# International Institutions and Political Liberalization: Evidence from the World Bank Loans Program

*Appendix of Supporting Information*
*(Not for publication)*

# A  Complete World Bank income classification schedule

The following classifications and implications apply (quotes from the World Bank Income Classification spreadsheet from the Open Data program website, `http://data.worldbank.org/about/country-classifications`):

- At $755 GNI/capita in 2000 is the transition from civil-works eligible to ineligible and lower-income to lower-middle income. States crossing this threshold are no longer eligible for civil works preference "granting civil works preference to eligible domestic contractors in evaluating civil works bids procured under international competitive bidding."

- At $885 GNI/capita in 2000 we have the following: "beginning in FY94, [this was] implemented as the effective operational cutoff for [International Development Association (IDA) funds] eligibility," where IDA funds are "deeply concessional...interest-free loans and grants for programs aimed at boosting economic growth and improving living conditions."

- At $1445 GNI/capita in 2000 is a ceiling fully disqualifying a country for IDA funds. Countries also lose eligibility for 20-year IBRD terms.

- At $2995 GNI/capita in 2000, countries lose eligibility for 17-year IBRD terms and move from lower-middle to upper-middle income status. They still maintain eligibility for 15-year IBRD terms up until graduation.

- At $5225 GNI/capita in 2000 is a trigger to initiate graduation.

- At $9265 GNI/capita in 2000 countries transition from upper-middle to upper income status.

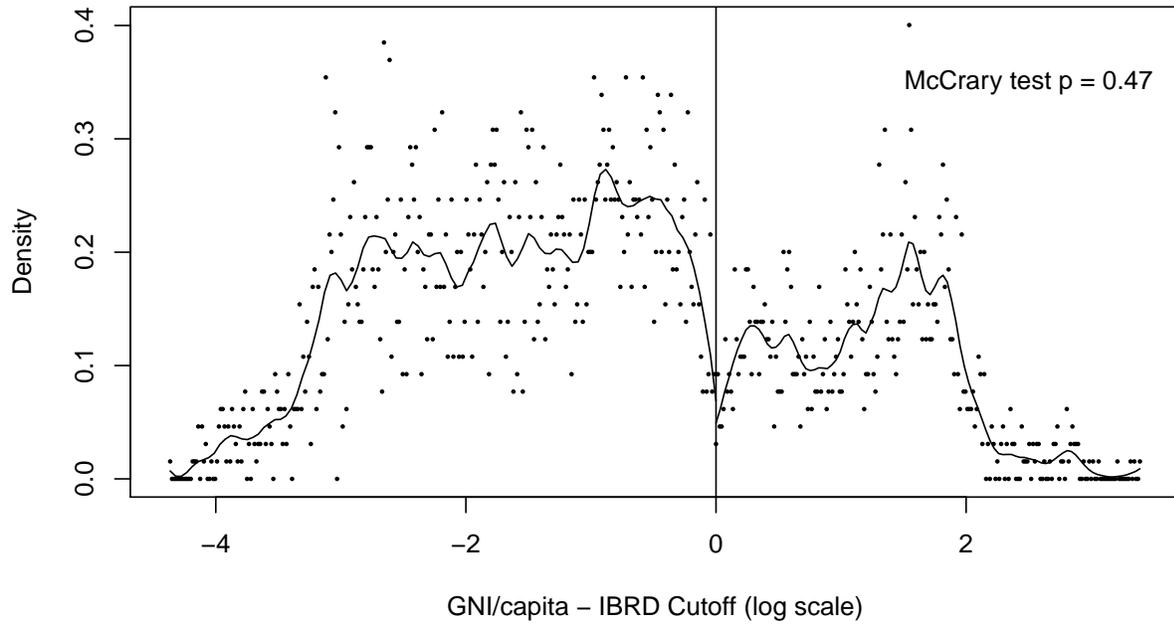# B  Checking for manipulation of GNI per capita values



Figure 1: Graphical output from McCrary (2008)'s density test for sorting. $x$-axis is on the log scale, centered at the IBRD graduation eligibility threshold. Test $p$-value $> .90$.
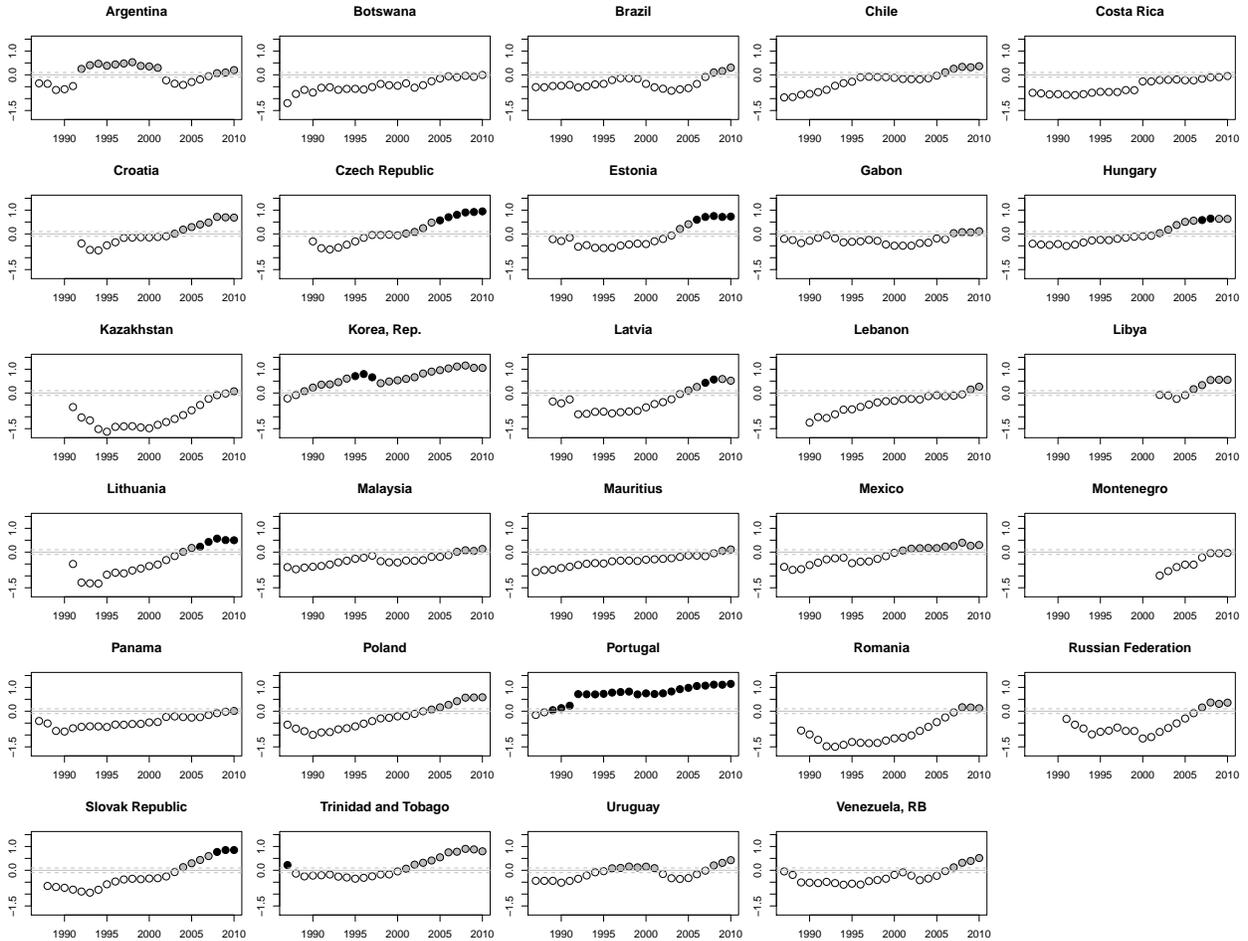
Figure 2: Income trajectories for all countries that have approached (within 0.1 log(GNI/capita)) the IBRD graduation eligibility income threshold since 1987. Points are colored white for years when income is below the threshold, gray for years when the country is above the threshold and thus eligible, but not yet graduated, and black for years when the country has graduated. The 0.1 bandwidth is indicated by the dashed lines. The graphs show that it is rare for countries to cross the graduation eligibility threshold more than once, although Argentina, Trinidad and Tobago, and Uruguay did so. Income on the *y*-axis is measured in terms of log GNI/capita in 2000 dollars, standardized to the standard deviation of IBRD graduation-ineligible countries. There is no visible pattern of sorting around the threshold, along the lines of the result of the test presented in Figure 1.

# C Estimation details

Under the local linear specification, we have,

$$Y_{i,t+\tau} - Y_{i,\underline{t}} = \beta_{0,\tau} + \beta_{1,\tau}Z_{it} + \gamma_\tau \tilde{X}_{it} + \lambda_\tau \tilde{X}_{it}Z_{it} + v_{\tau,it}, \tag{1}$$

for $i$ inside the bandwidth, where $v_{\tau,it}$ is a mean zero error. The regression discontinuity estimate of the conditional treatment effect (conditional on $\tilde{X}_{it} = 0$, that is, being located at the cut point) is given by $\beta_{1,\tau}$. The other coefficients are nuisance terms: $\beta_{0,\tau}$ captures mean outcomes under the non-eligibility control condition at the cut point, $\gamma_\tau$ captures the slope of the outcome over values of $\tilde{X}_{it}$ just to the left of the cut point, and $\lambda_\tau$ captures how the slope to the right of the cut point differs from the slope to the left of the cut point.

Given the tight bandwidth and correspondingly low number of observations, we fit the local linear regressions with a rectangular kernel.[1] For inference, we account for likely serial correlation in outcomes as well as $X_{it}$ (and thus in $Z_{it}$) by estimating cluster-robust standard errors clustered by $i$.

In our analysis of alternative explanations, we also study effects on outcome variances (rather than means) using an extension of the local linear regression approach. We begin with the variance decomposition,

$$\text{Var}\left[Y_{i,t+\tau}|Z_{it},\tilde{X}_{it}\right] = \text{E}\left[Y_{i,t+\tau}^2|Z_{it},\tilde{X}_{it}\right] - \left\{\text{E}\left[Y_{i,t+\tau}|Z_{it},\tilde{X}_{it}\right]\right\}^2.$$

A working model for $\text{E}\left[Y_{i,t+\tau}|Z_{it},\tilde{X}_{it}\right]$ is given by using expression (1), dropping $Y_{i,\underline{t}}$ from the left hand side (i.e., a levels rather than a changes model), and then taking the expectation. A working linear approximation for $\text{E}\left[Y_{i,t+\tau}^2|Z_{it},\tilde{X}_{it}\right]$ is given by

$$\text{E}\left[Y_{i,t+\tau}^2|Z_{it},\tilde{X}_{it}\right] = \alpha_{0,\tau} + \alpha_{1,\tau}Z_{it} + \alpha_{2,\tau}\tilde{X}_{it} + \alpha_{3,\tau}Z_{it}\tilde{X}_{it}.$$

Substituting the linear approximations into the variance decomposition, the difference in variances at the cut point equals (after some algebra),

$$\theta_\tau \equiv \alpha_{1,\tau} - 2\beta_{0,\tau,l}\beta_{1,\tau,l} - \beta_{1,\tau,l}^2$$

where the $\beta_{k,\tau,l}$ refers to coefficients from the levels version of (1) (with $Y_{i,\underline{t}}$ dropped). For countries with log-income equal to $c_t$, $\theta_\tau$ estimates the effect of being graduation-eligible versus graduation-ineligible on the *variance* of outcomes in period $t + \tau$. We fit the models for $\text{E}\left[Y_{i,t+\tau}|Z_{it},\tilde{X}_{it}\right]$ and $\text{E}\left[Y_{i,t+\tau}^2|Z_{it},\tilde{X}_{it}\right]$ jointly using least squares.

Specifically, we estimate the $\alpha$ and $\beta$ coefficient vectors using least squares on the stacked data (with any kernel weighting as defined above for the local linear approximations), where the stacked data takes the form:

$$\begin{pmatrix} Y \\ Y^2 \end{pmatrix}_{2N \times 1}, \begin{pmatrix} 1 & X & Z & XZ & 0 & 0 & 0 & 0 \\ 1 & 0 & 0 & 0 & 1 & X & Z & XZ \end{pmatrix}_{2N \times 8},$$

equivalent to the first stage of a "seemingly unrelated regression." Call this the "large regression." Then,

$$\hat{\theta}_\tau = \hat{\alpha}_{1,\tau} - 2\hat{\beta}_{0,\tau,l}\hat{\beta}_{1,\tau,l} - \hat{\beta}_{1,\tau,l}^2 \equiv f\begin{pmatrix} \hat{\alpha}_{1,\tau} \\ \hat{\beta}_{0,\tau,l} \\ \hat{\beta}_{1,\tau,l} \end{pmatrix},$$

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[1]Imbens and Lemieux 2008.

where the right hand side estimates are the least squares estimates with,

$$\text{Cov} \begin{pmatrix} \hat{\alpha}_{1,\tau} \\ \hat{\beta}_{0,\tau,l} \\ \hat{\beta}_{1,\tau,l} \end{pmatrix} = \Sigma.$$

A consistent estimate, $\hat{\Sigma}$, is available from relevant portions of the coefficient covariance matrix from the large regression, applying the usual cluster- and heteroskedasticity-robust methods. By the multivariate delta method, a linearized approximation of the variance of $\hat{\theta}_\tau$ is given by,

$$\text{Var}\left[\hat{\theta}_\tau\right] \approx \nabla f' \Sigma \nabla f.$$

Let $\hat{f} = (\hat{\alpha}, \hat{\beta}_0, \hat{\beta}_1)'$ Substituting the sample estimates yields a consistent estimate of this variance approximation:

$$\hat{V}\left[\hat{\theta}_\tau\right] = \nabla \hat{f}' \hat{\Sigma} \nabla \hat{f}.$$

The standard error for $\hat{\theta}_\tau$ is obtained from the square root of this variance approximation.

# D   Case table for main results

(See next page.)

| # | Placebo Country | Year | Grad. elig. | Instantaneous Country | Year | Grad. elig. | One-yr. out Country | Year | Grad. elig. | Two-yrs. out Country | Year | Grad. elig. | Three yrs. out Country | Year | Grad. elig. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1 | Argentina | 2007 | 0 | Argentina | 2007 | 0 | Argentina | 2007 | 0 | Botswana | 2006 | 0 | Chile | 1996 | 0 |
| 2 | Brazil | 2007 | 0 | Brazil | 2007 | 0 | Brazil | 2007 | 0 | Chile | 1996 | 0 | Chile | 1997 | 0 |
| 3 | Botswana | 2006 | 0 | Botswana | 2006 | 0 | Botswana | 2006 | 0 | Chile | 1997 | 0 | Chile | 1998 | 0 |
| 4 | Botswana | 2008 | 0 | Botswana | 2008 | 0 | Chile | 1996 | 0 | Chile | 1998 | 0 | Chile | 1999 | 0 |
| 5 | Botswana | 2009 | 0 | Chile | 1996 | 0 | Chile | 1998 | 0 | Chile | 1999 | 0 | Chile | 2005 | 0 |
| 6 | Botswana | 2010 | 0 | Chile | 1997 | 0 | Chile | 1999 | 0 | Czech Republic | 1997 | 0 | Czech Republic | 1997 | 0 |
| 7 | Chile | 1996 | 0 | Chile | 1998 | 0 | Chile | 2005 | 0 | Czech Republic | 1998 | 0 | Czech Republic | 1998 | 0 |
| 8 | Chile | 1997 | 0 | Chile | 1999 | 0 | Czech Republic | 1997 | 0 | Czech Republic | 1999 | 0 | Czech Republic | 1999 | 0 |
| 9 | Chile | 1998 | 0 | Chile | 2005 | 0 | Czech Republic | 1998 | 0 | Czech Republic | 2000 | 0 | Czech Republic | 2000 | 0 |
| 10 | Chile | 1999 | 0 | Czech Republic | 1997 | 0 | Czech Republic | 1999 | 0 | Estonia | 2003 | 0 | Estonia | 2003 | 0 |
| 11 | Chile | 2005 | 0 | Czech Republic | 1998 | 0 | Czech Republic | 2000 | 0 | Gabon | 1992 | 0 | Gabon | 1992 | 0 |
| 12 | Costa Rica | 2009 | 0 | Czech Republic | 1999 | 0 | Estonia | 2003 | 0 | Croatia | 2002 | 0 | Croatia | 2002 | 0 |
| 13 | Costa Rica | 2010 | 0 | Czech Republic | 2000 | 0 | Croatia | 1992 | 0 | Hungary | 2000 | 0 | Hungary | 2000 | 0 |
| 14 | Czech Republic | 1997 | 0 | Estonia | 2003 | 0 | Gabon | 2000 | 0 | Hungary | 2001 | 0 | Hungary | 2001 | 0 |
| 15 | Czech Republic | 1998 | 0 | Gabon | 1992 | 0 | Croatia | 2003 | 0 | St. Kitts and Nevis | 1992 | 0 | St. Kitts and Nevis | 1992 | 0 |
| 16 | Czech Republic | 1999 | 0 | Croatia | 2002 | 0 | Hungary | 2002 | 0 | St. Kitts and Nevis | 1993 | 0 | St. Kitts and Nevis | 1993 | 0 |
| 17 | Czech Republic | 2000 | 0 | Hungary | 2000 | 0 | Hungary | 2001 | 0 | St. Kitts and Nevis | 1994 | 0 | St. Kitts and Nevis | 1994 | 0 |
| 18 | Estonia | 2003 | 0 | Hungary | 2001 | 0 | St. Kitts and Nevis | 1992 | 0 | Korea, Rep. | 1988 | 0 | Korea, Rep. | 1988 | 0 |
| 19 | Gabon | 1992 | 0 | Kazakhstan | 2008 | 0 | St. Kitts and Nevis | 1993 | 0 | Lebanon | 2005 | 0 | Lebanon | 2005 | 0 |
| 20 | Croatia | 2002 | 0 | St. Kitts and Nevis | 1992 | 0 | St. Kitts and Nevis | 1994 | 0 | Libya | 2002 | 0 | Libya | 2002 | 0 |
| 21 | Hungary | 2000 | 0 | St. Kitts and Nevis | 1993 | 0 | Korea, Rep. | 1988 | 0 | Libya | 2005 | 0 | Libya | 2005 | 0 |
| 22 | Hungary | 2001 | 0 | St. Kitts and Nevis | 1994 | 0 | Lebanon | 2005 | 0 | Latvia | 2004 | 0 | Latvia | 2004 | 0 |
| 23 | Kazakhstan | 2008 | 0 | Korea, Rep. | 1988 | 0 | Libya | 1992 | 0 | Mexico | 2000 | 0 | Mexico | 2000 | 0 |
| 24 | Kazakhstan | 2009 | 0 | Lebanon | 2005 | 0 | Libya | 1994 | 0 | Poland | 2003 | 0 | Poland | 2003 | 0 |
| 25 | St. Kitts and Nevis | 1992 | 0 | Lebanon | 2008 | 0 | Latvia | 2005 | 0 | Portugal | 1988 | 0 | Portugal | 1988 | 0 |
| 26 | St. Kitts and Nevis | 1993 | 0 | Libya | 2002 | 0 | Mexico | 2000 | 0 | Russian Federation | 2006 | 0 | Slovak Republic | 2003 | 0 |
| 27 | St. Kitts and Nevis | 1994 | 0 | Libya | 2005 | 0 | Poland | 2004 | 0 | Slovak Republic | 2003 | 0 | Seychelles | 1987 | 0 |
| 28 | Lebanon | 2005 | 0 | Latvia | 2004 | 0 | Portugal | 2003 | 0 | Seychelles | 1988 | 0 | Seychelles | 1988 | 0 |
| 29 | Lebanon | 2008 | 0 | Mexico | 2000 | 0 | Romania | 1988 | 0 | Seychelles | 1988 | 0 | Trinidad and Tobago | 2000 | 0 |
| 30 | Libya | 2002 | 0 | Montenegro | 2008 | 0 | Russian Federation | 2006 | 0 | Trinidad and Tobago | 1987 | 0 | Uruguay | 1994 | 0 |
| 31 | Libya | 2005 | 0 | Mauritius | 2008 | 0 | Slovak Republic | 2008 | 0 | Uruguay | 2000 | 0 | Uruguay | 1995 | 0 |
| 32 | Latvia | 2004 | 0 | Panama | 2005 | 0 | Seychelles | 1988 | 0 | Uruguay | 1994 | 0 | Venezuela, RB | 1987 | 0 |
| 33 | Mexico | 2000 | 0 | Poland | 2004 | 0 | Seychelles | 2003 | 0 | Mexico | 1995 | 0 | Venezuela, RB | 2001 | 0 |
| 34 | Montenegro | 2008 | 0 | Portugal | 1988 | 0 | Trinidad and Tobago | 2000 | 1 | Poland | 1988 | 1 | Antigua and Barbuda | 1990 | 1 |
| 35 | Montenegro | 2009 | 0 | Romania | 2007 | 0 | Uruguay | 1994 | 1 | Portugal | 2002 | 1 | Antigua and Barbuda | 1991 | 1 |
| 36 | Montenegro | 2010 | 0 | Russian Federation | 2006 | 0 | Uruguay | 1995 | 1 | Russian Federation | 2006 | 1 | Antigua and Barbuda | 1992 | 1 |
| 37 | Mauritius | 2008 | 0 | Slovak Republic | 2003 | 0 | Venezuela, RB | 1987 | 1 | Slovak Republic | 2003 | 1 | Czech Republic | 2001 | 1 |
| 38 | Panama | 2008 | 0 | Seychelles | 1987 | 0 | Venezuela, RB | 1988 | 1 | Seychelles | 1988 | 1 | Czech Republic | 2002 | 1 |
| 39 | Panama | 2009 | 0 | Seychelles | 1988 | 0 | Venezuela, RB | 2000 | 1 | Seychelles | 1992 | 1 | Croatia | 2003 | 1 |
| 40 | Poland | 2003 | 0 | Trinidad and Tobago | 2000 | 0 | Antigua and Barbuda | 1994 | 1 | Czech Republic | 2001 | 1 | Hungary | 2002 | 1 |
| 41 | Romania | 2007 | 0 | Uruguay | 1994 | 0 | Antigua and Barbuda | 2006 | 1 | Czech Republic | 2002 | 1 | St. Kitts and Nevis | 1996 | 1 |
| 42 | Russian Federation | 2006 | 0 | Uruguay | 1995 | 0 | Antigua and Barbuda | 2003 | 1 | Croatia | 2003 | 1 | Korea, Rep. | 1989 | 1 |
| 43 | Slovak Republic | 2003 | 0 | Uruguay | 2007 | 0 | Czech Republic | 2007 | 1 | Hungary | 2002 | 1 | Lithuania | 2004 | 1 |
| 44 | Trinidad and Tobago | 2000 | 0 | Venezuela, RB | 1987 | 1 | Czech Republic | 2001 | 1 | St. Kitts and Nevis | 2001 | 1 | Mexico | 2001 | 1 |
| 45 | Uruguay | 1994 | 0 | Venezuela, RB | 1988 | 1 | Gabon | 2002 | 1 | Korea, Rep. | 1989 | 1 | Poland | 2004 | 1 |
| 46 | Uruguay | 1995 | 0 | Venezuela, RB | 2000 | 1 | Croatia | 2006 | 1 | Lithuania | 2004 | 1 | Portugal | 1989 | 1 |
| 47 | Uruguay | 2007 | 0 | Argentina | 2008 | 1 | Hungary | 2008 | 1 | Mexico | 2001 | 1 | Seychelles | 1989 | 1 |
| 48 | Venezuela, RB | 2001 | 1 | Antigua and Barbuda | 1990 | 1 | St. Kitts and Nevis | 1990 | 1 | Poland | 2004 | 1 | Seychelles | 1990 | 1 |
| 49 | Venezuela, RB | 2006 | 1 | Antigua and Barbuda | 1991 | 1 | Korea, Rep. | 1991 | 1 | Portugal | 1989 | 1 | Trinidad and Tobago | 2001 | 1 |
| 50 | Argentina | 2008 | 1 | Antigua and Barbuda | 1992 | 1 | Lithuania | 2001 | 1 | Seychelles | 1989 | 1 | Uruguay | 1996 | 1 |
| 51 | Argentina | 2009 | 1 | Czech Republic | 2001 | 1 | Mexico | 2002 | 1 | Seychelles | 1990 | 1 | Uruguay | 2001 | 1 |
| 52 | Antigua and Barbuda | 1990 | 1 | Gabon | 2001 | 1 | Malaysia | 2007 | 1 | Trinidad and Tobago | 2001 | 1 | | | |
| 53 | Antigua and Barbuda | 1991 | 1 | Gabon | 2008 | 1 | Poland | 2008 | 1 | Uruguay | 1996 | 1 | | | |
| 54 | Antigua and Barbuda | 1992 | 1 | Croatia | 2003 | 1 | Portugal | 1990 | 1 | Uruguay | 2001 | 1 | | | |
| 55 | Czech Republic | 2001 | 1 | Hungary | 2002 | 1 | Seychelles | 1989 | 1 | | | | | | |
| 56 | Czech Republic | 2002 | 1 | Korea, Rep. | 1989 | 1 | Seychelles | 1990 | 1 | | | | | | |
| 57 | Gabon | 2001 | 1 | Lithuania | 2004 | 1 | Trinidad and Tobago | 2001 | 1 | | | | | | |
| 58 | Gabon | 2008 | 1 | Mexico | 2001 | 1 | Uruguay | 1996 | 1 | | | | | | |
| 59 | Gabon | 2009 | 1 | Malaysia | 2007 | 1 | Uruguay | 2001 | 1 | | | | | | |
| 60 | Croatia | 2003 | 1 | Malaysia | 2008 | 1 | | | | | | | | | |
| 61 | Hungary | 2002 | 1 | Poland | 2004 | 1 | | | | | | | | | |
| 62 | Kazakhstan | 2010 | 1 | Portugal | 1989 | 1 | | | | | | | | | |
| 63 | St. Kitts and Nevis | 1996 | 1 | Seychelles | 1989 | 1 | | | | | | | | | |
| 64 | Korea, Rep. | 1989 | 1 | Seychelles | 1990 | 1 | | | | | | | | | |
| 65 | Lithuania | 2004 | 1 | Trinidad and Tobago | 2001 | 1 | | | | | | | | | |
| 66 | Mexico | 2001 | 1 | Uruguay | 1996 | 1 | | | | | | | | | |
| 67 | Mauritius | 2009 | 1 | Uruguay | 2001 | 1 | | | | | | | | | |
| 68 | Malaysia | 2007 | 1 | | | | | | | | | | | | |
| 69 | Malaysia | 2008 | 1 | | | | | | | | | | | | |
| 70 | Malaysia | 2009 | 1 | | | | | | | | | | | | |
| 71 | Panama | 2010 | 1 | | | | | | | | | | | | |
| 72 | Poland | 2004 | 1 | | | | | | | | | | | | |
| 73 | Portugal | 1989 | 1 | | | | | | | | | | | | |
| 74 | Seychelles | 1989 | 1 | | | | | | | | | | | | |
| 75 | Seychelles | 1990 | 1 | | | | | | | | | | | | |
| 76 | Trinidad and Tobago | 2001 | 1 | | | | | | | | | | | | |
| 77 | Uruguay | 1996 | 1 | | | | | | | | | | | | |
| 78 | Uruguay | 2001 | 1 | | | | | | | | | | | | |

# E  Full tables for main results

The next four tables show the full regression output for the main results. Note that the ordering of the presentation is slightly different: below we present results for Freedom House first, followed by Polity, then the Aggregate Freedom House-Polity Score, and finally the Unified Democracy Score, where the latter two constitute our preferred specifications since these measures likely contain much less measurement error.

Table 1: Effects on political liberalization (Freedom House)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.06 | 0.02 | −0.01 | −0.00 | 0.20 |
|  | (0.08) | (0.07) | (0.08) | (0.13) | (0.22) |
| IBRD grad. elig. | −0.06 | 0.04 | 0.15 | 0.38$^{\dagger}$ | 0.15 |
|  | (0.10) | (0.14) | (0.14) | (0.19) | (0.20) |
| Log GNI/cap. - c | −0.85 | −0.05 | −0.72 | −1.21 | 0.35 |
|  | (1.11) | (0.78) | (1.04) | (1.78) | (2.81) |
| Interaction term | 3.38* | −0.38 | 0.53 | −1.02 | −1.95 |
|  | (1.67) | (2.14) | (2.86) | (2.52) | (2.99) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.04 | 0.00 | 0.03 | 0.07 | 0.01 |
| adj. $R^2$ | 0.00 | −0.04 | −0.02 | 0.01 | −0.05 |
| Resid. sd | 0.23 | 0.23 | 0.30 | 0.38 | 0.46 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 2: Effects on political liberalization (Polity)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | $0.06^{\dagger}$ | 0.02 | −0.14 | −0.18 | $-0.03^{\dagger}$ |
| | (0.04) | (0.02) | (0.14) | (0.17) | (0.02) |
| IBRD grad. elig. | −0.06 | 0.26 | $0.44^{\dagger}$ | 0.26 | 0.02 |
| | (0.06) | (0.16) | (0.23) | (0.17) | (0.10) |
| Log GNI/cap. - c | $0.80^{\dagger}$ | 0.17 | −1.95 | −2.57 | $-0.96^{*}$ |
| | (0.41) | (0.37) | (1.66) | (1.97) | (0.45) |
| Interaction term | 1.61 | −2.29 | −1.72 | 0.94 | 0.51 |
| | (1.53) | (1.62) | (2.63) | (2.19) | (0.93) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.14 | 0.13 | 0.14 | 0.08 | 0.13 |
| adj. $R^2$ | 0.10 | 0.09 | 0.08 | 0.01 | 0.06 |
| Resid. sd | 0.18 | 0.21 | 0.25 | 0.22 | 0.10 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 3: Effects on political liberalization (Aggregate Polity and Freedom House)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.05 | 0.04 | −0.09 | −0.16 | −0.02 |
| | (0.05) | (0.04) | (0.12) | (0.16) | (0.09) |
| IBRD grad. elig. | −0.10 | $0.19^{*}$ | $0.36^{*}$ | $0.36^{*}$ | 0.18 |
| | (0.06) | (0.08) | (0.14) | (0.17) | (0.12) |
| Log GNI/cap. - c | 0.67 | 0.28 | −1.62 | −2.94 | −1.78 |
| | (0.61) | (0.56) | (1.51) | (1.96) | (1.25) |
| Interaction term | 1.97 | $-2.26^{\dagger}$ | −1.20 | 1.28 | 0.78 |
| | (1.31) | (1.23) | (1.80) | (2.14) | (1.61) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.13 | 0.18 | 0.19 | 0.14 | 0.06 |
| adj. $R^2$ | 0.08 | 0.13 | 0.13 | 0.08 | −0.01 |
| Resid. sd | 0.17 | 0.12 | 0.17 | 0.20 | 0.17 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 4: Effects on political liberalization (Unified Democracy Score)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.05 | 0.07 | −0.05 | 0.01 | 0.14 |
| | (0.08) | (0.07) | (0.07) | (0.10) | (0.18) |
| IBRD grad. elig. | −0.13 | 0.06 | 0.39*** | 0.39* | 0.24 |
| | (0.11) | (0.13) | (0.11) | (0.15) | (0.14) |
| Log GNI/cap. - c | −0.11 | 0.88 | −1.17 | −1.08 | 0.78 |
| | (1.14) | (0.82) | (0.84) | (1.40) | (2.13) |
| Interaction term | 2.85 | −1.56 | −1.37 | −2.58 | −3.89 |
| | (1.87) | (2.47) | (3.16) | (2.62) | (2.61) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.04 | 0.05 | 0.12 | 0.07 | 0.04 |
| adj. $R^2$ | −0.00 | 0.00 | 0.07 | 0.01 | −0.02 |
| Resid. sd | 0.23 | 0.20 | 0.28 | 0.34 | 0.39 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; [*] $p < .05$; [**] $p < .01$; [***] $p < .001$

# F    Effects of Graduation Eligibility on Graduation

Our theoretical discussion proposed that countries crossing the eligibility threshold enact liberalizing reforms in the pursuit of graduation. At the same time, the country trajectories displayed in the main text suggest that graduation is not a forgone conclusion for countries that become graduation eligible. We can use our regression discontinuity design to try to characterize this further. Table 5 shows regression discontinuity estimates for the effect of being graduation-eligible in year $t$ on actual graduation one, two, three, four, and five years after year $t$. Recall that with these estimates, members of the "control" group increasingly enter into the treatment group over successive years, as displayed in Table 8. Thus, what the effects in Table 5 characterize is the extent to which eligibility in year $t$ actually *hastens* graduation relative to those that have not yet crossed the threshold (but will likely do so soon after year $t$). The estimates are very noisy, owing to the fact that actual graduation is infrequent in our sample, which itself is small. They indicate that on average, eligibility tends to accelerate graduation, but that there is substantial variation in outcomes.

Table 5: Effects of graduation eligibility in year $t$ on graduation in future years

|  | +1 year | +2 years | +3 years. | +4 years | +5 years |
|---|---|---|---|---|---|
| RD estimate: | −0.00 | 0.28 | 0.46$^\dagger$ | 0.33 | 0.20 |
|  | (0.22) | (0.28) | (0.28) | (0.30) | (0.32) |
| $N$ | 61 | 55 | 52 | 49 | 46 |
| $R^2$ | 0.06 | 0.11 | 0.12 | 0.07 | 0.06 |

Least squares regression discontinuity estimates.

0.10 bandwidth around cut point and rectangular kernel.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

# G   Alternative Explanations

Our findings show that IBRD graduation eligibility, and therefore the initiation of the graduation process, is associated with pronounced improvements in political liberalization. We have proposed that this is due to state leaders' desires to graduate, which allows them to join an elite group of states. In this section, we consider two alternative explanations for our findings. Graduation eligibility means that governments may soon lose access to borrowing from the Bank. Governments may therefore try to substitute using funds from international financial markets. To do so, they may democratize to attain lower interest rates.[2] A second alternative explanation is that once countries become eligible to graduate, they may increase domestic taxes as an alternative source of funds. To compensate their domestic populations for tax increases, states may provide greater political freedoms in return. This explanation is consistent with the literature linking increased taxation with greater political freedom.[3] Thus, both alternative mechanisms center around the possibility that states seek to compensate for the lost funding associated with graduation by securing money elsewhere.

However, we view these explanations as unlikely to pertain in this case, for several reasons. First, one criterion the Bank uses when determining whether countries may graduate is that they must have access to international capital markets already. Further, this requirement in practice means that recipients have not borrowed from the Bank in several years. Thus, graduation does not actually imply a loss of much, if any, revenue.[4]

Second, countries may use World Bank loans as a form of insurance when market-based lending dries up, but their abilities to do so are largely unhampered by graduation. One official of the World Bank who worked with several countries eligible for graduation and one country that "de-graduated," explained that in the event of a crisis:

> Most of the governments would not shed many tears over no longer being IBRD eligible. Even though the interest rates may be below-market, the transaction costs for these loans are quite high. In addition, investment loans (non-budget support) are actually very costly for borrowers to manage....More than missing IBRD loans as a back-up source of money, what countries getting richer fast really want is the Bank's technical assistance. But that never goes away, since any country can get World Bank technical assistance whenever they want on a fee-for-service basis.

In fact, the official noted, "One argument is that the IBRD needs these countries more than they need the IBRD–especially during global slowdowns–since the interest earnings pays for lots of other World Bank stuff, including the subsidy on IDA credits."[5]

Third, we identify our effect off of the change in graduation *eligibility*. This change involves no immediate loss of revenues, so we would not expect an immediate effect on democracy. It

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[2]Tomz 2007.

[3]Ahmed 2012; Morrison 2009; Moore 2004; North and Weingast 1989; Ross 2004; Smith 2008; Stasavage 2002; Timmons 2005.

[4]Further, note that the Bank can still address credit constraints during financial crises (Winters, 2012). Also note that our findings do not speak to the effects of World Bank conditionality more generally, since states that become eligible for graduation have not borrowed in many years and thus have not faced such conditionality. Though see Winters and Gould (2011) for an interesting analysis.

[5]Interview by authors. July 30, 2012.

is possible that countries are forward-looking, and alter their policies in anticipation of future financial crises, but this is unlikely since countries tend to graduate during boom times, and it is known that countries do not typically anticipate crises.[6] As an official of the World Bank noted, "countries are very myopic."[7] Another official of the Bank noted that countries graduate because "they assume...they have reached a point where they don't need the Bank."[8]

Table 6: Effects on alternative outcomes

| Set | Outcome[◇] | RD Estimate | (S.E.) | N[‡] |
|-----|-----------|------------|--------|------|
| I | Wtd. Total Tariff Rate[a] (Inst., var.) | -0.39 | (0.24) | 48 |
| | Wtd. Total Tariff Rate[a] (1-yr fwd., var.) | -0.11 | (0.14) | 43 |
| | Wtd. Total Tariff Rate[a] (2-yr fwd., var.) | 0.24 | (0.60) | 38 |
| | Total Tax Rate on Profit[a] (Inst., var.) | -0.05 | (0.14) | 27 |
| | Total Tax Rate on Profit[a] (1-yr fwd., var.) | 0.08 | (0.13) | 21 |
| | Total Tax Rate on Profit[a] (2-yr fwd., var.) | -0.07 | (0.19) | 21 |
| II | FDI Pct. GDP[a] (Inst., mean) | -0.95 | (0.58) | 70 |
| | FDI Pct. GDP[a] (1-yr fwd., mean) | -0.14 | (0.19) | 61 |
| | FDI Pct. GDP[a] (2-yr fwd., mean) | 0.03 | (0.34) | 55 |
| | Intl. Capital Pct. GDP[a] (Inst., mean) | -0.58 | (0.41) | 47 |
| | Intl. Capital Pct. GDP[a] (1-yr fwd., mean) | -0.54 | (0.39) | 38 |
| | Intl. Capital Pct. GDP[a] (2-yr fwd., mean) | -0.48* | (0.21) | 32 |
| III | Total Tax Revenue[a] (Inst., mean) | -0.01 | (0.02) | 30 |
| | Total Tax Revenue[a] (1-yr fwd., mean) | -0.01 | (0.03) | 26 |
| | Total Tax Revenue[a] (2-yr fwd., mean) | 0.01 | (0.02) | 25 |

Data sources: [a]World Bank Development Indicators.

[◇] outcomes are standardized relative to the pooled mean and standard deviation of graduation ineligible countries.

[‡] missing data is due to either lead periods being beyond sample range or incidental missing values.

[†] significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

Nonetheless, we subject these two alternative explanations to empirical examination. If the first explanation were true, we would expect to see countries adopt other policies to demonstrate credibility, such as "accepting the golden straightjacket" by diminishing uncertainty through reducing policy variance.[9] Further, if the purpose of introducing these policies were to improve access to private financial markets and FDI, we should see countries borrow more heavily from these markets, and obtain increased FDI flows. If the second explanation were true, we would expect governments to receive higher tax revenues.

Table 6 presents estimates on a set of alternative outcomes to test these claims. As with our analysis of political liberalization effects, all outcomes are standardized relative to the pooled means and standard deviations for countries ineligible to graduate. Sets I and II contain estimates that get at the first alternative explanation — the idea that governments take actions to position themselves more favorably vis-a-vis international capital markets. Set I contains estimates of instantaneous and two-years forward conditional effect of graduation eligibility on the *variance* of

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[6]Reinhart and Rogoff 2009.

[7]Interview by authors. August 6, 2012.

[8]Interview by authors. October 10, 2012.

[9]Friedman 2000; Handley and Limão 2012; Tomz 2007.

countries' tariff rates. To the extent that governments are required to "adopt the golden straight-jacket" we should see convergence in policies such as tariff rates, in which case the sign on these effects should be negative. The evidence does not provide clear indication of such convergence: none of the effects are statistically significant and their signs bounce around. For the case to be strong that these patterns reflect restraint before international markets, we would expect to see countries taking in more FDI or international capital. The estimates in set II do not suggest that such increases in FDI inflows or reliance on international capital markets occurs and even suggest, remarkably, that the opposite may be true. Finally, with respect to the domestic revenue raising argument, the estimates in set III show no evidence of an increase in the tax take.

# H  Disaggregated Results

The estimates presented in the main text show gross effects on political liberalization, and so the question arises, what is the precise nature of these effects? This question is addressed in part by Table 7, which shows effects on components of the Freedom House and Policy scores.

Table 7: Unpacking the effects on political liberalization

|  | Coef. | (S.E.) | N |
|---|---|---|---|
| FH Civ. Lib. (Instant.) | 0.00 | (0.22) | 69 |
| FH Civ. Lib. (1 yr. fwd.) | 0.31 | (0.24) | 60 |
| FH Civ. Lib. (2 yr. fwd.) | 0.42 | (0.27) | 54 |
| FH Pol. Rgts. (Instant.) | 0.08 | (0.13) | 69 |
| FH Pol. Rgts. (1 yr. fwd.) | 0.02 | (0.18) | 60 |
| FH Pol. Rgts. (2 yr. fwd.) | 0.32 | (0.25) | 54 |
| FH Press (Instant.) | 0.02 | (0.11) | 46 |
| FH Press (1 yr. fwd.) | 0.17 | (0.14) | 40 |
| FH Press (2 yr. fwd.) | $0.26^*$ | (0.13) | 37 |
| Polity Compet. (Instant.) | -0.01 | (0.02) | 58 |
| Polity Compet. (1 yr. fwd.) | 0.00 | (0.02) | 49 |
| Polity Compet. (2 yr. fwd.) | 0.00 | (0.03) | 43 |
| Polity Open. Exec. (Instant.) | 0.06 | (0.06) | 58 |
| Polity Open. Exec. (1 yr. fwd.) | 0.07 | (0.07) | 49 |
| Polity Open. Exec. (2 yr. fwd.) | 0.00 | (0.00) | 43 |
| Polity Compet. Exec. (Instant.) | $0.09^*$ | (0.04) | 58 |
| Polity Compet. Exec. (1 yr. fwd.) | $0.13^*$ | (0.05) | 49 |
| Polity Compet. Exec. (2 yr. fwd.) | $0.10^\dagger$ | (0.05) | 43 |
| Polity Exec. Const. (Instant.) | 0.05 | (0.03) | 58 |
| Polity Exec. Const. (1 yr. fwd.) | 0.07 | (0.04) | 49 |
| Polity Exec. Const. (2 yr. fwd.) | 0.01 | (0.01) | 43 |
| Polity Reg. Partic. (Instant.) | -0.06 | (0.04) | 58 |
| Polity Reg. Partic. (1 yr. fwd.) | -0.07 | (0.05) | 49 |
| Polity Reg. Partic. (2 yr. fwd.) | -0.03 | (0.06) | 43 |

# I  Anticipation

The fact that GNI/capita can be forecast implies that governments can form expectations about how likely they are to be eligible for graduation in the coming years. Theoretically, the implications of this are not clear. On the one hand, if the benefits associated with reforms are only obtained under formal eligibility, then it is reasonable to believe that governments refrain from initiating any such reforms until they are actually eligible. They may take preparatory actions prior to eligibility, but such actions could either dampen or amplify the effects of crossing the eligibility threshold per se. For example, if such preparatory actions include liberalizing reforms, this would dampen the effect. However, if these actions include setting up conditions under which liberalization can occur more quickly after crossing the threshold, then this would amplify the effect.

Our account suggests that countries do not engage in anticipatory behavior, however. Countries begin talks with the Bank over the required reforms for graduation only upon crossing the eligibility threshold. Since these countries learn what kinds of changes will be asked of them during these discussions, it makes sense to wait until they begin them to undergo reforms. Why make costly changes before they are needed, when both the precise nature of the changes and the country's eligibility are uncertain? However, it is still possible that countries do so anyway. It is clear that, empirically, there is some uncertainty as to whether a country will in fact cross the threshold in a subsequent year given that it is located 0.1 log(GNI/capita) units below the threshold. Table 3 suggests that from the year a country enters the bandwidth, there is on average a 35% chance of crossing the eligibility threshold in that year, a 67% chance in the next year, and a 77%, 80%, and 84% chance two, three, and four years ahead, respectively.

Table 8: Eligibility status relative to year of treatment-control comparison for countries in bandwidth

|  | -1 yr. | +0 yr. | +1 yr. | +2 yrs. | +3 yrs. | +4 yrs. |
|---|---|---|---|---|---|---|
| Grad. ineligible % | 86 | 65 | 33 | 23 | 20 | 16 |
| Grad. eligible % | 14 | 35 | 67 | 77 | 80 | 84 |
| N[‡] | 80 | 85 | 78 | 69 | 60 | 55 |

[‡] Missing data is due to either lead periods that are beyond the sample range or incidental missing values.

# J  Lingering Effects

Previously, we examined the possibilities of sorting or anticipation effects, finding no statistical evidence for such effects. Here we consider another dynamic aspect of our natural experiment: the possibility that some countries tend to "linger" below the graduation-eligibility threshold, and that such lingering countries may bias our analysis if lingering countries resist liberalization for reasons independent of IBRD graduation. We thus first construct a histogram of the share of years that the countries in our sample spent above the threshold. If our natural experiment were such that a country's position relative to the threshold were really random, then these shares should be uniformly distributed between 0 and 1. Figure 3 provides a visual test. We see that some countries may tend to linger — that is indicated by the spike of countries on the left of the histogram. Thus, we run a robustness check on our main results, where we control for this share-of-years-above-the-threshold variable which serves as a proxy for some latent factor that could bias our analysis. The following tables display the results of these robustness checks, and are essentially unchanged relative to our main results.
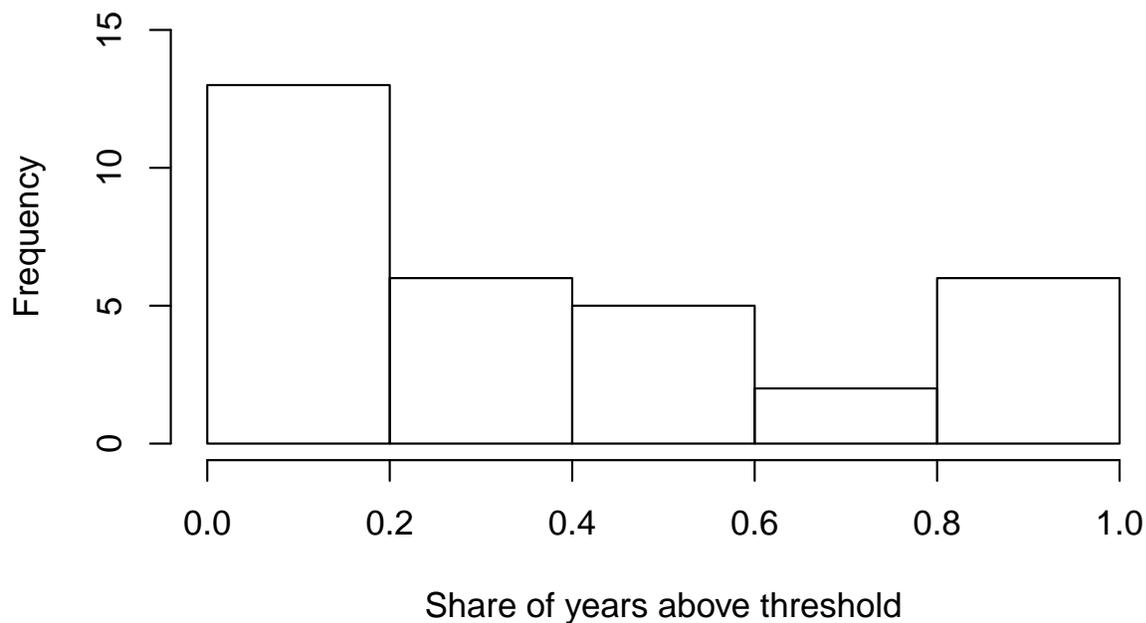


Figure 3: Histogram of shares of years that countries appearing inside the 0.10 bandwidth are situated above the income-per-capita graduation eligibility threshold as opposed to below the threshold.

Table 9: Effects on political liberalization (Freedom House, controlling for "lingering effects")

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.05 | 0.05 | −0.03 | −0.09 | 0.01 |
| | (0.09) | (0.08) | (0.11) | (0.14) | (0.15) |
| IBRD grad. elig. | −0.05 | 0.08 | 0.14 | 0.33$^*$ | 0.06 |
| | (0.10) | (0.14) | (0.13) | (0.16) | (0.20) |
| Log GNI/cap. - c | −0.80 | 0.18 | −0.85 | −1.77 | −0.93 |
| | (1.14) | (0.87) | (1.18) | (1.66) | (2.03) |
| Share yrs. above cut | −0.02 | −0.08 | 0.04 | 0.17 | 0.37 |
| | (0.11) | (0.09) | (0.17) | (0.25) | (0.38) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 3.32$^\dagger$ | −0.77 | 0.77 | −0.13 | 0.01 |
| | (1.72) | (2.27) | (3.46) | (2.30) | (2.34) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.04 | 0.01 | 0.03 | 0.09 | 0.08 |
| adj. $R^2$ | −0.01 | −0.05 | −0.04 | 0.01 | −0.00 |
| Resid. sd | 0.23 | 0.23 | 0.30 | 0.38 | 0.45 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 10: Effects on political liberalization (Polity, controlling for "lingering effects")

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.02 | −0.00 | −0.21 | −0.23 | −0.04 |
| | (0.05) | (0.03) | (0.18) | (0.21) | (0.05) |
| IBRD grad. elig. | −0.17$^\dagger$ | 0.24 | 0.40$^\dagger$ | 0.23 | 0.02 |
| | (0.09) | (0.15) | (0.20) | (0.14) | (0.10) |
| Log GNI/cap. - c | 0.26 | 0.05 | −2.41 | −2.90 | −1.02$^*$ |
| | (0.48) | (0.39) | (1.90) | (2.22) | (0.41) |
| Share yrs. above cut | 0.29$^{**}$ | 0.06 | 0.15 | 0.11 | 0.02 |
| | (0.09) | (0.08) | (0.14) | (0.14) | (0.08) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 2.40 | −2.06 | −0.79 | 1.48 | 0.61 |
| | (1.67) | (1.64) | (2.93) | (2.66) | (1.01) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.26 | 0.14 | 0.16 | 0.10 | 0.13 |
| adj. $R^2$ | 0.21 | 0.07 | 0.09 | 0.01 | 0.03 |
| Resid. sd | 0.17 | 0.21 | 0.25 | 0.22 | 0.11 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 11: Effects on political liberalization (Aggregate Freedom House and Polity, controlling for "lingering effects")

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.01 | 0.04 | −0.13 | −0.21 | −0.06 |
| | (0.06) | (0.05) | (0.15) | (0.20) | (0.11) |
| IBRD grad. elig. | −0.17* | 0.20* | 0.33* | 0.33* | 0.16 |
| | (0.08) | (0.09) | (0.13) | (0.15) | (0.11) |
| Log GNI/cap. - c | 0.29 | 0.31 | −1.91 | −3.29 | −2.04 |
| | (0.60) | (0.58) | (1.66) | (2.13) | (1.22) |
| Share yrs. above cut | 0.20* | −0.01 | 0.10 | 0.12 | 0.08 |
| | (0.09) | (0.04) | (0.10) | (0.14) | (0.11) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 2.52$^\dagger$ | −2.32$^\dagger$ | −0.61 | 1.86 | 1.18 |
| | (1.50) | (1.30) | (2.14) | (2.37) | (1.47) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.19 | 0.18 | 0.21 | 0.17 | 0.08 |
| adj. $R^2$ | 0.14 | 0.12 | 0.13 | 0.08 | −0.02 |
| Resid. sd | 0.16 | 0.13 | 0.17 | 0.20 | 0.17 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 12: Effects on political liberalization (Unified Democracy Score, controlling for "lingering effects")

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.08 | 0.07 | −0.13 | −0.14 | −0.12 |
| | (0.10) | (0.09) | (0.10) | (0.12) | (0.12) |
| IBRD grad. elig. | −0.10 | 0.06 | 0.33*** | 0.30* | 0.11 |
| | (0.11) | (0.13) | (0.09) | (0.13) | (0.12) |
| Log GNI/cap. - c | 0.04 | 0.86 | −1.73$^\dagger$ | −2.07 | −0.95 |
| | (1.22) | (0.90) | (0.99) | (1.34) | (1.01) |
| Share yrs. above cut | −0.08 | 0.00 | 0.17 | 0.31$^\dagger$ | 0.50$^\dagger$ |
| | (0.13) | (0.08) | (0.11) | (0.16) | (0.29) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 2.60 | −1.54 | −0.37 | −1.01 | −1.24 |
| | (2.04) | (2.53) | (3.35) | (2.16) | (1.52) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.04 | 0.05 | 0.15 | 0.14 | 0.21 |
| adj. $R^2$ | −0.01 | −0.01 | 0.09 | 0.07 | 0.14 |
| Resid. sd | 0.24 | 0.20 | 0.28 | 0.33 | 0.35 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

# K    Effect Heterogeneity for EU Candidates and U.S. Allies

We have argued that the effect of IBRD graduation eligibility is driven by the appeal of the invitation to "join the club" of developed countries. At the same time, many of the countries going through this process are also EU Candidate countries as well as U.S. allies. As such, it is possible that these effects are conditional on the presence of external liberalizing pressure from the EU or the U.S. To assess this, we examine interaction effects between graduation eligibility and either EU candidate or U.S. ally status. As per the European Commission (EC) website,[10] the following countries either acceded after 1987, were applicant countries, or were "potential candidate" countries as designated by the EC: Albania, Bosnia and Herzegovina, Bulgaria, Czech Republic, Croatia, Estonia, Cyprus, Hungary, Kosovo, Latvia, Lithuania, Macedonia (FYR), Malta, Montenegro, Poland, Romania, Serbia, Slovak Republic, Slovenia, and Turkey. We thus code these as EU candidate countries and examine the interaction effect of this variable with graduation eligibility. The results are shown below in section K.1. The section also contains tables that list the samples for the 2-year-forward estimates, which tend to have the most pronounced effects. We find that the interaction effect is negative (and significant for the Polity and Aggregate Freedom House-Polity scores), indicating that our main results are driven by *non-candidate* countries.

We perform a similar exercise for U.S. allies. We use the Correlates of War Alliance 4.0 data, which lists the following countries as having an alliance with the U.S. in the period following 1987: Antigua and Barbuda, Argentina, Australia, The Bahamas, Barbados, Belgium, Belize, Bolivia, Brazil, Canada, Chile, Colombia, Costa Rica, Czech Republic, Denmark, Dominica, Dominican Republic, Ecuador, El Salvador, France, Germany, Greece, Grenada, Guatemala, Guyana, Haiti, Honduras, Hungary, Iceland, Italy, Jamaica, Japan, Liberia, Luxembourg, Mexico, Netherlands, Nicaragua, Norway, Pakistan, Panama, Paraguay, Peru, Philippines, Poland, Portugal, South Korea, Spain, St. Kitts and Nevis, St. Lucia, St. Vincent and the Grenadines, Suriname, Trinidad and Tobago, Turkey, United Kingdom, Uruguay, and Venezuela. We code these as U.S. allies and examine the interaction effect of this variable with graduation eligibility. The results are shown in section K.2, and as with the EU candidate analysis, the section also contains tables that list the samples for the 2-year-forward estimates. Again, we find that the interaction effect is negative (and significant for Freedom House, Polity, and Unified Democracy scores), which indicates that our main results are driven by *non-ally* countries.

One explanation for these patterns may be ceiling effects. That is, it could be that EU candidate countries and U.S. allies already exhibit high liberalization scores and thus have less room to reform. We test this possibility in section K.3 below. The evidence suggests that this is indeed the case: both EU candidate countries and U.S. allies exhibit substantially and significantly higher levels on all of the liberalization scores. As such, there may be less scope for graduation-eligibility to have an effect. However, we note that for the case of U.S. allies, we cannot adjudicate between this potential mechanism and the idea that these countries may be insulated from U.S. pressure to reform due to their strategic importance to the U.S.

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[10]`http://ec.europa.eu/economy_finance/international/enlargement/index_en.htm`,    (accessed 10/25/16).

## K.1 European Union Candidate Interaction

Table 13: Effects on political liberalization (Freedom House, EU candidate interaction)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.09 | −0.03 | −0.05 | −0.05 | 0.20 |
|  | (0.08) | (0.07) | (0.08) | (0.15) | (0.27) |
| IBRD grad. elig. | −0.05 | −0.05 | 0.08 | 0.51 | 0.25 |
|  | (0.10) | (0.12) | (0.16) | (0.41) | (0.36) |
| EU candidate | 0.10[†] | 0.13[†] | 0.10 | 0.13 | −0.00 |
|  | (0.05) | (0.06) | (0.06) | (0.09) | (0.14) |
| Log GNI/cap. - c | −0.99 | −0.21 | −0.80 | −1.31 | 0.35 |
|  | (1.09) | (0.72) | (1.09) | (1.92) | (2.93) |
| IBRD grad. elig. X EU candidate | −0.06 | 0.10 | 0.08 | −0.23 | −0.12 |
|  | (0.09) | (0.13) | (0.19) | (0.35) | (0.33) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 3.72$^*$ | 1.28 | 1.63 | −1.83 | −2.94 |
|  | (1.65) | (1.76) | (2.30) | (4.39) | (4.99) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.07 | 0.11 | 0.07 | 0.09 | 0.02 |
| adj. $R^2$ | 0.00 | 0.04 | −0.02 | −0.01 | −0.09 |
| Resid. sd | 0.23 | 0.22 | 0.30 | 0.38 | 0.47 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 14: Effects on political liberalization (Polity, EU candidate interaction)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.06 | 0.02 | −0.19 | −0.24 | −0.04 |
| | (0.04) | (0.01) | (0.17) | (0.21) | (0.04) |
| IBRD grad. elig. | 0.04 | 0.52$^*$ | 0.70$^*$ | 0.44$^\dagger$ | 0.18 |
| | (0.07) | (0.26) | (0.31) | (0.24) | (0.11) |
| EU candidate | 0.02 | 0.01 | 0.09 | 0.10 | 0.01 |
| | (0.03) | (0.02) | (0.08) | (0.10) | (0.05) |
| Log GNI/cap. - c | 0.77$^\dagger$ | 0.16 | −2.19 | −2.89 | −1.01$^*$ |
| | (0.42) | (0.32) | (1.83) | (2.16) | (0.41) |
| IBRD grad. elig. X EU candidate | −0.16$^{**}$ | −0.34 | −0.38$^\dagger$ | −0.22$^\dagger$ | −0.16 |
| | (0.05) | (0.21) | (0.19) | (0.13) | (0.11) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 0.76 | −4.79$^*$ | −3.29 | −0.04 | −0.93 |
| | (1.51) | (2.00) | (3.32) | (2.50) | (1.07) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.17 | 0.27 | 0.23 | 0.13 | 0.22 |
| adj. $R^2$ | 0.11 | 0.19 | 0.14 | 0.01 | 0.11 |
| Resid. sd | 0.18 | 0.20 | 0.24 | 0.22 | 0.10 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 15: Effects on political liberalization (Aggregate Freedom House and Polity, EU candidate interaction)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.02 | 0.01 | −0.14 | −0.24 | −0.05 |
| | (0.06) | (0.05) | (0.15) | (0.20) | (0.12) |
| IBRD grad. elig. | −0.03 | 0.29$^*$ | 0.46$^*$ | 0.56$^*$ | 0.33$^\dagger$ |
| | (0.07) | (0.12) | (0.18) | (0.21) | (0.16) |
| EU candidate | 0.06 | 0.06 | 0.10 | 0.13 | 0.05 |
| | (0.04) | (0.04) | (0.07) | (0.10) | (0.08) |
| Log GNI/cap. - c | 0.55 | 0.10 | −1.90 | −3.39 | −1.96 |
| | (0.63) | (0.56) | (1.64) | (2.09) | (1.33) |
| IBRD grad. elig. X EU candidate | −0.13$^*$ | −0.15 | −0.16 | −0.24$^*$ | −0.17 |
| | (0.06) | (0.10) | (0.10) | (0.10) | (0.10) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 1.64 | −2.76$^\dagger$ | −1.32 | 0.60 | −0.27 |
| | (1.20) | (1.43) | (2.10) | (2.16) | (1.59) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.15 | 0.23 | 0.24 | 0.22 | 0.10 |
| adj. $R^2$ | 0.08 | 0.16 | 0.15 | 0.12 | −0.03 |
| Resid. sd | 0.17 | 0.12 | 0.17 | 0.20 | 0.17 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^* p < .05$; $^{**} p < .01$; $^{***} p < .001$

Table 16: Effects on political liberalization (Unified Democracy Score, EU candidate interaction)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.01 | 0.05 | −0.07 | −0.04 | 0.14 |
| | (0.08) | (0.07) | (0.08) | (0.11) | (0.23) |
| IBRD grad. elig. | −0.06 | 0.05 | 0.35$^*$ | 0.69$^*$ | 0.52$^*$ |
| | (0.12) | (0.16) | (0.15) | (0.29) | (0.25) |
| EU candidate | 0.11 | 0.06 | 0.04 | 0.14 | 0.01 |
| | (0.07) | (0.07) | (0.10) | (0.14) | (0.14) |
| Log GNI/cap. - c | −0.27 | 0.79 | −1.20 | −1.19 | 0.77 |
| | (1.08) | (0.75) | (0.83) | (1.35) | (2.25) |
| IBRD grad. elig. X EU candidate | −0.16 | −0.01 | 0.04 | −0.45 | −0.36 |
| | (0.11) | (0.14) | (0.18) | (0.31) | (0.22) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 2.73 | −1.11 | −0.87 | −5.10 | −6.89$^\dagger$ |
| | (1.89) | (2.77) | (3.09) | (3.44) | (3.82) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.07 | 0.07 | 0.13 | 0.14 | 0.09 |
| adj. $R^2$ | 0.01 | −0.01 | 0.05 | 0.05 | −0.01 |
| Resid. sd | 0.23 | 0.20 | 0.28 | 0.33 | 0.38 |

Ordinary least squares estimates within 0.15 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^* p < .05$; $^{**} p < .01$; $^{***} p < .001$

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| Category | Country | Year |
|---|---|---|
| Treated EU Candidate | Czech Republic | 2001 |
| | Czech Republic | 2002 |
| | Croatia | 2003 |
| | Hungary | 2002 |
| | Lithuania | 2004 |
| | Poland | 2004 |
| Control EU Candidate | Czech Republic | 1997 |
| | Czech Republic | 1998 |
| | Czech Republic | 1999 |
| | Czech Republic | 2000 |
| | Estonia | 2003 |
| | Croatia | 2002 |
| | Hungary | 2000 |
| | Hungary | 2001 |
| | Latvia | 2004 |
| | Montenegro | 2008 |
| | Poland | 2003 |
| | Romania | 2007 |
| | Slovak Republic | 2003 |
| Treated Non-candidate | Argentina | 2008 |
| | Gabon | 2007 |
| | Gabon | 2008 |
| | Korea, Rep. | 1989 |
| | Mexico | 2001 |
| | Malaysia | 2007 |
| | Malaysia | 2008 |
| | Portugal | 1989 |
| | Trinidad and Tobago | 2001 |
| | Uruguay | 1996 |
| | Uruguay | 2001 |
| Control Non-candidate | Argentina | 2007 |
| | Brazil | 2007 |
| | Botswana | 2006 |
| | Botswana | 2008 |
| | Chile | 1996 |
| | Chile | 1997 |
| | Chile | 1998 |
| | Chile | 1999 |
| | Chile | 2005 |
| | Gabon | 1992 |
| | Kazakhstan | 2008 |
| | Korea, Rep. | 1988 |
| | Lebanon | 2005 |
| | Lebanon | 2008 |
| | Libya | 2002 |
| | Libya | 2005 |
| | Mexico | 2000 |
| | Mauritius | 2008 |
| | Panama | 2008 |
| | Portugal | 1988 |
| | Russian Federation | 2006 |
| | Trinidad and Tobago | 2000 |
| | Uruguay | 1994 |
| | Uruguay | 1995 |
| | Uruguay | 2007 |
| | Venezuela, RB | 1987 |
| | Venezuela, RB | 2001 |
| | Venezuela, RB | 2006 |

Table 17: EU Candidate Cases, 2 yr. (Polity-FH Aggregate)

| Category | Country | Year |
|---|---|---|
| Treated EU Candidate | Czech Republic | 2001 |
| | Czech Republic | 2002 |
| | Croatia | 2003 |
| | Hungary | 2002 |
| | Lithuania | 2004 |
| | Poland | 2004 |
| Control EU Candidate | Czech Republic | 1997 |
| | Czech Republic | 1998 |
| | Czech Republic | 1999 |
| | Czech Republic | 2000 |
| | Estonia | 2003 |
| | Croatia | 2002 |
| | Hungary | 2000 |
| | Hungary | 2001 |
| | Latvia | 2004 |
| | Montenegro | 2008 |
| | Poland | 2003 |
| | Romania | 2007 |
| | Slovak Republic | 2003 |
| Treated Non-candidate | Argentina | 2008 |
| | Antigua and Barbuda | 1990 |
| | Antigua and Barbuda | 1991 |
| | Antigua and Barbuda | 1992 |
| | Gabon | 2007 |
| | Gabon | 2008 |
| | St. Kitts and Nevis | 1996 |
| | Korea, Rep. | 1989 |
| | Mexico | 2001 |
| | Malaysia | 2007 |
| | Malaysia | 2008 |
| | Portugal | 1989 |
| | Seychelles | 1989 |
| | Seychelles | 1990 |
| | Trinidad and Tobago | 2001 |
| | Uruguay | 1996 |
| | Uruguay | 2001 |
| Control Non-candidate | Argentina | 2007 |
| | Brazil | 2007 |
| | Botswana | 2006 |
| | Botswana | 2008 |
| | Chile | 1996 |
| | Chile | 1997 |
| | Chile | 1998 |
| | Chile | 1999 |
| | Chile | 2005 |
| | Gabon | 1992 |
| | Kazakhstan | 2008 |
| | St. Kitts and Nevis | 1992 |
| | St. Kitts and Nevis | 1993 |
| | St. Kitts and Nevis | 1994 |
| | Korea, Rep. | 1988 |
| | Lebanon | 2005 |
| | Lebanon | 2008 |
| | Libya | 2002 |
| | Libya | 2005 |
| | Mexico | 2000 |
| | Mauritius | 2008 |
| | Panama | 2008 |
| | Portugal | 1988 |
| | Russian Federation | 2006 |
| | Seychelles | 1987 |
| | Seychelles | 1988 |
| | Trinidad and Tobago | 2000 |
| | Uruguay | 1994 |
| | Uruguay | 1995 |
| | Uruguay | 2007 |
| | Venezuela, RB | 1987 |
| | Venezuela, RB | 2001 |
| | Venezuela, RB | 2006 |

Table 18: EU Candidate Cases, 2 yr. (FH Score)

| Category | Country | Year |
|---|---|---|
| Treated EU Candidate | Czech Republic | 2001 |
| | Czech Republic | 2002 |
| | Croatia | 2003 |
| | Hungary | 2002 |
| | Lithuania | 2004 |
| | Poland | 2004 |
| Control EU Candidate | Czech Republic | 1997 |
| | Czech Republic | 1998 |
| | Czech Republic | 1999 |
| | Czech Republic | 2000 |
| | Estonia | 2003 |
| | Croatia | 2002 |
| | Hungary | 2000 |
| | Hungary | 2001 |
| | Latvia | 2004 |
| | Montenegro | 2008 |
| | Poland | 2003 |
| | Romania | 2007 |
| | Slovak Republic | 2003 |
| Treated Non-candidate | Argentina | 2008 |
| | Gabon | 2007 |
| | Gabon | 2008 |
| | Korea, Rep. | 1989 |
| | Mexico | 2001 |
| | Malaysia | 2007 |
| | Malaysia | 2008 |
| | Portugal | 1989 |
| | Trinidad and Tobago | 2001 |
| | Uruguay | 1996 |
| | Uruguay | 2001 |
| Control Non-candidate | Argentina | 2007 |
| | Brazil | 2007 |
| | Botswana | 2006 |
| | Botswana | 2008 |
| | Chile | 1996 |
| | Chile | 1997 |
| | Chile | 1998 |
| | Chile | 1999 |
| | Chile | 2005 |
| | Gabon | 1992 |
| | Kazakhstan | 2008 |
| | Korea, Rep. | 1988 |
| | Lebanon | 2005 |
| | Lebanon | 2008 |
| | Libya | 2002 |
| | Libya | 2005 |
| | Mexico | 2000 |
| | Mauritius | 2008 |
| | Panama | 2008 |
| | Portugal | 1988 |
| | Russian Federation | 2006 |
| | Trinidad and Tobago | 2000 |
| | Uruguay | 1994 |
| | Uruguay | 1995 |
| | Uruguay | 2007 |
| | Venezuela, RB | 1987 |
| | Venezuela, RB | 2001 |
| | Venezuela, RB | 2006 |

Table 19: EU Candidate Cases, 2 yr. (Polity Score)

| Category | Country | Year |
|---|---|---|
| Treated EU Candidate | Czech Republic | 2001 |
| | Czech Republic | 2002 |
| | Croatia | 2003 |
| | Hungary | 2002 |
| | Lithuania | 2004 |
| | Poland | 2004 |
| Control EU Candidate | Czech Republic | 1997 |
| | Czech Republic | 1998 |
| | Czech Republic | 1999 |
| | Czech Republic | 2000 |
| | Estonia | 2003 |
| | Croatia | 2002 |
| | Hungary | 2000 |
| | Hungary | 2001 |
| | Latvia | 2004 |
| | Montenegro | 2008 |
| | Poland | 2003 |
| | Romania | 2007 |
| | Slovak Republic | 2003 |
| Treated Non-candidate | Argentina | 2008 |
| | Antigua and Barbuda | 1990 |
| | Antigua and Barbuda | 1991 |
| | Antigua and Barbuda | 1992 |
| | Gabon | 2007 |
| | Gabon | 2008 |
| | St. Kitts and Nevis | 1996 |
| | Korea, Rep. | 1989 |
| | Mexico | 2001 |
| | Malaysia | 2007 |
| | Malaysia | 2008 |
| | Portugal | 1989 |
| | Seychelles | 1989 |
| | Seychelles | 1990 |
| | Trinidad and Tobago | 2001 |
| | Uruguay | 1996 |
| | Uruguay | 2001 |
| Control Non-candidate | Argentina | 2007 |
| | Brazil | 2007 |
| | Botswana | 2006 |
| | Botswana | 2008 |
| | Chile | 1996 |
| | Chile | 1997 |
| | Chile | 1998 |
| | Chile | 1999 |
| | Chile | 2005 |
| | Gabon | 1992 |
| | Kazakhstan | 2008 |
| | St. Kitts and Nevis | 1992 |
| | St. Kitts and Nevis | 1993 |
| | St. Kitts and Nevis | 1994 |
| | Korea, Rep. | 1988 |
| | Lebanon | 2005 |
| | Lebanon | 2008 |
| | Libya | 2002 |
| | Libya | 2005 |
| | Mexico | 2000 |
| | Mauritius | 2008 |
| | Panama | 2008 |
| | Portugal | 1988 |
| | Russian Federation | 2006 |
| | Seychelles | 1987 |
| | Seychelles | 1988 |
| | Trinidad and Tobago | 2000 |
| | Uruguay | 1994 |
| | Uruguay | 1995 |
| | Uruguay | 2007 |
| | Venezuela, RB | 1987 |
| | Venezuela, RB | 2001 |
| | Venezuela, RB | 2006 |

Table 20: EU Candidate Cases, 2 yr. (UDS Score)

## K.2 U.S. Ally Interaction

Table 21: Effects on political liberalization (Freedom House, US ally interaction)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.01 | 0.05 | −0.06 | −0.01 | 0.30 |
|  | (0.08) | (0.08) | (0.08) | (0.17) | (0.36) |
| IBRD grad. elig. | −0.14 | 0.02 | $0.31^{\dagger}$ | $0.73^{*}$ | 0.44 |
|  | (0.11) | (0.15) | (0.18) | (0.31) | (0.35) |
| US Ally | −0.08 | −0.05 | 0.05 | 0.01 | −0.11 |
|  | (0.06) | (0.06) | (0.06) | (0.11) | (0.22) |
| Log GNI/cap. - c | −0.78 | 0.04 | −0.85 | −1.24 | 0.60 |
|  | (1.04) | (0.79) | (1.00) | (1.94) | (3.24) |
| IBRD grad. elig. X US Ally | 0.16 | −0.00 | −0.57 | $-0.92^{**}$ | $-0.91^{**}$ |
|  | (0.10) | (0.17) | (0.38) | (0.33) | (0.30) |
| IBRD grad. elig. X (Log GNI/cap. - c) | $2.90^{*}$ | −0.11 | 5.52 | $5.72^{\dagger}$ | 5.35 |
|  | (1.43) | (2.58) | (5.08) | (2.95) | (3.64) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.07 | 0.01 | 0.18 | 0.33 | 0.27 |
| adj. $R^2$ | 0.01 | −0.07 | 0.10 | 0.26 | 0.19 |
| Resid. sd | 0.23 | 0.23 | 0.28 | 0.32 | 0.41 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 22: Effects on political liberalization (Polity, US ally interaction)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | $0.08^{*}$ | $-0.00$ | $-0.14$ | $-0.19$ | $-0.03$ |
| | (0.04) | (0.03) | (0.11) | (0.14) | (0.05) |
| IBRD grad. elig. | $-0.02$ | $0.40^{\dagger}$ | $0.53^{*}$ | $0.29^{\dagger}$ | $0.09$ |
| | (0.07) | (0.20) | (0.22) | (0.15) | (0.07) |
| US Ally | $-0.02$ | $0.03$ | $-0.00$ | $0.01$ | $-0.00$ |
| | (0.03) | (0.03) | (0.06) | (0.07) | (0.05) |
| Log GNI/cap. - c | $0.79^{\dagger}$ | $0.11$ | $-1.94$ | $-2.63$ | $-0.94$ |
| | (0.44) | (0.36) | (1.54) | (1.85) | (0.58) |
| IBRD grad. elig. X US Ally | $-0.15$ | $-0.56^{\dagger}$ | $-0.55^{\dagger}$ | $-0.06$ | $-0.22^{\dagger}$ |
| | (0.09) | (0.31) | (0.29) | (0.09) | (0.12) |
| IBRD grad. elig. X (Log GNI/cap. - c) | $2.53$ | $2.03$ | $4.48$ | $1.40$ | $2.45^{\dagger}$ |
| | (1.55) | (3.45) | (3.04) | (2.17) | (1.22) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.20 | 0.45 | 0.26 | 0.08 | 0.23 |
| adj. $R^2$ | 0.14 | 0.40 | 0.18 | $-0.04$ | 0.11 |
| Resid. sd | 0.18 | 0.17 | 0.24 | 0.22 | 0.10 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 23: Effects on political liberalization (Aggregate Freedom House and Polity, US ally interaction)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.06 | 0.04 | $-0.13$ | $-0.22$ | $-0.10$ |
|  | (0.06) | (0.06) | (0.11) | (0.14) | (0.10) |
| IBRD grad. elig. | $-0.10$ | $0.23^*$ | $0.42^{**}$ | $0.43^*$ | $0.28^*$ |
|  | (0.08) | (0.10) | (0.14) | (0.16) | (0.13) |
| US Ally | $-0.03$ | $-0.00$ | 0.04 | 0.07 | 0.08 |
|  | (0.04) | (0.04) | (0.05) | (0.07) | (0.08) |
| Log GNI/cap. - c | 0.65 | 0.29 | $-1.77$ | $-3.24^\dagger$ | $-2.07^\dagger$ |
|  | (0.62) | (0.58) | (1.44) | (1.81) | (1.18) |
| IBRD grad. elig. X US Ally | 0.00 | $-0.20$ | $-0.19$ | $-0.07$ | $-0.18$ |
|  | (0.08) | (0.14) | (0.12) | (0.11) | (0.13) |
| IBRD grad. elig. X (Log GNI/cap. - c) | $2.13^\dagger$ | $-0.69$ | 0.62 | 1.62 | 1.97 |
|  | (1.16) | (1.68) | (1.80) | (1.88) | (1.56) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.13 | 0.30 | 0.21 | 0.16 | 0.10 |
| adj. $R^2$ | 0.06 | 0.23 | 0.12 | 0.04 | $-0.03$ |
| Resid. sd | 0.17 | 0.12 | 0.17 | 0.21 | 0.17 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^* p < .05$; $^{**} p < .01$; $^{***} p < .001$

Table 24: Effects on political liberalization (Unified Democracy Score, US ally interaction)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | $0.15^{\dagger}$ | 0.09 | −0.07 | 0.04 | 0.30 |
| | (0.08) | (0.07) | (0.08) | (0.11) | (0.27) |
| IBRD grad. elig. | $-0.22^{\dagger}$ | 0.10 | $0.52^{**}$ | $0.67^{*}$ | 0.44 |
| | (0.12) | (0.12) | (0.16) | (0.31) | (0.26) |
| US Ally | $-0.15^{**}$ | −0.02 | 0.02 | −0.04 | −0.19 |
| | (0.05) | (0.07) | (0.08) | (0.11) | (0.18) |
| Log GNI/cap. - c | 0.02 | 0.92 | −1.22 | −0.96 | 1.19 |
| | (1.04) | (0.85) | (0.89) | (1.53) | (2.37) |
| IBRD grad. elig. X US Ally | 0.12 | $-0.17^{\dagger}$ | $-0.56^{\dagger}$ | $-0.80^{*}$ | $-0.76^{**}$ |
| | (0.10) | (0.10) | (0.30) | (0.34) | (0.22) |
| IBRD grad. elig. X (Log GNI/cap. - c) | 2.85 | −0.23 | 3.75 | 3.51 | 2.70 |
| | (1.76) | (2.59) | (4.41) | (3.06) | (2.26) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.10 | 0.11 | 0.29 | 0.35 | 0.37 |
| adj. $R^2$ | 0.04 | 0.04 | 0.22 | 0.28 | 0.30 |
| Resid. sd | 0.23 | 0.20 | 0.25 | 0.29 | 0.32 |

Ordinary least squares estimates within 0.15 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

| Category | Country | Year |
|---|---|---|
| Treated US Allies | Argentina | 2008 |
| | Czech Republic | 2001 |
| | Czech Republic | 2002 |
| | Hungary | 2002 |
| | Korea, Rep. | 1989 |
| | Mexico | 2001 |
| | Poland | 2004 |
| | Portugal | 1989 |
| | Trinidad and Tobago | 2001 |
| | Uruguay | 1996 |
| | Uruguay | 2001 |
| Control US Allies | Argentina | 2007 |
| | Brazil | 2007 |
| | Chile | 1996 |
| | Chile | 1997 |
| | Chile | 1998 |
| | Chile | 1999 |
| | Chile | 2005 |
| | Czech Republic | 1997 |
| | Czech Republic | 1998 |
| | Czech Republic | 1999 |
| | Czech Republic | 2000 |
| | Hungary | 2000 |
| | Hungary | 2001 |
| | Korea, Rep. | 1988 |
| | Mexico | 2000 |
| | Panama | 2008 |
| | Poland | 2003 |
| | Portugal | 1988 |
| | Trinidad and Tobago | 2000 |
| | Uruguay | 1994 |
| | Uruguay | 1995 |
| | Uruguay | 2007 |
| | Venezuela, RB | 1987 |
| | Venezuela, RB | 2001 |
| | Venezuela, RB | 2006 |
| Treated Non-allies | Gabon | 2007 |
| | Gabon | 2008 |
| | Croatia | 2003 |
| | Lithuania | 2004 |
| | Malaysia | 2007 |
| | Malaysia | 2008 |
| Control Non-allies | Botswana | 2006 |
| | Botswana | 2008 |
| | Estonia | 2003 |
| | Gabon | 1992 |
| | Croatia | 2002 |
| | Kazakhstan | 2008 |
| | Lebanon | 2005 |
| | Lebanon | 2008 |
| | Libya | 2002 |
| | Libya | 2005 |
| | Latvia | 2004 |
| | Montenegro | 2008 |
| | Mauritius | 2008 |
| | Romania | 2007 |
| | Russian Federation | 2006 |
| | Slovak Republic | 2003 |

Table 25: US Ally Cases, 2 yr. (Polity-FH Aggregate)

| Category | Country | Year |
|---|---|---|
| Treated US Allies | Argentina | 2008 |
| | Antigua and Barbuda | 1990 |
| | Antigua and Barbuda | 1991 |
| | Antigua and Barbuda | 1992 |
| | Czech Republic | 2001 |
| | Czech Republic | 2002 |
| | Hungary | 2002 |
| | St. Kitts and Nevis | 1996 |
| | Korea, Rep. | 1989 |
| | Mexico | 2001 |
| | Poland | 2004 |
| | Portugal | 1989 |
| | Trinidad and Tobago | 2001 |
| | Uruguay | 1996 |
| | Uruguay | 2001 |
| Control US Allies | Argentina | 2007 |
| | Brazil | 2007 |
| | Chile | 1996 |
| | Chile | 1997 |
| | Chile | 1998 |
| | Chile | 1999 |
| | Chile | 2005 |
| | Czech Republic | 1997 |
| | Czech Republic | 1998 |
| | Czech Republic | 1999 |
| | Czech Republic | 2000 |
| | Hungary | 2000 |
| | Hungary | 2001 |
| | St. Kitts and Nevis | 1992 |
| | St. Kitts and Nevis | 1993 |
| | St. Kitts and Nevis | 1994 |
| | Korea, Rep. | 1988 |
| | Mexico | 2000 |
| | Panama | 2008 |
| | Poland | 2003 |
| | Portugal | 1988 |
| | Trinidad and Tobago | 2000 |
| | Uruguay | 1994 |
| | Uruguay | 1995 |
| | Uruguay | 2007 |
| | Venezuela, RB | 1987 |
| | Venezuela, RB | 2001 |
| | Venezuela, RB | 2006 |
| Treated Non-allies | Gabon | 2007 |
| | Gabon | 2008 |
| | Croatia | 2003 |
| | Lithuania | 2004 |
| | Malaysia | 2007 |
| | Malaysia | 2008 |
| | Seychelles | 1989 |
| | Seychelles | 1990 |
| Control Non-allies | Botswana | 2006 |
| | Botswana | 2008 |
| | Estonia | 2003 |
| | Gabon | 1992 |
| | Croatia | 2002 |
| | Kazakhstan | 2008 |
| | Lebanon | 2005 |
| | Lebanon | 2008 |
| | Libya | 2002 |
| | Libya | 2005 |
| | Latvia | 2004 |
| | Montenegro | 2008 |
| | Mauritius | 2008 |
| | Romania | 2007 |
| | Russian Federation | 2006 |
| | Slovak Republic | 2003 |
| | Seychelles | 1987 |
| | Seychelles | 1988 |

Table 26: US Ally Cases, 2 yr. (FH Score)

| Category | Country | Year |
|---|---|---|
| Treated US Allies | Argentina | 2008 |
| | Czech Republic | 2001 |
| | Czech Republic | 2002 |
| | Hungary | 2002 |
| | Korea, Rep. | 1989 |
| | Mexico | 2001 |
| | Poland | 2004 |
| | Portugal | 1989 |
| | Trinidad and Tobago | 2001 |
| | Uruguay | 1996 |
| | Uruguay | 2001 |
| Control US Allies | Argentina | 2007 |
| | Brazil | 2007 |
| | Chile | 1996 |
| | Chile | 1997 |
| | Chile | 1998 |
| | Chile | 1999 |
| | Chile | 2005 |
| | Czech Republic | 1997 |
| | Czech Republic | 1998 |
| | Czech Republic | 1999 |
| | Czech Republic | 2000 |
| | Hungary | 2000 |
| | Hungary | 2001 |
| | Korea, Rep. | 1988 |
| | Mexico | 2000 |
| | Panama | 2008 |
| | Poland | 2003 |
| | Portugal | 1988 |
| | Trinidad and Tobago | 2000 |
| | Uruguay | 1994 |
| | Uruguay | 1995 |
| | Uruguay | 2007 |
| | Venezuela, RB | 1987 |
| | Venezuela, RB | 2001 |
| | Venezuela, RB | 2006 |
| Treated Non-allies | Gabon | 2007 |
| | Gabon | 2008 |
| | Croatia | 2003 |
| | Lithuania | 2004 |
| | Malaysia | 2007 |
| | Malaysia | 2008 |
| Control Non-allies | Botswana | 2006 |
| | Botswana | 2008 |
| | Estonia | 2003 |
| | Gabon | 1992 |
| | Croatia | 2002 |
| | Kazakhstan | 2008 |
| | Lebanon | 2005 |
| | Lebanon | 2008 |
| | Libya | 2002 |
| | Libya | 2005 |
| | Latvia | 2004 |
| | Montenegro | 2008 |
| | Mauritius | 2008 |
| | Romania | 2007 |
| | Russian Federation | 2006 |
| | Slovak Republic | 2003 |

Table 27: US Ally Cases, 2 yr. (Polity Score)

| Category | Country | Year |
|---|---|---|
| Treated US Allies | Argentina | 2008 |
| | Antigua and Barbuda | 1990 |
| | Antigua and Barbuda | 1991 |
| | Antigua and Barbuda | 1992 |
| | Czech Republic | 2001 |
| | Czech Republic | 2002 |
| | Hungary | 2002 |
| | St. Kitts and Nevis | 1996 |
| | Korea, Rep. | 1989 |
| | Mexico | 2001 |
| | Poland | 2004 |
| | Portugal | 1989 |
| | Trinidad and Tobago | 2001 |
| | Uruguay | 1996 |
| | Uruguay | 2001 |
| Control US Allies | Argentina | 2007 |
| | Brazil | 2007 |
| | Chile | 1996 |
| | Chile | 1997 |
| | Chile | 1998 |
| | Chile | 1999 |
| | Chile | 2005 |
| | Czech Republic | 1997 |
| | Czech Republic | 1998 |
| | Czech Republic | 1999 |
| | Czech Republic | 2000 |
| | Hungary | 2000 |
| | Hungary | 2001 |
| | St. Kitts and Nevis | 1992 |
| | St. Kitts and Nevis | 1993 |
| | St. Kitts and Nevis | 1994 |
| | Korea, Rep. | 1988 |
| | Mexico | 2000 |
| | Panama | 2008 |
| | Poland | 2003 |
| | Portugal | 1988 |
| | Trinidad and Tobago | 2000 |
| | Uruguay | 1994 |
| | Uruguay | 1995 |
| | Uruguay | 2007 |
| | Venezuela, RB | 1987 |
| | Venezuela, RB | 2001 |
| | Venezuela, RB | 2006 |
| Treated Non-allies | Gabon | 2007 |
| | Gabon | 2008 |
| | Croatia | 2003 |
| | Lithuania | 2004 |
| | Malaysia | 2007 |
| | Malaysia | 2008 |
| | Seychelles | 1989 |
| | Seychelles | 1990 |
| Control Non-allies | Botswana | 2006 |
| | Botswana | 2008 |
| | Estonia | 2003 |
| | Gabon | 1992 |
| | Croatia | 2002 |
| | Kazakhstan | 2008 |
| | Lebanon | 2005 |
| | Lebanon | 2008 |
| | Libya | 2002 |
| | Libya | 2005 |
| | Latvia | 2004 |
| | Montenegro | 2008 |
| | Mauritius | 2008 |
| | Romania | 2007 |
| | Russian Federation | 2006 |
| | Slovak Republic | 2003 |
| | Seychelles | 1987 |
| | Seychelles | 1988 |

Table 28: US Ally Cases, 2 yr. (UDS Score)

## K.3 Checking for Ceiling Effects for EU Candidates and U.S. Allies

Table 29: Checking ceiling effects for political liberalization (EU candidates vs non-candidates, control obs. only)

|  | FH | Polity | FH-Polity | UDS |
|---|---|---|---|---|
| (Constant) | 0.12 | 0.43 | 0.45 | 0.10 |
|  | (0.41) | (0.39) | (0.45) | (0.43) |
| EU candidate | 1.05** | 0.73* | 0.88* | 1.29*** |
|  | (0.30) | (0.30) | (0.34) | (0.31) |
| Log GNI/cap. - c | 5.13 | 2.84 | 3.54 | 7.46 |
|  | (5.41) | (4.37) | (5.34) | (6.11) |
| $N$ | 29 | 23 | 23 | 29 |
| $R^2$ | 0.20 | 0.15 | 0.19 | 0.24 |
| adj. $R^2$ | 0.14 | 0.07 | 0.11 | 0.18 |
| Resid. sd | 0.85 | 0.75 | 0.79 | 0.92 |

Ordinary least squares estimates within 0.10 bandwidth to the left of cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; [*] $p < .05$; [**] $p < .01$; [***] $p < .001$

Table 30: Checking ceiling effects for political liberalization (US allies vs non-allies, control obs. only)

|  | FH | Polity | FH-Polity | UDS |
|---|---|---|---|---|
| (Constant) | 0.20 | 0.56[†] | 0.60[†] | 0.28 |
|  | (0.36) | (0.32) | (0.34) | (0.41) |
| US ally | 1.39*** | 1.05** | 1.26** | 1.46*** |
|  | (0.32) | (0.35) | (0.35) | (0.39) |
| Log GNI/cap. - c | −7.27[†] | −6.98 | −8.24[†] | −6.55 |
|  | (3.85) | (4.33) | (4.37) | (4.80) |
| $N$ | 29 | 23 | 23 | 29 |
| $R^2$ | 0.55 | 0.43 | 0.53 | 0.48 |
| adj. $R^2$ | 0.51 | 0.37 | 0.48 | 0.44 |
| Resid. sd | 0.64 | 0.62 | 0.60 | 0.76 |

Ordinary least squares estimates within 0.10 bandwidth to the left of cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; [*] $p < .05$; [**] $p < .01$; [***] $p < .001$

# L  Robustness checks

Figure 4 is a coefficient plot that displays results from a number of robustness checks. The point estimates remain quite stable. The estimates change only slightly when we adjust the kernel or bandwidth (note that a bandwidth smaller than .075 reduces the number of observations to about a dozen, making the estimation highly unreliable). While the placebo tests presented in the main text do not lead to a rejection of the null hypothesis of no effect, the point estimates are not exactly zero. Therefore, to assess the sensitivity to possible "anticipation effects" (e.g., countries adjusting outcomes in anticipation of crossing the threshold), we fit the local linear regressions controlling for outcomes in the baseline year ($Y_t$, labeled as "lagged Y" in the coefficient plot). The estimates do not change appreciably. Estimates on the level outcomes (that is, when the outcome differencing strategy is not used) also yield much noisier estimates (as evident from the substantially wider confidence intervals) that bounce around substantially. When we perform the analysis on levels, we find some evidence of an unusual, negative placebo effect on the $Y_t$ outcomes (Section L.4). Given the presence of this effect, a potential worry is that the effects that we estimate for the changes are tainted, perhaps reflecting mean reversion. To assess this possibility, we estimate the level effects controlling for the $Y_t$ outcomes, which yields estimates that are nearly identical to what we obtain in the analysis with the outcome defined in terms of changes. Tables with the estimates from these robustness checks are shown in the remainder of this section.



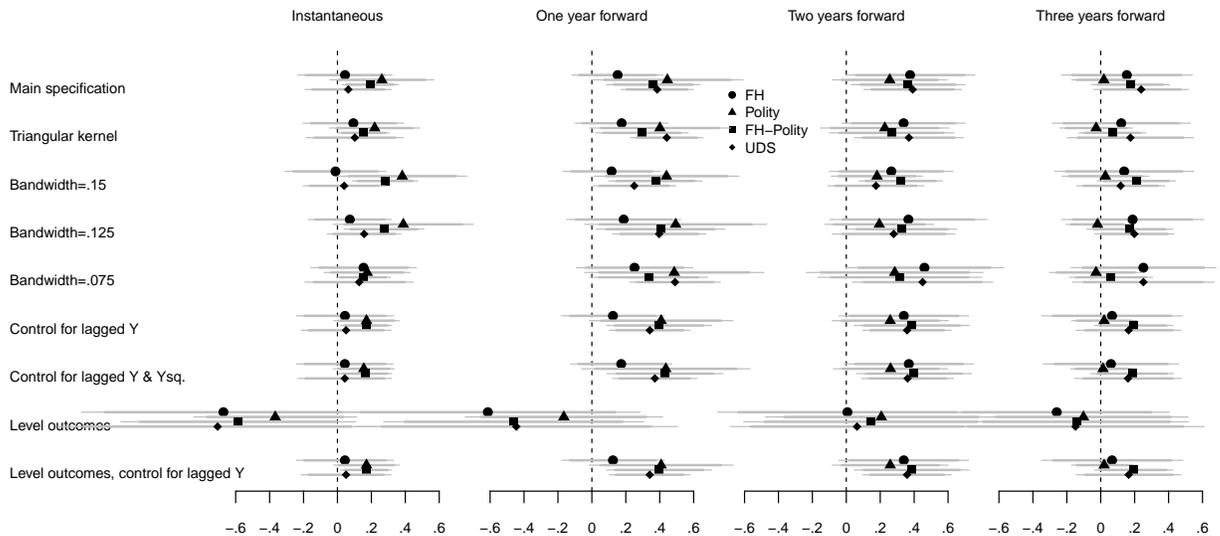Figure 4: Coefficient plot of alternative estimates of the instantaneous and two-years forward effects on Freedom House scores ("FH", dots), Polity scores (triangles), aggregate Freedom House-Polity scores (squares), and Unified Democracy Scores (diamonds). Thin gray segments are 95% confidence intervals, and thicker gray segments are 90% confidence intervals. Full regression output for these estimates is displayed below.

## L.1 Triangular kernel

Table 31: Effects on political liberalization (Freedom House, tri. kernel)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | $-0.08$ | 0.01 | 0.04 | 0.12 | 0.33 |
|  | (0.08) | (0.08) | (0.07) | (0.12) | (0.25) |
| IBRD grad. elig. | $-0.02$ | 0.09 | 0.18 | $0.34^{\dagger}$ | 0.12 |
|  | (0.10) | (0.15) | (0.14) | (0.19) | (0.21) |
| Log GNI/cap. - c | $-1.61$ | $-0.07$ | 0.71 | 1.96 | 3.67 |
|  | (1.32) | (1.32) | (1.23) | (2.01) | (3.88) |
| Interaction term | 3.65 | $-1.60$ | $-3.13$ | $-6.45$ | $-7.76$ |
|  | (2.25) | (2.61) | (3.04) | (3.97) | (5.04) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.04 | 0.01 | 0.06 | 0.12 | 0.05 |
| adj. $R^2$ | 0.00 | $-0.03$ | 0.01 | 0.07 | $-0.01$ |
| Resid. sd | 0.14 | 0.16 | 0.19 | 0.25 | 0.32 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 32: Effects on political liberalization (Polity, tri. kernel)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.06 | 0.03 | $-0.12$ | $-0.16$ | 0.00 |
|  | (0.04) | (0.02) | (0.15) | (0.19) | (0.02) |
| IBRD grad. elig. | $-0.12^{*}$ | 0.22 | $0.40^{\dagger}$ | 0.23 | $-0.03$ |
|  | (0.05) | (0.14) | (0.21) | (0.19) | (0.11) |
| Log GNI/cap. - c | 0.80 | 0.45 | $-1.53$ | $-2.02$ | $-0.12$ |
|  | (0.61) | (0.38) | (1.87) | (2.47) | (0.36) |
| Interaction term | $3.17^{\dagger}$ | $-1.77$ | $-1.35$ | 0.49 | $-0.11$ |
|  | (1.81) | (2.22) | (2.71) | (2.56) | (1.38) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.17 | 0.16 | 0.15 | 0.05 | 0.04 |
| adj. $R^2$ | 0.13 | 0.11 | 0.09 | $-0.03$ | $-0.04$ |
| Resid. sd | 0.10 | 0.14 | 0.20 | 0.17 | 0.07 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 33: Effects on political liberalization (Aggregate Freedom House-Polity, tri. kernel)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.04 | 0.07 | −0.04 | −0.07 | 0.08 |
|  | (0.06) | (0.04) | (0.13) | (0.18) | (0.06) |
| IBRD grad. elig. | −0.11 | 0.15$^\dagger$ | 0.30$^*$ | 0.27 | 0.07 |
|  | (0.07) | (0.08) | (0.14) | (0.19) | (0.10) |
| Log GNI/cap. - c | 0.41 | 0.86 | −0.36 | −0.99 | 0.36 |
|  | (0.82) | (0.72) | (1.79) | (2.52) | (1.11) |
| Interaction term | 2.93$^\dagger$ | −2.54$^\dagger$ | −2.16 | −0.48 | −1.10 |
|  | (1.61) | (1.45) | (2.16) | (2.84) | (1.83) |
| N | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.11 | 0.24 | 0.22 | 0.12 | 0.04 |
| adj. $R^2$ | 0.07 | 0.19 | 0.17 | 0.05 | −0.04 |
| Resid. sd | 0.10 | 0.08 | 0.13 | 0.15 | 0.10 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 34: Effects on political liberalization (Unified Democracy Scores, tri. kernel)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.04 | 0.06 | −0.04 | 0.06 | 0.22 |
|  | (0.09) | (0.10) | (0.07) | (0.12) | (0.23) |
| IBRD grad. elig. | −0.15 | 0.10 | 0.44$^{***}$ | 0.37$^*$ | 0.18 |
|  | (0.11) | (0.15) | (0.11) | (0.16) | (0.19) |
| Log GNI/cap. - c | −0.54 | 0.70 | −0.93 | 0.35 | 2.88 |
|  | (1.61) | (1.58) | (1.16) | (1.86) | (3.30) |
| Interaction term | 4.06$^\dagger$ | −2.19 | −3.45 | −4.90 | −6.50 |
|  | (2.26) | (2.75) | (2.75) | (3.29) | (4.37) |
| N | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.07 | 0.04 | 0.25 | 0.13 | 0.07 |
| adj. $R^2$ | 0.03 | 0.00 | 0.21 | 0.08 | 0.01 |
| Resid. sd | 0.15 | 0.14 | 0.16 | 0.21 | 0.26 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

## L.2 Alternative bandwidths

### L.2.1 0.15 bandwidth

Table 35: Effects on political liberalization (Freedom House, alternative bandwidth)

|                   | Placebo | Instant.           | 1 yr.  | 2 yr.  | 3 yr.  |
|-------------------|---------|--------------------|--------|--------|--------|
| (Constant)        | −0.08   | −0.05              | −0.06  | −0.02  | 0.12   |
|                   | (0.07)  | (0.06)             | (0.07) | (0.10) | (0.17) |
| IBRD grad. elig.  | −0.00   | −0.01              | 0.12   | 0.27   | 0.14   |
|                   | (0.09)  | (0.15)             | (0.15) | (0.19) | (0.21) |
| Log GNI/cap. - c  | −1.34   | −1.22[†]           | −1.47  | −1.57  | −1.37  |
|                   | (0.98)  | (0.73)             | (0.95) | (1.14) | (1.79) |
| Interaction term  | 2.95**  | 3.03               | 2.73   | 1.81   | 1.60   |
|                   | (1.09)  | (2.28)             | (2.37) | (2.18) | (2.11) |
| $N$               | 123     | 106                | 94     | 84     | 77     |
| $R^2$             | 0.06    | 0.05               | 0.04   | 0.05   | 0.01   |
| adj. $R^2$        | 0.03    | 0.02               | 0.01   | 0.01   | −0.03  |
| Resid. sd         | 0.25    | 0.28               | 0.35   | 0.41   | 0.51   |

Ordinary least squares estimates within 0.15 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

Table 36: Effects on political liberalization (Polity, alternative bandwidth)

|                   | Placebo  | Instant. | 1 yr.     | 2 yr.     | 3 yr.     |
|-------------------|----------|----------|-----------|-----------|-----------|
| (Constant)        | −0.07    | −0.04    | −0.14     | −0.17     | −0.08     |
|                   | (0.11)   | (0.04)   | (0.10)    | (0.12)    | (0.07)    |
| IBRD grad. elig.  | 0.19[†]  | 0.38[†]  | 0.44*     | 0.18      | 0.03      |
|                   | (0.11)   | (0.19)   | (0.22)    | (0.14)    | (0.13)    |
| Log GNI/cap. - c  | −1.75    | −0.90    | −1.91[†]  | −2.40[†]  | −1.80[†]  |
|                   | (1.91)   | (0.58)   | (1.02)    | (1.22)    | (1.04)    |
| Interaction term  | 1.72     | −2.68    | −1.67     | 2.27[†]   | 2.30*     |
|                   | (2.15)   | (2.15)   | (1.72)    | (1.14)    | (0.95)    |
| $N$               | 106      | 89       | 77        | 67        | 60        |
| $R^2$             | 0.04     | 0.11     | 0.13      | 0.12      | 0.15      |
| adj. $R^2$        | 0.01     | 0.07     | 0.09      | 0.08      | 0.10      |
| Resid. sd         | 0.30     | 0.25     | 0.28      | 0.23      | 0.19      |

Ordinary least squares estimates within 0.15 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

Table 37: Effects on political liberalization (Aggregate Freedom House and Polity, alternative bandwidth)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.06 | −0.03 | −0.11 | −0.13 | −0.05 |
|  | (0.09) | (0.05) | (0.09) | (0.12) | (0.10) |
| IBRD grad. elig. | 0.10 | 0.28** | 0.38** | 0.32* | 0.21† |
|  | (0.10) | (0.10) | (0.14) | (0.13) | (0.12) |
| Log GNI/cap. - c | −1.41 | −0.95 | −1.87† | −2.39† | −2.31† |
|  | (1.57) | (0.71) | (0.99) | (1.22) | (1.24) |
| Interaction term | 2.09 | −1.57 | −0.95 | 1.00 | 1.16 |
|  | (1.69) | (1.42) | (1.23) | (1.44) | (1.48) |
| $N$ | 106 | 89 | 77 | 67 | 60 |
| $R^2$ | 0.05 | 0.10 | 0.14 | 0.13 | 0.11 |
| adj. $R^2$ | 0.02 | 0.07 | 0.10 | 0.09 | 0.07 |
| Resid. sd | 0.24 | 0.19 | 0.23 | 0.23 | 0.26 |

Ordinary least squares estimates within 0.15 bandwidth around cut point.

Standard errors account for clustering by country.

† significant at $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

Table 38: Effects on political liberalization (Unified Democracy Score, alternative bandwidth)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.02 | −0.03 | −0.08 | 0.01 | 0.06 |
|  | (0.07) | (0.06) | (0.07) | (0.09) | (0.15) |
| IBRD grad. elig. | −0.01 | 0.04 | 0.25* | 0.17 | 0.12 |
|  | (0.09) | (0.12) | (0.12) | (0.14) | (0.13) |
| Log GNI/cap. - c | −0.73 | −1.14† | −1.73* | −1.10 | −1.12 |
|  | (0.89) | (0.65) | (0.77) | (0.87) | (1.56) |
| Interaction term | 1.63 | 2.67 | 2.31 | 1.92 | 2.28 |
|  | (1.10) | (2.18) | (1.94) | (1.34) | (1.63) |
| $N$ | 123 | 106 | 94 | 84 | 77 |
| $R^2$ | 0.02 | 0.06 | 0.08 | 0.05 | 0.03 |
| adj. $R^2$ | −0.01 | 0.03 | 0.05 | 0.02 | −0.01 |
| Resid. sd | 0.24 | 0.25 | 0.32 | 0.35 | 0.42 |

Ordinary least squares estimates within 0.15 bandwidth around cut point.

Standard errors account for clustering by country.

† significant at $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

## L.2.2  0.125 bandwidth

Table 39: Effects on political liberalization (Freedom House, alternative bandwidth)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.03 | 0.01 | 0.02 | 0.03 | 0.22 |
|  | (0.07) | (0.06) | (0.07) | (0.12) | (0.20) |
| IBRD grad. elig. | −0.02 | 0.07 | 0.19 | 0.37 | 0.19 |
|  | (0.09) | (0.13) | (0.17) | (0.24) | (0.21) |
| Log GNI/cap. - c | −0.36 | −0.22 | −0.01 | −0.50 | 0.82 |
|  | (0.84) | (0.54) | (1.04) | (1.49) | (2.30) |
| Interaction term | 1.44 | −0.67 | −1.76 | −2.28 | −3.75 |
|  | (1.01) | (1.42) | (1.70) | (2.79) | (3.52) |
| $N$ | 97 | 84 | 73 | 63 | 59 |
| $R^2$ | 0.02 | 0.01 | 0.03 | 0.07 | 0.02 |
| adj. $R^2$ | −0.01 | −0.03 | −0.01 | 0.02 | −0.03 |
| Resid. sd | 0.22 | 0.22 | 0.30 | 0.36 | 0.44 |

Ordinary least squares estimates within 0.125 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; [*] $p < .05$; [**] $p < .01$; [***] $p < .001$

Table 40: Effects on political liberalization (Polity, alternative bandwidth)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.05 | 0.02 | −0.12 | −0.16 | −0.02 |
|  | (0.03) | (0.01) | (0.12) | (0.16) | (0.02) |
| IBRD grad. elig. | 0.02 | 0.39[†] | 0.49[†] | 0.19 | −0.02 |
|  | (0.07) | (0.21) | (0.27) | (0.16) | (0.09) |
| Log GNI/cap. - c | 0.50 | 0.17 | −1.43 | −2.08 | −0.60 |
|  | (0.30) | (0.18) | (1.35) | (1.76) | (0.36) |
| Interaction term | 0.39 | −5.11[†] | −3.94[†] | 1.37 | 0.66 |
|  | (1.23) | (2.68) | (2.05) | (1.81) | (0.86) |
| $N$ | 85 | 71 | 60 | 50 | 46 |
| $R^2$ | 0.08 | 0.17 | 0.18 | 0.06 | 0.09 |
| adj. $R^2$ | 0.05 | 0.13 | 0.14 | 0.00 | 0.03 |
| Resid. sd | 0.20 | 0.23 | 0.27 | 0.20 | 0.10 |

Ordinary least squares estimates within 0.125 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; [*] $p < .05$; [**] $p < .01$; [***] $p < .001$

Table 41: Effects on political liberalization (Aggregate Freedom House and Polity, alternative bandwidth)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.04 | 0.03 | −0.07 | −0.13 | −0.01 |
|  | (0.05) | (0.04) | (0.11) | (0.15) | (0.08) |
| IBRD grad. elig. | −0.02 | 0.27* | 0.40* | 0.33[†] | 0.17 |
|  | (0.06) | (0.12) | (0.19) | (0.16) | (0.13) |
| Log GNI/cap. - c | 0.55 | 0.14 | −1.24 | −2.40 | −1.45 |
|  | (0.52) | (0.38) | (1.17) | (1.71) | (1.05) |
| Interaction term | 0.44 | −3.78* | −2.99* | 0.91 | 0.15 |
|  | (0.79) | (1.61) | (1.43) | (1.84) | (1.46) |
| $N$ | 85 | 71 | 60 | 50 | 46 |
| $R^2$ | 0.09 | 0.21 | 0.20 | 0.12 | 0.07 |
| adj. $R^2$ | 0.05 | 0.17 | 0.16 | 0.07 | 0.00 |
| Resid. sd | 0.16 | 0.15 | 0.20 | 0.19 | 0.17 |

Ordinary least squares estimates within 0.125 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

Table 42: Effects on political liberalization (Unified Democracy Score, alternative bandwidth)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.08 | 0.01 | −0.07 | 0.04 | 0.11 |
|  | (0.08) | (0.07) | (0.07) | (0.08) | (0.17) |
| IBRD grad. elig. | −0.07 | 0.16 | 0.40** | 0.28 | 0.20 |
|  | (0.09) | (0.11) | (0.14) | (0.18) | (0.12) |
| Log GNI/cap. - c | 0.47 | −0.46 | −1.62 | −0.41 | 0.12 |
|  | (0.94) | (0.72) | (1.04) | (1.12) | (2.02) |
| Interaction term | 0.28 | −1.02 | −0.94 | −1.46 | −1.74 |
|  | (1.19) | (1.65) | (1.30) | (2.27) | (3.25) |
| $N$ | 97 | 84 | 73 | 63 | 59 |
| $R^2$ | 0.01 | 0.03 | 0.09 | 0.05 | 0.03 |
| adj. $R^2$ | −0.02 | −0.01 | 0.05 | 0.00 | −0.03 |
| Resid. sd | 0.23 | 0.20 | 0.29 | 0.33 | 0.38 |

Ordinary least squares estimates within 0.125 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$

## L.2.3 0.075 bandwidth

Table 43: Effects on political liberalization (Freedom House, alternative bandwidth)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | $-0.09$ | $-0.01$ | 0.04 | 0.12 | 0.33 |
|  | (0.09) | (0.10) | (0.08) | (0.15) | (0.26) |
| IBRD grad. elig. | 0.02 | 0.15 | 0.25 | $0.46^{\dagger}$ | 0.25 |
|  | (0.12) | (0.16) | (0.18) | (0.24) | (0.22) |
| Log GNI/cap. - c | $-2.17$ | $-0.77$ | 1.04 | 2.29 | 4.15 |
|  | (1.75) | (2.14) | (2.19) | (3.26) | (4.53) |
| Interaction term | 2.89 | $-2.43$ | $-6.84^{\dagger}$ | $-11.55^{\dagger}$ | $-13.40$ |
|  | (2.93) | (2.88) | (3.51) | (6.80) | (8.19) |
| $N$ | 52 | 45 | 39 | 34 | 32 |
| $R^2$ | 0.02 | 0.04 | 0.11 | 0.17 | 0.11 |
| adj. $R^2$ | $-0.04$ | $-0.03$ | 0.04 | 0.08 | 0.01 |
| Resid. sd | 0.23 | 0.25 | 0.27 | 0.37 | 0.46 |

Ordinary least squares estimates within 0.075 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 44: Effects on political liberalization (Polity, alternative bandwidth)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.06 | 0.03 | $-0.18$ | $-0.23$ | $-0.01$ |
|  | (0.05) | (0.03) | (0.20) | (0.26) | (0.05) |
| IBRD grad. elig. | $-0.11$ | 0.18 | $0.49^{\dagger}$ | 0.29 | $-0.03$ |
|  | (0.06) | (0.13) | (0.27) | (0.27) | (0.14) |
| Log GNI/cap. - c | 0.64 | 0.41 | $-3.01$ | $-3.89$ | $-0.56$ |
|  | (0.88) | (0.46) | (3.18) | (4.24) | (1.29) |
| Interaction term | 2.92 | 0.32 | $-0.75$ | 2.92 | 1.14 |
|  | (2.51) | (3.63) | (4.02) | (4.27) | (2.36) |
| $N$ | 45 | 36 | 30 | 25 | 23 |
| $R^2$ | 0.12 | 0.16 | 0.15 | 0.06 | 0.04 |
| adj. $R^2$ | 0.05 | 0.09 | 0.05 | $-0.07$ | $-0.11$ |
| Resid. sd | 0.19 | 0.25 | 0.32 | 0.27 | 0.10 |

Ordinary least squares estimates within 0.075 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 45: Effects on political liberalization (Aggregate Freedom House and Polity, alternative bandwidth)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.03 | 0.05 | −0.06 | −0.12 | 0.08 |
| | (0.07) | (0.06) | (0.17) | (0.25) | (0.09) |
| IBRD grad. elig. | −0.08 | 0.15$^\dagger$ | 0.33$^\dagger$ | 0.32 | 0.06 |
| | (0.09) | (0.08) | (0.18) | (0.25) | (0.13) |
| Log GNI/cap. - c | 0.11 | 0.63 | −0.80 | −2.00 | 0.81 |
| | (1.20) | (1.35) | (2.98) | (4.32) | (2.31) |
| Interaction term | 2.04 | −1.57 | −2.15 | 0.51 | −1.45 |
| | (1.91) | (2.34) | (3.43) | (4.81) | (3.34) |
| N | 45 | 36 | 30 | 25 | 23 |
| $R^2$ | 0.04 | 0.21 | 0.21 | 0.12 | 0.04 |
| adj. $R^2$ | −0.02 | 0.13 | 0.12 | −0.00 | −0.11 |
| Resid. sd | 0.16 | 0.14 | 0.21 | 0.25 | 0.19 |

Ordinary least squares estimates within 0.075 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 46: Effects on political liberalization (Unified Democracy Score, alternative bandwidth)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.02 | 0.08 | −0.02 | 0.03 | 0.20 |
| | (0.10) | (0.12) | (0.09) | (0.15) | (0.24) |
| IBRD grad. elig. | −0.11 | 0.13 | 0.49$^{**}$ | 0.45$^*$ | 0.25 |
| | (0.13) | (0.16) | (0.14) | (0.21) | (0.21) |
| Log GNI/cap. - c | −1.03 | 1.07 | −0.20 | −0.68 | 2.25 |
| | (2.18) | (2.39) | (1.71) | (2.71) | (3.91) |
| Interaction term | 3.54 | −4.26 | −7.15$^{**}$ | −5.71 | −7.96 |
| | (2.83) | (3.00) | (2.07) | (6.18) | (7.34) |
| N | 52 | 45 | 39 | 34 | 32 |
| $R^2$ | 0.04 | 0.07 | 0.36 | 0.15 | 0.08 |
| adj. $R^2$ | −0.02 | 0.01 | 0.30 | 0.07 | −0.02 |
| Resid. sd | 0.23 | 0.19 | 0.20 | 0.29 | 0.37 |

Ordinary least squares estimates within 0.075 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

## L.3  Controlling for baseline

Table 47: Effects on political liberalization (Freedom House, lagged DV)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.11 | 0.02 | −0.06 | 0.02 | 0.47 |
|  | (0.11) | (0.11) | (0.16) | (0.25) | (0.33) |
| IBRD grad. elig. | −0.05 | 0.04 | 0.17 | $0.37^\dagger$ | 0.06 |
|  | (0.10) | (0.15) | (0.15) | (0.19) | (0.20) |
| Log GNI/cap. - c | −0.96 | −0.04 | −0.71 | −0.74 | 1.65 |
|  | (1.13) | (0.81) | (1.39) | (2.38) | (3.16) |
| Lagged Y | 0.01 | −0.00 | −0.05 | −0.11 | −0.21 |
|  | (0.02) | (0.03) | (0.08) | (0.13) | (0.17) |
| Lagged Y sq. | 0.02 | −0.00 | 0.06 | 0.06 | −0.01 |
|  | (0.04) | (0.04) | (0.07) | (0.10) | (0.13) |
| Interaction term | $3.46^*$ | −0.38 | 0.32 | −1.50 | −2.79 |
|  | (1.72) | (2.12) | (3.02) | (2.82) | (3.01) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.06 | 0.00 | 0.06 | 0.14 | 0.19 |
| adj. $R^2$ | −0.01 | −0.08 | −0.02 | 0.05 | 0.10 |
| Resid. sd | 0.23 | 0.23 | 0.30 | 0.37 | 0.43 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^*p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 48: Effects on political liberalization (Polity, lagged DV)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.29* | 0.30* | −0.16 | −0.47 | 0.01 |
| | (0.13) | (0.12) | (0.37) | (0.39) | (0.08) |
| IBRD grad. elig. | −0.09 | 0.16$^{\dagger}$ | 0.44$^{\dagger}$ | 0.26 | 0.01 |
| | (0.07) | (0.09) | (0.25) | (0.17) | (0.10) |
| Log GNI/cap. - c | 1.05$^{\dagger}$ | 1.52$^{\dagger}$ | −1.83 | −3.52 | −0.85* |
| | (0.56) | (0.89) | (2.41) | (2.57) | (0.39) |
| Lagged Y | −0.00 | −0.13 | −0.07 | 0.05 | 0.02 |
| | (0.02) | (0.08) | (0.10) | (0.04) | (0.01) |
| Lagged Y sq. | −0.16* | −0.06 | 0.07 | 0.15 | −0.04 |
| | (0.08) | (0.06) | (0.14) | (0.14) | (0.05) |
| Interaction term | 1.36 | −3.27* | −1.78 | 1.98 | 0.39 |
| | (1.58) | (1.32) | (2.63) | (2.87) | (0.94) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.30 | 0.36 | 0.18 | 0.21 | 0.16 |
| adj. $R^2$ | 0.24 | 0.30 | 0.08 | 0.10 | 0.04 |
| Resid. sd | 0.17 | 0.18 | 0.25 | 0.21 | 0.10 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 49: Effects on political liberalization (Aggregate Freedom House and Polity, lagged DV)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.10 | 0.11$^{\dagger}$ | −0.25 | −0.45 | −0.08 |
| | (0.08) | (0.06) | (0.22) | (0.28) | (0.13) |
| IBRD grad. elig. | −0.10 | 0.16* | 0.43* | 0.40* | 0.19 |
| | (0.07) | (0.08) | (0.17) | (0.17) | (0.12) |
| Log GNI/cap. - c | 0.69 | 0.65 | −2.31 | −4.26$^{\dagger}$ | −2.22$^{\dagger}$ |
| | (0.63) | (0.59) | (1.90) | (2.39) | (1.28) |
| Lagged Y | 0.01 | −0.02 | 0.02 | 0.08** | 0.06* |
| | (0.02) | (0.03) | (0.03) | (0.02) | (0.02) |
| Lagged Y sq. | −0.04 | −0.01 | 0.06 | 0.08 | −0.02 |
| | (0.03) | (0.02) | (0.04) | (0.06) | (0.03) |
| Interaction term | 1.96 | −2.52* | −1.19 | 2.45 | 1.24 |
| | (1.37) | (1.15) | (2.02) | (2.53) | (1.61) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.15 | 0.22 | 0.26 | 0.33 | 0.13 |
| adj. $R^2$ | 0.08 | 0.14 | 0.17 | 0.23 | −0.00 |
| Resid. sd | 0.17 | 0.12 | 0.17 | 0.18 | 0.17 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 50: Effects on political liberalization (Unified Democracy Score, lagged DV)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.08 | 0.11 | −0.04 | 0.12 | 0.40[†] |
| | (0.10) | (0.09) | (0.11) | (0.15) | (0.22) |
| IBRD grad. elig. | −0.14 | 0.04 | 0.37** | 0.36* | 0.16 |
| | (0.11) | (0.14) | (0.13) | (0.14) | (0.16) |
| Log GNI/cap. - c | −0.11 | 1.07 | −1.04 | −0.54 | 1.90 |
| | (1.18) | (0.91) | (1.07) | (1.72) | (2.37) |
| Lagged Y | 0.02 | −0.01 | −0.08 | −0.11 | −0.19 |
| | (0.03) | (0.01) | (0.05) | (0.11) | (0.14) |
| Lagged Y sq. | −0.02 | −0.01 | 0.04 | 0.01 | −0.01 |
| | (0.03) | (0.02) | (0.03) | (0.05) | (0.07) |
| Interaction term | 2.91 | −1.62 | −1.42 | −2.87 | −4.42[†] |
| | (2.02) | (2.43) | (3.38) | (2.45) | (2.24) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.05 | 0.06 | 0.18 | 0.16 | 0.31 |
| adj. $R^2$ | −0.02 | −0.01 | 0.10 | 0.07 | 0.23 |
| Resid. sd | 0.24 | 0.20 | 0.27 | 0.33 | 0.33 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$

## L.4 Level Outcomes

Table 51: Effects on political liberalization (Freedom House, level outcomes)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 1.07*** | 1.25*** | 1.21** | 1.14** | 1.39*** |
|  | (0.27) | (0.36) | (0.39) | (0.38) | (0.30) |
| IBRD grad. elig. | −0.42 | −0.67 | −0.61 | 0.01 | −0.26 |
|  | (0.34) | (0.43) | (0.46) | (0.39) | (0.34) |
| Log GNI/cap. - c | 4.62 | 7.50 | 6.05 | 5.07 | 6.30 |
|  | (4.58) | (5.82) | (6.28) | (6.63) | (6.43) |
| Interaction term | −5.00 | −4.74 | −0.16 | −5.30 | −5.92 |
|  | (6.09) | (7.69) | (8.37) | (7.59) | (7.53) |
| $N$ | 83 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.02 | 0.04 | 0.03 | 0.04 | 0.04 |
| adj. $R^2$ | −0.01 | −0.01 | −0.02 | −0.02 | −0.02 |
| Resid. sd | 0.92 | 0.94 | 0.94 | 0.92 | 0.85 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; [*]$p < .05$; [**]$p < .01$; [***]$p < .001$

Table 52: Effects on political liberalization (Polity, level outcomes)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 1.13*** | 1.41*** | 1.17*** | 1.11** | 1.32*** |
|  | (0.21) | (0.23) | (0.30) | (0.35) | (0.28) |
| IBRD grad. elig. | −0.27 | −0.37 | −0.17 | 0.21 | −0.10 |
|  | (0.31) | (0.25) | (0.30) | (0.35) | (0.31) |
| Log GNI/cap. - c | 4.28 | 8.47 | 4.53 | 3.84 | 6.28 |
|  | (3.75) | (5.18) | (5.43) | (6.22) | (6.03) |
| Interaction term | −5.79 | −8.67 | −2.22 | −5.88 | −7.13 |
|  | (4.85) | (5.55) | (6.14) | (6.67) | (6.25) |
| $N$ | 72 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.02 | 0.09 | 0.05 | 0.07 | 0.08 |
| adj. $R^2$ | −0.02 | 0.04 | −0.01 | −0.01 | −0.00 |
| Resid. sd | 0.75 | 0.64 | 0.67 | 0.70 | 0.67 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

[†] significant at $p < .10$; [*]$p < .05$; [**]$p < .01$; [***]$p < .001$

Table 53: Effects on political liberalization (Aggregate Freedom House and Polity, level outcomes)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 1.33*** | 1.67*** | 1.51*** | 1.41*** | 1.65*** |
| | (0.23) | (0.25) | (0.31) | (0.38) | (0.31) |
| IBRD grad. elig. | −0.37 | −0.58 | −0.46 | 0.15 | −0.14 |
| | (0.36) | (0.35) | (0.39) | (0.38) | (0.34) |
| Log GNI/cap. - c | 5.97 | 10.99$^\dagger$ | 7.89 | 6.50 | 8.36 |
| | (4.37) | (5.77) | (6.21) | (7.22) | (7.22) |
| Interaction term | −7.68 | −10.62 | −3.59 | −7.92 | −9.12 |
| | (5.57) | (6.51) | (7.65) | (7.74) | (7.56) |
| $N$ | 72 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.03 | 0.10 | 0.08 | 0.10 | 0.10 |
| adj. $R^2$ | −0.01 | 0.05 | 0.01 | 0.03 | 0.03 |
| Resid. sd | 0.82 | 0.77 | 0.79 | 0.81 | 0.77 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^* p < .05$; $^{**} p < .01$; $^{***} p < .001$

Table 54: Effects on political liberalization (Unified Democracy Score, level outcomes)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 1.17*** | 1.44** | 1.32** | 1.25** | 1.45*** |
| | (0.33) | (0.44) | (0.46) | (0.45) | (0.39) |
| IBRD grad. elig. | −0.43 | −0.71 | −0.45 | 0.06 | −0.15 |
| | (0.41) | (0.48) | (0.48) | (0.38) | (0.39) |
| Log GNI/cap. - c | 4.39 | 8.47 | 5.72 | 4.54 | 6.53 |
| | (4.77) | (6.38) | (6.51) | (7.02) | (6.78) |
| Interaction term | −3.73 | −4.66 | −0.49 | −5.29 | −6.74 |
| | (6.63) | (8.15) | (8.33) | (8.81) | (8.68) |
| $N$ | 83 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.02 | 0.04 | 0.03 | 0.03 | 0.04 |
| adj. $R^2$ | −0.02 | −0.01 | −0.02 | −0.03 | −0.02 |
| Resid. sd | 1.00 | 1.01 | 1.01 | 0.99 | 0.92 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^\dagger$ significant at $p < .10$; $^* p < .05$; $^{**} p < .01$; $^{***} p < .001$

## L.5 Level Outcomes and controlling for baseline

Table 55: Effects on political liberalization (Freedom House, level outcomes and lagged DV)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | 0.08 | 0.02 | 0.03 | 0.11 | 0.45 |
|  | (0.08) | (0.07) | (0.11) | (0.20) | (0.30) |
| IBRD grad. elig. | 0.05 | 0.04 | 0.12 | $0.34^{\dagger}$ | 0.07 |
|  | (0.10) | (0.15) | (0.15) | (0.20) | (0.21) |
| Log GNI/cap. - c | 1.00 | −0.04 | −0.47 | −0.58 | 1.61 |
|  | (1.13) | (0.78) | (1.20) | (2.20) | (3.10) |
| Lagged Y | 0.98*** | 1.00*** | 0.96*** | 0.90*** | 0.79*** |
|  | (0.02) | (0.03) | (0.07) | (0.11) | (0.16) |
| Interaction term | −3.55* | −0.38 | 0.51 | −1.45 | −2.79 |
|  | (1.69) | (2.10) | (2.97) | (2.65) | (3.00) |
| N | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.94 | 0.94 | 0.90 | 0.85 | 0.77 |
| adj. $R^2$ | 0.93 | 0.94 | 0.89 | 0.84 | 0.75 |
| Resid. sd | 0.23 | 0.23 | 0.30 | 0.37 | 0.42 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 56: Effects on political liberalization (Polity, level outcomes and lagged DV)

|  | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | $-0.09^{\dagger}$ | 0.22* | −0.06 | −0.26 | −0.05* |
|  | (0.05) | (0.10) | (0.21) | (0.22) | (0.02) |
| IBRD grad. elig. | 0.07 | $0.17^{\dagger}$ | $0.41^{\dagger}$ | 0.26 | 0.02 |
|  | (0.06) | (0.10) | (0.22) | (0.17) | (0.10) |
| Log GNI/cap. - c | $-0.91^{\dagger}$ | 1.37 | −1.56 | −2.96 | −1.05* |
|  | (0.46) | (0.87) | (1.99) | (2.22) | (0.49) |
| Lagged Y | 1.03*** | 0.86*** | 0.94*** | 1.06*** | 1.01*** |
|  | (0.03) | (0.09) | (0.09) | (0.06) | (0.01) |
| Interaction term | −1.46 | −3.21* | −1.75 | 1.35 | 0.61 |
|  | (1.52) | (1.42) | (2.51) | (2.43) | (0.97) |
| N | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.95 | 0.93 | 0.87 | 0.91 | 0.98 |
| adj. $R^2$ | 0.95 | 0.92 | 0.86 | 0.91 | 0.98 |
| Resid. sd | 0.18 | 0.18 | 0.25 | 0.22 | 0.10 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 57: Effects on political liberalization (Aggregate Freedom House and Polity, level outcomes and lagged DV)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.06 | 0.09$^{\dagger}$ | −0.16 | −0.32 | −0.12 |
| | (0.06) | (0.05) | (0.17) | (0.22) | (0.10) |
| IBRD grad. elig. | 0.10 | 0.17$^{*}$ | 0.40$^{*}$ | 0.39$^{*}$ | 0.19 |
| | (0.07) | (0.08) | (0.16) | (0.17) | (0.12) |
| Log GNI/cap. - c | −0.73 | 0.62 | −2.03 | −3.93$^{\dagger}$ | −2.37$^{\dagger}$ |
| | (0.63) | (0.59) | (1.74) | (2.20) | (1.28) |
| Lagged Y | 1.01$^{***}$ | 0.97$^{***}$ | 1.04$^{***}$ | 1.10$^{***}$ | 1.06$^{***}$ |
| | (0.02) | (0.03) | (0.04) | (0.06) | (0.02) |
| Interaction term | −1.89 | −2.52$^{*}$ | −1.10 | 2.24 | 1.35 |
| | (1.34) | (1.16) | (1.94) | (2.34) | (1.67) |
| $N$ | 68 | 58 | 49 | 43 | 40 |
| $R^2$ | 0.96 | 0.98 | 0.96 | 0.95 | 0.96 |
| adj. $R^2$ | 0.96 | 0.98 | 0.95 | 0.95 | 0.96 |
| Resid. sd | 0.17 | 0.12 | 0.17 | 0.19 | 0.17 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 58: Effects on political liberalization (Unified Democracy Score, level outcomes and lagged DV)

| | Placebo | Instant. | 1 yr. | 2 yr. | 3 yr. |
|---|---|---|---|---|---|
| (Constant) | −0.05 | 0.10 | 0.02 | 0.13 | 0.39$^{\dagger}$ |
| | (0.09) | (0.07) | (0.08) | (0.14) | (0.23) |
| IBRD grad. elig. | 0.13 | 0.05 | 0.34$^{**}$ | 0.36$^{**}$ | 0.16 |
| | (0.11) | (0.14) | (0.12) | (0.13) | (0.16) |
| Log GNI/cap. - c | 0.10 | 1.01 | −0.80 | −0.51 | 1.87 |
| | (1.16) | (0.88) | (0.93) | (1.65) | (2.30) |
| Lagged Y | 1.00$^{***}$ | 0.98$^{***}$ | 0.95$^{***}$ | 0.90$^{***}$ | 0.81$^{***}$ |
| | (0.02) | (0.02) | (0.04) | (0.09) | (0.12) |
| Interaction term | −2.84 | −1.62 | −1.32 | −2.85 | −4.43$^{\dagger}$ |
| | (1.90) | (2.47) | (3.18) | (2.41) | (2.24) |
| $N$ | 78 | 69 | 60 | 54 | 51 |
| $R^2$ | 0.95 | 0.96 | 0.93 | 0.90 | 0.88 |
| adj. $R^2$ | 0.94 | 0.96 | 0.92 | 0.89 | 0.87 |
| Resid. sd | 0.24 | 0.20 | 0.28 | 0.32 | 0.33 |

Ordinary least squares estimates within 0.10 bandwidth around cut point.

Standard errors account for clustering by country.

$^{\dagger}$ significant at $p < .10$; $^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

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