

Referee Report for EJ Manuscript #2008152
“Studying Abroad and the Effect on International Labor Market Mobility”

SUMMARY

This paper offers a direct test of the ability of policy to influence international labor mobility. Specifically, the authors evaluate the impact of the EU’s ERASMUS program—which provides scholarships for international study—on the probability of subsequent employment in a foreign country. The authors are able to use variation in the availability of scholarships under ERASMUS to identify to effect of studying abroad on subsequent foreign employment in a two-stage least squares model. Consequently, their analysis yields an estimate of the causal effect of study abroad on foreign employment, a parameter that is of more general interest than the ERASMUS program’s particular impacts. The authors match a carefully collected data set on ERASMUS availability by year, institution and department to longitudinal data on German university graduates. They find IV estimates of the causal impact of study abroad that are considerably larger—roughly four times larger—than their OLS estimate of a six percentage point difference in foreign employment across individuals with and without study abroad experience. They conclude that their results are robust to a number of alternative specifications and explanations, and discuss possible mechanisms behind the study abroad-work abroad relationship.

MAJOR COMMENTS

1. *Making the identification more credible.* The authors take great care to persuade the reader that the ERASMUS ratio is not driven by student demand for study abroad. (See also comment 2 below and minor comment 2.) But the ERASMUS dummy is still the most credibly exogenous measure in the current paper, and the authors should present the dummy results first for this reason.

The dummy is the most credible because, as the authors explain, the timing of the introduction of ERASMUS into a department is uncertain due to the lengthy application process to the EU. While it may be the case that departments where students are most likely to study abroad apply first, the departmental fixed effects will absorb this average difference in student tastes across departments. The authors should, however, also report results from a collapsed before-and-after specification like that in Bertrand, Duflo and Mullainathan (<http://www.economics.harvard.edu/faculty/mullainathan/files/trustdifferences.pdf>)

The problem the authors cannot circumvent with fixed effects is that students may endogenously choose departments on the basis of study abroad opportunities. The authors argue that students do not do this, but a more compelling response would be to just use an IV that doesn’t suffer from this problem.

There are a number of other, more credibly exogenous measures that come to mind. The downside is that these are less transparent than either the ratio or the dummy. They are all preferable to the ratio, however, and if they produce similar results to the ratio the authors should consider substituting the new results for the ratio results.

Ideas for these alternative instruments include:

- Number of ERASMUS slots in an individual's uni-year, minus own department. This avoids problems with students' endogenous choice of departments but not of uni's. This is similar to the instrument for local labor demand developed by Bartik (*Who Benefits from State and Local Development Policies*, 1991) and used by Blanchard and Katz (*Brookings*, 1992), Saks (*Journal of Urban*, forth.?) and others.
- Number of ERASMUS slots in field-year minus own uni's department. This uses the network effects mentioned in the text to generate variation in slots but again avoids problems with students at a given uni choosing departments according to study abroad options.
- Distance weighted number of ERASMUS slots surrounding home town in third year of university.

Note that these should all probably be normalized by something—maybe overall year cohort size.

These alternatives are less transparent than the dummy, but preferable to using the number of slots in an endogenously chosen department. The best order for presenting the IV results is then the following: ERASMUS dummy; one of the alternative adjusted ratios above; and ERASMUS ratio.

2. *Clarify background.* Additional clarification of the process of selecting a university and department in Germany, as well as the department's process of joining ERASMUS adding scholarships, would bolster the authors' argument that ERASMUS slots are exogenous.

Specific points to address include:

Clarification of uni and major (department) selection in German universities – How easy is it for a typical student to get into her first choice of unis? Of departments? Do students apply to the uni and department together (British-style) or to the uni and then choose a department once in (American-style)? How hard/easy is it to switch unis/departments after entry?

Clarification of scholarship process: How is the initial number of scholarships decided when a department first joins? What happens if a dept doesn't fill its slots one year? How are additional scholarships allocated?

3. *Present the reduced form.* The authors report IV estimates that are considerably larger than OLS. The reduced form estimates could in this case help readers assess the plausibility of these large IV estimates. If the authors find significant RF impacts of ERASMUS on working abroad, this to my mind makes large IV estimates more plausible since large causal effects would be expected to produce significant RF results. The flip side to the usefulness of the RF specification is that it may suggest to some readers that there are trends in work abroad that correlate with ERASMUS, but this is already a concern with any IV strategy and the authors do a nice job of including sufficient time trends that these concerns can be put to rest.

4. *Assessing heterogeneous treatment effects.* Because of endogenous student choice of departments, allowing the effect of ERASMUS to differ across departments/majors is not a compelling way to assess heterogeneous TEs (discussed on p. 21). These identify the effect of ERASMUS on groups (departments) that differ according to unobserved characteristics of students. It again just raises the specter of contamination through students choosing departments in response to ERASMUS availability. If students differ across departments—with some departments attracting students who will be more strongly affected by ERASMUS—then why should we believe that students who were on the margin between departments wouldn't change their choice once a program that will benefit them differentially more becomes available in the non-chosen department?

I recommend dropping the specifications that include ERASMUS interacted with subject. Instead, the authors should use the reduced form to assess the plausibility of the large IV estimates as discussed above and expand the credit constraint analysis in Table 10. The hetero TEs across credit constraint groups is much more compelling, since it is very unlikely that students change credit groups with the advent of ERASMUS. In addition to Table 10, it would be useful to see 2SLS results estimated separately on constrained and unconstrained students—this would allow readers to see larger causal estimates for the latter group directly.

5. *Clarify motivation.* In general the paper is well written, but authors should be more clear about whether this is a policy evaluation of ERASMUS with application to international labor mobility (ILM), or a study of one potential cause of ILM using ERASMUS as identifying variation.

MINOR COMMENTS

1. The paper's organization would be improved by having a separate section early on with all the details of the ERASMUS program. This information is currently split between Sections 2 and 3. This would also allow the authors to shorten Section 3.
2. Clarify the definition of the ERASMUS ratio. Is this the number of scholarships divided by the number of students in the department-uni-year? Or is the denominator something else? The text says it is the number of students "competing" for the scholarships, which connotes an application process into which only some of the department's students will select.
3. Do the authors have any sense of why the non-ERASMUS predictors of study abroad differ across destinations in Table 3—especially parent education?