 2,478 men, 1,652 women, and 58 people of unknown gender across 53 departments. We subsetted these data to only those students with known gender and email address. This produced an experimental subject pool of 3,945 students; 2,348 of whom were male and 1,597 of whom were female. We note that this pool of experimental subjects included some students with little to no interest in political methodology or statistics—for example, students pursuing a humanities oriented approach to political philosophy. Indeed, approximately 10-15% of all graduate students fall into this category (American Political Science Association, 2015), which makes them "never appliers" (i.e., people for whom the treatment effect would always be zero) to the conference.

[3]Although some department webpages were out of date, to our knowledge this was random error with no systematic over- or under-reporting of names or email addresses according to gender or research area.

As we discuss below, this makes our study different from *closely targeted* studies (such as Hoxby and Turner, 2014). The primary consequence of this is that our treatment effect estimates are likely smaller than those that would have been obtained from an intervention on a more narrowly targeted population of potential applicants.

Students were then randomly assigned to either a control or treated group. Because a student's department is a predictor of conference attendance, and because the treatment effect could vary by gender, we blocked on both department and gender. We randomly assigned treatment at the individual rather than group level in order to maximize statistical power. This had the potential of leading to contamination across students (perhaps by sharing or forwarding the intervention email); we discuss this in Section 5.

Randomization produced 1,976 students assigned to treatment and 1,969 to control, bringing the total number of participants to n = 3,945. Of the 1,976 students assigned to treatment, 1,174 were men and 802 were women. In addition, 637 came from political science departments ranked in the Top 10, 611 from departments ranked 11 to 25, and 728 from departments ranked 26 to 50. Of the 1,969 students assigned to the control condition, 1,174 were men and 795 were women. In addition, 635 came from departments ranked in the Top 10, 610 from departments ranked 11 to 25, and 724 from departments ranked 26 to 50. The Supplemental Information includes additional summary statistics, including gender breakdown by department.

The intervention came in the form of two encouraging emails, one sent on March 4, 2014 and the other on March 19, 2014. Both emails were sent from the personal email account of the current president of the Society for Political Methodology, the academic organization hosting the conference, and used the student's first name for personalized encouragement. The email discussed some of the benefits of attending the conference and concluded by encouraging the student to consider applying. The email text can be found in the Supplemental Information.[4]

[4]General calls for proposals began around March 3, 2014, approximately one day before the first email intervention was sent. These included emails circulated to various email lists, including the POLMETH

Soon after the deadline for applications (March 28, 2014), we collected from the conference organizers (1) the names and affiliations of all graduate students who applied to the conference, (2) the names and affiliations of sponsoring faculty (if any), (3) proposal titles and abstracts, and (4) acceptance status. We combined these data with the data on whether the students had or had not received the treatment.

4 Results

We present results relating to two outcomes: (1) the decision by a student to apply to the conference and (2) whether the student's proposal was accepted.

4.1 Analysis of Applications to the Conference

We first examine the effect of the encouragement treatment on decisions to apply to the conference. Table 2 presents estimates of the sample average treatment effect (SATE), which in this case is the fraction of applicants among the treated students minus the fraction of applicants among the control students, averaged over gender and tier of school. This table also presents randomization-based p-values for the null hypothesis of no individual-level effect.

Table 2 shows positive, statistically significant effects of encouragement on the decision to apply for students overall and for students in all subgroups except for those students from schools ranked 11th to 25th. The overall effect of the encouragement is to increase applications by about 2.7 percentage points. Applications from men are increased by about 2.3 percentage points and applications from women are increased by about 3.2 percentage points. The treatment effects are largest for students from top 10 schools (a 3.8 percentage point increase compared to 1.8 percentage points and 2.5 percentage points for the other

listserv, which means that some portion of students likely received both the email interventions and other promotional notices. There is no reason to believe the promotional emails affected either the control or treated groups disproportionately.

Table 2: Sample Average Treatment Effects of Encouragement on Application

Subgroup	n	SATE Estimate	p-value
Full Sample	3945	0.027	<0.001
Men	2348	0.023	0.002
Women	1597	0.032	<0.001
Top 10	1272	0.038	0.003
Men Top 10	758	0.035	0.042
Women Top 10	514	0.043	0.035
Top 11 to 25	1221	0.018	0.081
Men Top 11 to 25	734	0.013	0.317
Women Top 11 to 25	487	0.024	0.204
Top 26 to 50	1452	0.025	<0.001
Men Top 26 to 50	856	0.021	0.013
Women Top 26 to 50	596	0.030	0.018

Table 3: Female - Male Heterogeneity in Sample Average Treatment Effects of Encouragement on Application

Subgroup	Male SATE	Female SATE	δ	95% CI for δ lower	95% CI for δ upper	$\Pr(\delta > 0)$
Full Sample	0.023	0.032	0.009	-0.007	0.026	0.869
Top 10	0.035	0.043	0.008	-0.028	0.044	0.669
Top 11 to 25	0.013	0.024	0.011	-0.016	0.039	0.793
Top 26 to 50	0.021	0.030	0.009	-0.013	0.031	0.794

tiers). These schools, on average, have the strongest graduate training in statistics, suggesting that our encouragement is most likely to influence students who have selected into graduate programs known for quantitative research.

Is there any evidence that treatment effects are larger for women than for men? We adopt a Bayesian approach to inference in order to answer this question[5] and assume that application decisions within each university-gender stratum are generated from a binomial distribution with a university-gender-specific probability of application. We take the prior distribution for each of these parameters to be a beta distribution with first parameter equal

[5]Inference based on the randomization distribution of treatment is not well-suited to questions regarding differences in treatment effect size between subgroups. We use a Bayesian approach due to the small number of students in some gender-school strata and the relative ease with which uncertainty statements can be constructed regarding differences in SATE. Our results do not hinge on the particulars of this approach. Qualitatively similar results can be obtained from a reasonable frequentist approach.

to 0.1 and second parameter equal to 0.1. As shown in Table 3, these Bayesian estimates of SATE are identical to the standard estimates in Table 2. The quantity labeled δ in Table 3 is the SATE for women minus the SATE for men. A positive value of δ implies that the intervention created a greater increase (in percentage point terms) for women than for men. While it is the case the point estimates of δ are positive—for the full sample and for all three tiers of schools—the 95% credible intervals include 0. Looking at the posterior probability that $\delta > 0$ we see suggestive, but not conclusive, evidence that δ is positive. The strongest evidence comes from the full sample where we calculate the posterior probability that $\delta > 0$ to be about 0.87.

Because most graduate programs do not list students by research area, our subject pool includes students whose interests do not include data analysis. Such students are unlikely to apply to an applied statistics conference regardless of encouragement. As we noted above, data from the American Political Science Association suggest that approximately 10-15% of all graduate students are involved in the study of political philosophy (American Political Science Association, 2015). We can calculate a back-of-the-envelope revised SATE conditional on the "possible appliers," which relies on the fact that the total SATE is just the weighted sum of the conditional sample average effect among the "never appliers" (NA) and conditional sample average treatment effect among the "possible appliers." In notation:

$$
\begin{aligned}
SATE &= \frac{1}{|\mathcal{I}|}\sum_{i\in\mathcal{I}}[Y_i(1) - Y_i(0)] \\
&= \frac{1}{|\mathcal{I}|}\left[\sum_{i'\in\mathcal{I}_{na}}[Y_{i'}(1) - Y_{i'}(0)] + \sum_{i''\in\mathcal{I}_a}[Y_{i''}(1) - Y_{i''}(0)]\right] \\
&= \frac{1}{|\mathcal{I}|}\left[0 + \sum_{i''\in\mathcal{I}_a}[Y_{i''}(1) - Y_{i''}(0)]\right] \\
&= \frac{1}{|\mathcal{I}|}\sum_{i''\in\mathcal{I}_a}[Y_{i''}(1) - Y_{i''}(0)] \qquad (1)
\end{aligned}
$$

where $\mathcal{I}$ is the set of all experimental subjects, $\mathcal{I}_{na}$ is the set "never appliers", and $\mathcal{I}_a$ is the set of "possible appliers". The SATE among the "possible appliers" is

$$SATE_a = \frac{1}{|\mathcal{I}_a|} \sum_{i'' \in \mathcal{I}_a} [Y_{i''}(1) - Y_{i''}(0)]. \tag{2}$$

$SATE_a$ is thus $|\mathcal{I}|/|\mathcal{I}_a|$ times larger in magnitude than the overall $SATE$.

Using the most conservative 10% "never applier" rate gives us a revised SATE of around 3 percentage points overall, and around 3.6 percentage points for women.[6][7] Thus, we have reason to think that a more closely targeted study – one which only targeted those people whom policy experts have an *a priori* reason to think might respond – will actually achieve greater treatment effects. Here, taking into account this "never applier" phenomenon increases the treatment effect by around 10%.

4.2 Analysis of Acceptances Into the Conference

We next look at the relationship between treatment status and the acceptance of an application to attend the conference. We note that the conference committee was tasked with accepting a fixed number of applicants and could not increase or decrease the number accepted. This creates conceptual problems for standard definitions of causal effects and the requirement that treatment assignment be independent of potential outcomes (Holland, 1986). We therefore present descriptive statistics on this question in Table 4. The first column reports the number of students in each subgroup. The second and third columns report the fraction of accepted students within each subgroup disaggregated by treatment status. The denominator is all students in the relevant subgroup—both those who applied

[6] Using a less conservative 15% "never applier" rate gives us a revised SATE of around 3.17 percentage points overall, and around 3.76 percentage points for women.

[7] If women comprise a relatively greater proportion of students in non-technical subfields than in subfields heavily reliant on statistical reasoning, then the differences presented in Table 3 are likely to be smaller than the male-female differences in effect sizes among the subset of potentially encourageable students. On the basis of data obtained from the American Political Science Association, we have no evidence on this; indeed, it appears from our non-systematic review of graduate programs that the political philosophy students, who might *a priori* be likely never-appliers, are more likely to be men.

Table 4: Comparison of Acceptance Rates by Treatment Status (All Students).

Subgroup	n	Fraction Accepted among Treated	Fraction Accepted among Control	p-value
Full Sample	3945	0.020	0.013	0.076
Men	2348	0.022	0.013	0.093
Women	1597	0.016	0.013	0.533
Top 10	1272	0.038	0.022	0.083
Men Top 10	758	0.042	0.021	0.077
Women Top 10	514	0.031	0.023	0.746
Top 11 to 25	1221	0.015	0.015	0.841
Men Top 11 to 25	734	0.016	0.016	0.851
Women Top 11 to 25	487	0.012	0.012	0.751
Top 26 to 50	1452	0.008	0.003	0.167
Men Top 26 to 50	856	0.009	0.002	0.338
Women Top 26 to 50	596	0.007	0.003	0.611

(and thus could have been accepted) and those who did not apply (and thus could not have been accepted). The final column reports the randomization p-value for a test of no individual level effect whatsoever. That is, the null hypothesis here is that no student's acceptance status would have been changed by changing his or her treatment status.

Looking at the acceptance rates in Table 4, we see higher acceptance rates for the treated students than for the control students, with the exception of students from departments ranked 11 to 25. There were no differences at all for this latter group of students. While we cannot reject the null hypothesis of no individual-level effect whatsoever for any subgroup at the 0.05 level, we can reject this null at the 0.10 level for the full sample, all male students, all students from top 10 schools, and male students from top 10 schools. We cannot reject the null hypothesis for any subgroup of women at any reasonable significance level. We therefore have suggestive evidence that treated students were more likely to be accepted to the conference than control students. Male students, however, appear to drive this difference.

Looking at the fraction of accepted students within the control group shows additional patterns. Here we see no major differences in acceptance rates by gender either in the full

Table 5: Comparison of Acceptance Rates Among Applicants by Treatment Status.

Applicant Subgroup	n	Fraction Accepted among Treated	Fraction Accepted among Control	p-value
Full Sample	137	0.411	0.595	0.046
Men	81	0.481	0.556	0.538
Women	56	0.317	0.667	0.015
Top 10	76	0.480	0.538	0.617
Men Top 10	49	0.516	0.444	0.771
Women Top 10	27	0.421	0.750	0.206
Top 11 to 25	35	0.391	0.750	0.026
Men Top 11 to 25	19	0.500	0.857	0.166
Women Top 11 to 25	16	0.273	0.600	0.286
Top 26 to 50	26	0.273	0.500	0.358
Men Top 26 to 50	13	0.364	0.500	1.000
Women Top 26 to 50	13	0.182	0.500	0.415

sample or within tier of school. A simple calculation ignoring statistical uncertainty suggests that, in a counterfactual world in which the outreach experiment was not conducted, the ratio of men to women accepted to attend the conference would have been about 1.47 to 1. This can be compared to the actual male:female ratio of 1.67 to 1 (see Table 1). There is thus some weak evidence that the outreach experiment may have actually *worsened* the final female-male gender balance at the conference.

Because the groups of treated and control students who applied may not be balanced due to the treatment intervention being randomly assigned prior to the submission of applications, we look at those students who applied to attend the conference by looking at descriptive data, presented in Table 5.[8] The first column of this table reports the number of students in each subgroup. The second and third columns report the fraction of accepted students within each subgroup disaggregated by treatment status. The final column reports the randomization p-value for a test that we be describe below.

The differences in acceptance rates in this table should not be interpreted causally. Not

[8]The total number of applicants in this table (137) does not equal the number of total applicants in Table 1 (161). This is the result of 24 applicants from non-U.S. institutions and non-top-50 U.S. institutions.

only is there the same concern about interference among units that was discussed in regard to Table 4, but conditioning on a post-treatment variable threatens the independence of treatment status and unmeasured confounding variables. However, we can use the data in Table 5 to infer whether the program committee selected participants from among the applicants in a way that was statistically independent of treatment status. The program committee was not given any information on the treatment status of any individual and most of the committee members were not aware of the existence of this experiment. Thus, a rejection of the null hypothesis of independence of acceptance decisions and treatment status would suggest that unmeasured background factors that entered into the committee members' decision making processes differ systematically between the control applicants and the treated applicants.

The null hypothesis we employ here assumes that the number of acceptances is fixed within each gender and tier of school combination and that these acceptances are randomly assigned to applicants within the relevant gender-tier stratum independently of treatment status.[9] The p-values in Table 5 are randomization p-values that compare the observed difference in acceptance rates to the differences that arise from the appropriate randomization distribution.

Looking at Table 5 we see that we can reject the null of independence at the 0.05 level for the full sample, for all women, and for students from programs ranked 11 to 25. In each case, the fraction of accepted applicants is higher among the control applicants than among the treated applicants. We are not able to reject the null of independence for any of the male-only subgroups. Indeed, in one of the male subgroups (men at top 10 departments) the fraction of accepted applicants is higher among the treated applicants than among the control applicants—although not significantly so.

[9] It is also possible to think of the null as fixing the total number of acceptances overall but putting no constraints on the number of acceptances within gender-tier strata. Results based on this null hypothesis are qualitatively similar to those reported in Table 5, albeit with p-values that are slightly larger. In particular, the p-value for the full sample becomes 0.066, the p-value for all women becomes 0.030, and the p-value for all top 11 to 25 students becomes 0.067.

These results suggest that there tend to be aspects of the applications submitted by treated women that differ from the applications submitted by the control women and that these unmeasured differences in applications are associated with higher acceptance rates for the control women compared to treated women. Numerous self-selection stories are possible.

5 Follow-Up Survey

After the conference, we contacted all of the 3,945 students in the study by email, requesting that they participate in an Internet-based follow up survey. Of those contacted, 1,629 students (41%) responded to at least one of the survey questions. Of these, 786 were treated students (48%) and the remaining 843 were students in the control condition.

The survey asked questions regarding each student's demographics, academic background, and experience in graduate school. The survey also asked the treated students whether, and to what extent, they forwarded the encouragement email to other students. These questions addressed the potential for spillover or contamination effects. Results from the follow-up survey suggest that this is not a serious concern in our study. Of the 786 treated students, 22 (3%) reported that they forwarded the encouragement email to other students in their department and 8 of the 786 (1%) reported that they forwarded the encouragement email to students outside their department. Only three (0.003%) treated students reported that they forwarded the email to an institutional email list. This suggests that any spillover or contamination effects were likely minimal.

The follow-up survey can also be used to examine reasons for the differences in acceptance rates in Table 5. A possible reason for the relatively greater success for females in the control condition versus the treated condition is that the treated women tended to be less objectively qualified—perhaps because they were earlier in their graduate careers and/or had taken fewer quantitative methods courses. One question on the follow-up survey asked respondents how many years they have been in their current graduate program. 26 of the treated female

Table 6: Subfields of Female Applicants (Among Survey Respondents)

Subfield	Treated Female Applicants	Control Female Applicants	Rejected Treated Female Applicants	Rejected Control Female Applicants
American	5	5	1	3
Comparative	13	4	9	0
IR	4	1	3	0
Methodology	3	0	2	0

Table 7: Letters of Recommendation of Female Applicants (Among All Female Applicants)

Letter of Rec.	Treated Female	Control Female	Rejected Treated Female	Rejected Control Female
Networked Letter	10	6	3	0
Non-Networked Letter	10	5	5	1
No Letter	21	4	20	4

applicants and 10 of the control female applicants responded to this question. While the mean number of years is slightly lower for the treated women than the control women (4.3 to 4.6) a two sample t-test of the difference is not significant at conventional levels (p-value = 0.57). Neither do we see major differences in the number of quantitative methods classes taken. All respondents report having taken 5 or more classes with the exception of one treated female applicant and one control female applicant who each reported taking 4 quantitative methods courses.

An attribute that does seem to vary systematically by treatment status of female applicants who responded to the survey is their stated area of study. Table 6 presents these data. Treated female applicants are more likely to work in the areas of comparative politics and international relations than are their control counterparts. These students constitute the bulk of the rejections among the treated female applicants. Interestingly, the only female applicants who listed quantitative methods as their main field were in the treated group. Two of these three students were rejected from the conference.

Table 8: Letters of Recommendation of Male Applicants (Among All Male Applicants)

Letter of Rec.	Treated Male	Control Male	Rejected Treated Male	Rejected Control Male
Networked Letter	22	10	4	1
Non-Networked Letter	9	6	3	2
No Letter	23	11	21	9

6 Letters of Support

The conference data also reported (1) whether a letter of support was submitted for the applicant and, if a letter was submitted, (2) which faculty member wrote it. These data are available for all 56 of the female applicants who were in the outreach experiment. Table 7 displays data on whether a student's recommender submitted a letter of recommendation before the deadline and whether that letter was from a "networked" or "non-networked" advisor. We define a "networked" advisor to be someone who is either a) a fellow of the Society for Political Methodology, which is the academic society sponsoring the conference, b) a winner of a society-sponsored award, c) a current/former officer of the Society, or d) a current/former member of a Society committee.

Table 7 shows that *the major difference between the treated and control female applicants is whether the applicant's advisor submitted a letter before the application deadline.* 51% of the treated female applicants were lacking a letter of recommendation compared to 27% of the control female applicants. While the lack of a letter of recommendation was not formally disqualifying, only 1 of the 25 female applicants without a letter was accepted to the conference. This single factor seems to explain much of the difference in acceptance rates between treated and control women. Among female applicants with letters of recommendation, we do not see a major difference in the percentage of networked versus non-networked letters. Table 8 presents equivalent data for male applicants.

7 Discussion and Conclusion

In terms of our broader contributions to the literature on gender in STEM, we find that a simple email intervention had the effect of increasing interest in a STEM-related conference. We also provide suggestive, though not conclusive, evidence that the encouragement had a stronger effect among female students. This is a straightforward, low-cost intervention, one that can be applied across other STEM areas and perhaps generate more interest in the more technical areas of the social sciences specifically. In addition, as we note above, the treatment effect associated with such interventions could be substantially strengthened if combined with close targeting of the population.

However, we note that our findings suggest that large-scale interventions have the potential to expose other problems. In our experiment, the encouragement led to increased applications among the treated female graduate students, but these new applicants failed to gain acceptance into the PolMeth conference at rates equal to either the male applicants or to female applicants in the control condition. Although other research has raised the possibility of implicit bias against female STEM participants (Moss-Racusin et al., 2012), which could possibly apply in the application-review stage here, we believe that a more compelling explanation lies in the fact that female students in the treated condition were more likely to apply to the conference without having procured a faculty letter of support. Thus, the female treated students appeared to have weaknesses in their applications that may have translated into increased rejections. This is consistent with previous research demonstrating that female STEM students may be more likely to lack mentoring and networking opportunities (National Academy of Science, 2010), which in turn is consistent with the differences in "networked" letters of support by gender seen in Tables 7 and 8. This pattern is also consistent with research showing that male students develop greater professional confidence than do female students (Ferreira, 2003).

These findings ultimately suggest that, although encouragement can be effective in engaging female students, it may intensify pre-existing imbalances—and in the process actually do a disservice to groups historically marginalized in more technical fields. More research is required to understand whether such encouragement could also be counter-productive over longer periods of time; if encouraged female students apply to STEM-related programs but are rejected at higher rates, then these negative outcomes could actually serve to suppress potential future interest. Ultimately, this research suggests that low-cost interventions, or "nudges," can increase female interest in a STEM field; however, interest alone is not sufficient to overcome more serious obstacles to participation.

More broadly, however, our contribution here is also to highlight that modern social science methods – including causal inference techniques – can be implemented in education policy more generally. Here, the intervention of interest were two personalized emails; however, education policy researchers can examine the effects of these sorts of individual-level interventions with a similar kind of large-scale field experimentation context, doing so not just at the primary or high-school level, but also within professional or doctorate education. Indeed, we believe that ours will be among many such studies coupling advances in "big data" along with rigorous causal inference techniques to answer broad questions in education policy more effectively.

8 Supplemental Information

This section includes supplementary materials for the following paper:

> Cait Unkovic; Maya Sen; and Kevin Quinn. 2015. "Does Encouragement Matter in Improving Gender Imbalances in Technical Fields? Evidence from a Randomized Controlled Trial."

Detailed replication information will be posted upon publication.

8.1 Importance of Quantitative Methods in the Job Market

Here we present additional evidence of the importance of familiarity with, or research expertise in, quantitative methods for political scientists, as reported by the American Political Science Association. We also present evidence on the number of qualified candidates per research area, which shows that candidates within quantitative methods are in some years nearly guaranteed a tenure-track position compared to candidates specializing in other areas.

	AY 2009-10		
Area	Applicants	Jobs	Applicants/Job
American Politics	178	105	1.70
International Relations	219	96	2.28
Comparative Politics	290	85	3.41
Political Philosophy	141	26	5.42
Quantitative Methods	16	4	4
	AY 2010-11		
Area	Applicants	Jobs	Applicants/Job
American Politics	214	120	1.78
International Relations	225	123	1.83
Comparative Politics	341	94	3.63
Political Philosophy	113	34	3.32
Quantitative Methods	4	10	0.4
	AY 2011-12		
Area	Applicants	Jobs	Applicants/Job
American Politics	210	122	1.72
International Relations	281	147	1.91
Comparative Politics	276	100	2.76
Political Philosophy	130	32	4.06
Quantitative Methods	9	9	1

Table 9: Number of candidates and tenure-track job posts (at Assistant Professor rank) by research area, AYs 2009-10, 2010-11, 2011-12. Source: American Political Science Association.

Table 9 shows the number of positions in various research areas of political science from 2009 to 2012, the last year for which the number of candidates was recorded. Although the number of jobs specifically calling for methodology are smaller than others, we note that nearly all of the job postings in American politics call for some expertise in quantitative methods, as do many jobs in comparative politics and international relations (indicating quantitative methods as a secondary specialty). These statistics therefore underestimate the degree to which familiarity with statistical methods matter in procuring a position.

In addition, the number of jobs in quantitative methods appears to be increasing. In the Fall of 2014, the American Political Science Association reported the following open positions for AY 2015-2016 appointments for open-rank positions:

- 27 open positions in quantitative methodology, applied statistics, data science, or mathematical modeling
- 40 open positions in political philosophy (46 including cross postings)
- 18 open positions in public law (22 including cross postings)

We also note that how competitive each field is varies greatly by the number of applicants. Here, studying quantitative methods gives applicants a clear, perhaps overwhelming edge. For example, although the number of methodologically oriented candidates was high in 2009, the numbers drop off the next two years (even though the number of jobs hold steady). Thus, candidates with research specialties in quantitative methods in 2010 and 2011 *were nearly guaranteed tenure-track positions*. American politics, which is the substantive area of political science with the strongest reliance on applied statistics (the website 538.com is a clear example of this trend) also posts strong numbers, with fairly low applicants-to-jobs ratios. This is in stark contrast with those competing for jobs the humanities-oriented political philosophy area, for whom the ratio of applicants-to-jobs is the highest, as high as 4 or 5 in some years.

8.2 Political Methodology Attendance

In this section, we provide context on attendance at the Political Methodology Conference. Table 10 tabulates the number of attendees registered to attend the conference (across both faculty and graduate students) and the share who are women from 1984 (the first year of the conference) to the present. Since the mid-to-late 1990s, women have comprised about 20 to 25% of the attendees at the conference.

Year	Males	Females	Total	%Males	%Females
1984	15	1	16	93.8%	6.3%
1985	18	0	18	100.0%	0.0%
1986	18	1	19	94.7%	5.3%
1987	16	1	17	94.1%	5.9%
1988	14	0	14	100.0%	0.0%
1989	17	1	18	94.4%	5.6%
1990	28	9	37	75.7%	24.3%
1991	53	6	59	89.8%	10.2%
1992	49	10	59	83.1%	16.9%
1993	55	12	67	82.1%	17.9%
1994	41	9	50	82.0%	18.0%
1995	44	8	52	84.6%	15.4%
1996	51	9	60	85.0%	15.0%
1997	57	15	72	79.2%	20.8%
1998	87	19	106	82.1%	17.9%
1999	88	29	117	75.2%	24.8%
2000	96	22	118	81.4%	18.6%
2001	87	20	107	81.3%	18.7%
2002	92	23	115	80.0%	20.0%
2003	91	13	104	87.5%	12.5%
2004	106	27	133	79.7%	20.3%
2005	122	32	154	79.2%	20.8%
2006	70	25	95	73.7%	26.3%
2007	135	36	171	78.9%	21.1%
2008	181	59	240	75.4%	24.6%
2009	139	37	176	79.0%	21.0%
2010	129	43	172	75.0%	25.0%
2011	139	32	171	81.3%	18.7%
2012	123	35	158	77.8%	22.2%
2013	123	32	155	79.4%	20.6%
2014	109	39	148	73.6%	26.4%

Table 10: Historical Polmeth Attendance (Faculty and Student) by Sex.

8.3 Experimental Subjects by Department

In this section, we provide data on the number of male and female graduate students in the study, broken down by home department. This is presented in Table 3.

University	Female	Male	Dept. Rank
Harvard	68	101	1
Princeton	39	96	2
Stanford	48	45	3
University of Michigan	37	40	4
Yale University	52	66	4
University of California, Berkeley	53	76	6
Columbia University	77	99	7
MIT	32	48	8
University of California, San Diego	31	75	8
Duke	28	44	10
University of California, Los Angeles	49	68	10
University of Chicago	60	75	12
University of North Carolina, Chapel Hill	35	45	13
Washington University in St. Louis	12	24	13
New York University	27	55	15
Ohio State University	27	49	15
University of Rochester	15	34	15
University of Wisconsin-Madison	32	37	15
Cornell University	41	50	19
University of Minnesota, Twin Cities	34	48	19
Northwestern University	46	55	21
The University of Texas at Austin	45	77	21
University of California, Davis	19	42	23
University of Illinois at Urbana-Champaign	26	33	23
Emory University	19	18	25
Indiana University Bloomington	36	73	25
Texas A&M University, College Station	13	19	25
Penn State University	16	30	28
University of Maryland	19	27	28
University of Pennsylvania	24	36	28
University of Washington	48	43	28
Michigan State University	16	34	32
Rice	7	23	32
Stony Brook University	13	14	32
The University of Iowa	15	22	32
Notre Dame	34	53	36
The George Washington University	24	40	36
University of Virginia	29	39	36
Vanderbilt University	21	25	36
Florida State University	14	27	40
Georgetown University	58	62	40
Johns Hopkins University	5	7	40
University of California, Irvine	29	44	40
University of Pittsburgh	15	25	40
Brown University	24	29	45
Rutgers	52	40	45
University of Colorado, Boulder	25	39	45
University of Arizona	13	16	48
University of Georgia	21	49	48
Binghamton University, State University of New York	19	45	50
Maxwell School, Syracuse University	21	30	50
University of California, Santa Barbara	23	34	50
University of Florida	11	23	50
TOTAL	1597	2348	

Table 11: Gender of Students in the Study by University.

8.4 Treated/Control Subjects by Department

In this section, we provide data on the treatment status of students, broken down by home department and gender. This is presented in Table 4.

University	Treated Female	Control Female	Treated Male	Control Male	Dept. Rank
Harvard	34	34	51	50	1
Princeton	20	19	48	48	2
Stanford	24	24	23	22	3
University of Michigan	19	18	20	20	4
Yale University	26	26	33	33	4
University of California, Berkeley	26	27	38	38	6
Columbia University	39	38	49	50	7
MIT	16	16	24	24	8
University of California, San Diego	15	16	37	38	8
Duke	14	14	22	22	10
University of California, Los Angeles	25	24	34	34	10
University of Chicago	30	30	37	38	12
University of North Carolina, Chapel Hill	18	17	23	22	13
Washington University in St. Louis	6	6	12	12	13
New York University	14	13	28	27	15
Ohio State University	14	13	24	25	15
University of Rochester	8	7	17	17	15
University of Wisconsin-Madison	16	16	18	19	15
Cornell University	20	21	25	25	19
University of Minnesota, Twin Cities	17	17	24	24	19
Northwestern University	23	23	27	28	21
The University of Texas at Austin	22	23	39	38	21
University of California, Davis	9	10	21	21	23
University of Illinois at Urbana-Champaign	13	13	17	16	23
Emory University	10	9	9	9	25
Indiana University Bloomington	18	18	36	37	25
Texas A&M University, College Station	6	7	10	9	25
Penn State University	8	8	15	15	28
University of Maryland	9	10	14	13	28
University of Pennsylvania	12	12	18	18	28
University of Washington	24	24	21	22	28
Michigan State University	8	8	17	17	32
Rice	3	4	11	12	32
Stony Brook University	6	7	7	7	32
The University of Iowa	7	8	11	11	32
Notre Dame	17	17	26	27	36
The George Washington University	12	12	20	20	36
University of Virginia	15	14	20	19	36
Vanderbilt University	11	10	13	12	36
Florida State University	7	7	14	13	40
Georgetown University	29	29	31	31	40
Johns Hopkins University	3	2	4	3	40
University of California, Irvine	15	14	22	22	40
University of Pittsburgh	7	8	12	13	40
Brown University	12	12	14	15	45
Rutgers	26	26	20	20	45
University of Colorado, Boulder	13	12	19	20	45
University of Arizona	7	6	8	8	48
University of Georgia	11	10	25	24	48
Binghamton University, State University of New York	10	9	23	22	50
Maxwell School, Syracuse University	11	10	15	15	50
University of California, Santa Barbara	12	11	17	17	50
University of Florida	5	6	11	12	50
TOTAL	802	795	1174	1174	

Table 12: Treatment Status of Students in the Study by Gender and University.

9 Intervention Email Text

Here we include the actual text sent in the two email interventions. The first email intervention was sent on March 4, 2014. The second email interventions was sent on March 19, 2014. Both emails were sent from the personal account of the President of the Society for Political Methodology and were addressed in a personalized fashion for each recipient.

9.1 First Email

```
Dear [Student's First Name],

I'm writing to you on behalf of the Society for Political Methodology
to encourage you to consider submitting a proposal for a poster
presentation at the 2014 Political Methodology Summer Meeting
(PolMeth).

To give you some background, the PolMeth summer meeting provides an
exciting opportunity for students at all stages of their graduate
careers. The conference is small and focused, bringing together
approximately 150 faculty and graduate students from across political
science. The highlight of the conference is the graduate student
poster session, in which student participants can expect to receive
high-quality, detailed feedback from leading scholars in the field.
Many successful political scientists attended PolMeth as graduate
students, and many credit this experience as being an important
catalyst to their careers.

The Society for Political Methodology encourages poster proposals from
all fields of political science, and especially welcomes submissions
from comparative politics, international relations, race and ethnic
politics, and gender and politics. We encourage submissions that focus
on applied substantive topics as well as on methodological
innovations.

I am also pleased to announce that the Society for Political
Methodology will be able to offer some graduate student participants
support for travel and lodging as well as conference registration
fees.

The application can be completed at: [Conference URL]
information about the conference can be found at: [Conference URL]
I hope you will consider submitting a poster proposal to the 2014
Summer Meeting.

If you should have any questions, please contact me at
[Quinn Email].
```

Please note that the deadline for applications is Friday, March 28 at
11:59 pm Eastern.

Sincerely,
Kevin Quinn

Kevin Quinn
Professor of Law
UC Berkeley School of Law
490 Simon #7200
University of California, Berkeley
Berkeley, CA 94720-7200
- - - - - - - - - - - - - - - - - - -
President
The Society for Political Methodology

9.2 Second Email

Dear [Student's First Name],

I wrote to you about two weeks ago to encourage you to consider
submitting a proposal for a poster presentation at the 2014 Political
Methodology Summer Meeting (PolMeth). If you have submitted a
proposal, thank you. If you have not, I hope you will still consider
submitting a proposal.

To again give you some background, the PolMeth conference provides an
exciting opportunity for students at all stages of their graduate
careers. The conference is small and focused, bringing together
approximately 150 faculty and graduate students from across political
science. The highlight of the conference is the graduate student
poster session, in which student participants can expect to receive
high-quality, detailed feedback from leading scholars in the field.
Many successful political scientists attended PolMeth as graduate
students, and many credit this experience as being an important
catalyst to their careers.

The Society for Political Methodology encourages poster proposals from
all fields of political science, and especially welcomes submissions
from comparative politics, international relations, race and ethnic
politics, and gender and politics. We encourage submissions that focus
on applied substantive topics as well as on methodological
innovations.

I am also pleased to announce that the Society for Political
Methodology will be able to offer some graduate student participants
support for travel and lodging as well as conference registration
fees.

The application can be completed at:

[Conference URL]

and more information about the conference can be found at:

[Conference URL]

I hope you will consider submitting a poster proposal to the 2014 Summer Meeting. If you should have any questions, please contact me at [Quinn Email].

Please note that the deadline for applications is Friday, March 28 at 11:59 pm Eastern.

Sincerely,
Kevin Quinn

Kevin Quinn
Professor of Law
UC Berkeley School of Law
490 Simon #7200
University of California, Berkeley
Berkeley, CA 94720-7200
- - - - - - - - - - - - - - - - - - -
President
The Society for Political Methodology
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10 Responses to the Email Intervention

In some instances, students wrote back to us in response after both emails were sent, some to express enthusiasm or frustration and others to ask questions. Here we present some selected responses to the email intervention. These are intended to give a flavor of the kinds of responses and do not represent any kind of systematic analysis.

In terms of our protocol for responding, we did not respond to emails that simply wrote back with an acknowledgment or a stated expression that the student would/would not submit a proposal (for example Emails #1, 2, and 3, below). However, following the protocol followed by Hoxby and Turner (2014), we did answer specific questions with a brief email with stock phrases that were consistently used from email to email. An example of our style of response is presented with regards to Email #4, below. We have no reason to think that our subsequent (non-randomized) interactions with responding students at all affect the inferences presented in the main text. We do note, however, that a larger number of email responses appear to have come from men.

10.1 Email #1

```
Dear Professor Quinn,

Thank you for alerting my attention to the conference.
Unfortunately, I am unable to attend.

Sincerely,
[Student's Name]
```

10.2 Email #2

```
Hello Kevin,

I appreciate your invitation to participate in this summer's Political Methodology Meeting.
I am, however, a theorist and one whose approach to scholarly research
is firmly rooted in the liberal arts tradition. Still, many thanks for offering
me an opportunity to join this year's conference. I hope you and the other
attendees enjoy your stay in Athens, Georgia.
```

Best,
[Student's Name]

10.3 Email #3

Hi Kevin,

Thank you for the email and all the great information.
I intend on submitting a proposal in the next couple weeks!

Best,
[Student's Name]

10.4 Email #4

Dear Kevin,

Thank you for the information about PolMeth. I'd be interested in submitting
a proposal on a paper that I am currently working on, but I was
wondering how polished should a proposal for Polmeth presentation be.
I do have a baseline empirical design and some findings and
also hope to have a better draft with more rigorous evidence by summer,
but the current version definitely needs more time than just a few weeks.

Thanks for your time in advance!

Best,
[Student's Name]

10.5 Response to Email #4

Dear [Student's Name],

I'm glad to hear of your interest in the conference.

It's hard for me to say whether your paper is sufficiently
polished to present. I suggest you talk to your dissertation
advisor about this. He or she should be able to give you good advice.
Good luck.

Best,
KQ

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