Supporting materials for:
“From gridlock to ratchet: Conditional cooperation on climate change”

Sam S. Rowan

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SM-1 Full regression tables and summary statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TradeParis	1.01**	0.89*						
	(0.31)	(0.36)						
Trade$^{\text{EITE}}$Paris			0.72**	0.74**				
			(0.18)	(0.22)				
Trade$^{\text{Competition}}$Paris					0.82*	0.85*		
					(0.36)	(0.37)		
Trade$^{\text{Competition, EITE}}$Paris							0.67**	0.67**
							(0.22)	(0.24)
Trade openness		2.39		0.74		6.74		0.98
		(9.92)		(9.60)		(9.52)		(9.88)
Industry		−0.62		−0.65		−0.67		−0.81
		(0.71)		(0.69)		(0.71)		(0.70)
Renewable electricity		0.78		−0.10		2.22+		1.60
		(1.20)		(1.24)		(1.12)		(1.11)
Fossil rents		3.72		−2.11		5.07		2.27
		(8.86)		(8.93)		(8.87)		(8.83)
Paris target	0.71**	0.70**	0.66**	0.65**	0.77**	0.72**	0.74**	0.70**
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
(Intercept)	9.58	6.05	13.46*	27.92	24.16+	−5.08	18.17*	21.65
	(5.82)	(48.23)	(6.01)	(47.89)	(12.40)	(47.03)	(7.99)	(49.32)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	112	112	112	112	112	112	112	112
R^2	0.50	0.51	0.52	0.53	0.48	0.51	0.50	0.52

Table SM-1: Full regression results from models in table 1. Outcome variable is emissions change in 2021 NDC as a percentage of 2010 emissions levels, re-scaled so that positive values are emissions cuts. OLS regression models with standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
IOsParis	4.27**	4.25**	3.26*	3.34*	2.40	3.85**	3.41**
	(1.11)	(1.39)	(1.49)	(1.66)	(1.58)	(1.15)	(1.22)
TradeParis			0.41	0.43			
			(0.41)	(0.43)			
$\textbf{Trade}^{\text{EITE}}$Paris					0.43		
					(0.26)		
$\textbf{Trade}^{\text{Competition}}$Paris						0.48	
						(0.36)	
$\textbf{Trade}^{\text{Competition, EITE}}$Paris							0.39
							(0.24)
Trade exposure		0.51		−1.30			
		(9.78)		(9.94)			
Industry		−0.56		−0.55			
		(0.70)		(0.70)			
Renewable electricity		0.06		−0.08			
		(1.25)		(1.26)			
Fossil rents		5.54		4.72			
		(8.71)		(8.75)			
Paris target	0.70**	0.70**	0.69**	0.69**	0.66**	0.70**	0.69**
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
(Intercept)	127.68**	135.59+	101.77*	121.79	79.99+	130.13**	113.06**
	(33.93)	(73.27)	(42.46)	(74.55)	(44.26)	(33.86)	(34.84)
Controls	No	Yes	No	Yes	No	No	No
Observations	112	112	112	112	112	112	112
R^2	0.52	0.52	0.53	0.53	0.53	0.53	0.53

Table SM-2: Full regression results from models in table 2. Outcome variable is emissions change in 2021 NDC as a percentage of 2010 emissions levels, re-scaled so that positive values are emissions cuts. OLS regression models with standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Variable	N	Mean	Std. Dev.	Min.	Median	Max
Glasgow target	112	-11.7	69.1	-213.2	16.3	53.6
Paris target	112	-13.3	59.7	-162.2	3.5	59.9
IOsParis	112	-30.5	4.3	-35.8	-31.8	-23.6
TradeParis	112	-11.7	15.6	-47.0	-10.9	11.5
$\textbf{Trade}^{\text{EITE}}$Paris	112	-22.8	26.8	-72.6	-22.9	16.8
$\textbf{Trade}^{\text{Competition}}$Paris	112	-31.5	13.1	-55.8	-31.5	-9.0
$\textbf{Trade}^{\text{Competition, EITE}}$Paris	112	-30.0	21.7	-71.6	-25.6	0.8

Table SM-3: Summary statistics for observations in the main models (no missing target data)

	Glasgow	Paris	**IO**Paris	**Trade**Paris	$\textbf{Trade}^{\text{EITE}}$Paris
Glasgow target	1.000	0.675	0.421	0.390	0.495
Paris target	0.675	1.000	0.259	0.264	0.378
IOParis	0.421	0.259	1.000	0.688	0.738
TradeParis	0.390	0.264	0.688	1.000	0.844
$\textbf{Trade}^{\text{EITE}}$Paris	0.495	0.378	0.738	0.844	1.000

Table SM-4: Correlation matrix

SM-2 Measurement

SM-2.1 Climate targets

- M1 Some countries did not submit NDCs with mitigation targets, leading them to have missing values in the dataset. Since these targets are unlikely to be missing at random, in table SM-5, I multiply impute these missing values and re-estimate the relationship. I find the same results.
- M2 Since some countries did not submit NDCs with mitigation targets, this means that their values as trade partners in constructing the spatial weights are also missing. In the main results presented in text, I use the imputed values to generate the spatial weights (see previous item). I now use only the observed (non-imputed) values to generate the spatial weights, where missing trade partners' values stay missing and contribute nothing to measuring peers' trade-weight climate policy. I re-estimate the models and find the same effects as before.
- M3, M4 Some countries set very weak mitigation targets in their NDCs, which would allow emissions to rise 5 or more times by 2030 and still be in compliance with their targets. These outlying values can have high leverage on the coefficients, so I winsorize targets at the 5th and 95th percentiles in the main models. Now, I consider the stability of the coefficients by dropping observations. First, I removed three observations based on their extreme values of their Paris target (Paraguay) or their Glasgow target (Cote d'Ivoire, Gambia); in re-running the models, I find the same result (M3). Second, I drop 7 countries based on their Cook's distance (the prior three, plus Cambodia, Mali, Niger, and Pakistan); in re-running the models, I find the same result, which suggests that outlying observations are not driving the results (M4).
- M5 Countries choose to index their mitigation targets to measures of GHG emissions including land-use, land-use change, and forestry (LULUCF) or excluding these emissions. LULUCF emissions can be highly idiosyncratic year-on-year, as they can be influenced by deforestation shocks, such as wildfires and land clearing. Countries often prefer to use LULUCF accounting because forestry can act as a carbon sink and provide low-cost carbon dioxide removals that aid national inventories. But they are also more difficult to measure than emissions from other sources, such as electricity generation or transportation, and therefore many analyses exclude them. As a robustness test, I swap all countries' mitigation targets to be based on measures that exclude LULUCF emissions; in re-running the models, I find the same result.

	(1)	(2)	(3)	(4)	(5)
IOsParis	5.67**		2.57*	2.09*	3.17**
	(1.25)		(1.21)	(1.01)	(1.11)
TradeParis	0.06		0.17	0.13	0.01
	(0.30)		(0.33)	(0.28)	(0.30)
IOsParis (don't impute partner's target)		7.99**			
		(2.42)			
TradeParis (don't impute partner's target)		0.24			
		(0.54)			
Paris target		0.72**	0.81**	0.76**	
		(0.08)	(0.07)	(0.06)	
Paris target (impute)	0.58**				
	(0.06)				
Paris target (emissions excluding LULUCF)					0.81**
					(0.06)
(Intercept)	151.69**	99.48**	81.64*	70.13*	99.07**
	(36.88)	(31.91)	(34.46)	(28.77)	(31.60)
Observations	192	112	109	105	106
R^2	0.45	0.53	0.66	0.67	0.77

Table SM-5: Regressions with different measurement considerations. Outcome variable is emissions change in 2021 NDC as a percentage of 2010 emissions levels, re-scaled so that positive values are emissions cuts. OLS regression models with standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01

SM-2.2 Climate laws

One consideration with measuring the strength of peers' climate policy by their Paris mitigation targets is that, while the targets provide an indication of countries' directions and paces on mitigation, targets may be disconnected from enacted policies. As a robustness test, I rebuild the spatial weights swapping measures of climate targets for the number of climate laws a country has passed, using the Grantham Institute's climate laws database. I use two measures: (1) a straight count of national climate laws, and (2) a count of national climate laws passed between 2016 and 2019. In moving from targets to laws, some comparability across countries is lost. While targets can be standardized across countries, the impact of individual laws is more varied. Some laws enact economy-wide mitigation requirements, but others are more targeted on individual sectors. As such, laws provide a proxy for policymaking effort, but remain several steps removed from the underlying concept.

In table SM-6, I show six models using these different measures of peers' climate policy. I find that IO-weighted measures remain positive and statistically significant. Trade-weighted measures are not statistically significant.

	(1)	(2)	(3)	(4)	(5)	(6)
TradeLaws (count)	3.63+				−0.12	
	(1.93)				(2.25)	
TradeLaws (post-Paris)		5.14				−0.88
		(4.41)				(4.49)
IOLaws (count)			25.95**		26.22**	
			(7.25)		(8.80)	
IOLaws (post-Paris)				72.83**		74.28**
				(18.78)		(20.26)
Paris target	0.76**	0.77**	0.70**	0.69**	0.70**	0.69**
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
(Intercept)	−62.63+	−28.98	−284.81**	−243.29**	−285.67**	−243.37**
	(32.89)	(24.27)	(79.39)	(62.55)	(81.34)	(62.83)
Observations	112	112	112	112	112	112
R^2	0.47	0.46	0.51	0.52	0.51	0.52

Table SM-6: Regressions with climate laws as measure of peers' climate policy. Outcome variable is emissions change in 2021 NDC as a percentage of 2010 emissions levels, re-scaled so that positive values are emissions cuts. OLS regression models with standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01

SM-2.3 Spatial matrices

Trade flows. The models in the main text use row-standardized spatial matrices for dyadic trade ties and joint IO memberships. Concretely, the row-standardized measure divides the value of dyadic trade by a country's total trade: $(Imports_{i \leftarrow j} + Exports_{i \rightarrow j})/(Imports_i + Exports_i)$. This means that a country's trade to each of its partners is converted to a share of that country's total trade, and each country's total trade shares add to 1. This also implies that each country is equally exposed to international trade, since all countries' total trade exposures are the same. However, we know that countries are differentially integrated into global trade, with some countries trading high volumes and others trading lower volumes relative to the size of their economy. When governments set climate targets, if they take into account their trade partners' climate targets *and* they respond differently when they are more integrated into the global economy, then the row-standardized spatial matrix will misrepresent the data generating process.

I now investigate an alternative construction of the spatial weight for trade, where trade ties are normalized by dividing dyadic trade by a country's economic size rather than standardizing across countries as dyadic trade shares. Concretely, cells of the connectivity matrix are GDP-normalized by taking dyadic trade and dividing dyadic imports and exports by GDP: $(Imports_{i \leftarrow j} + Exports_{i \rightarrow j})/GDP_i$. With this measure, countries that trade large volumes relative to the size of their economy are more exposed to international trade and receive a larger spatial stimulus from their trade partners' climate targets. The Pearson correlation coefficient between the row-standardized and the GDP-normalized spatial weight for countries in the analysis is $r = 0.84$. I also multiply the GDP-normalized spatial weight by 1000 to bring it onto a similar scale as the row-standardized measure. I construct an analogous measure for EITE trade only.

In table SM-7, models 1 and 2 reproduce the results from the main text (table 1, models 1 and 2), which use a row-standardized spatial matrix for trade ties. In models 3 and 4, I use the GDP-normalized spatial matrix instead, and I recover the same positive and statistically significant coefficient. Models 5 and 6 find the same results with EITE trade. An interaction model can help adjudicate which of the row-standardized or GDP-normalized measures better characterizes climate target-setting. Interacting the row-standardized spatial weight with trade openness allows the effect of trade partners' climate targets to vary based on a country's level of trade openness because trade openness sums national imports and exports as a share of GDP. This is analogous to the GDP-normalized measure, but explicitly models whether the effect of trade partners' targets depends on levels of trade openness. In models 7 and 8, I interact the row-standardized spatial weight with a country's log-transformed trade openness, which was a control variable in model 2. I find that the coefficient on the interaction term is not statistically significant and the main effects are also no longer significant. That the interaction is not significant suggests that the effects of trade partners' climate targets does not differ by levels of trade openness. This lends support for relying on the row-standardized measure, where all states receive equivalent impulses from their trade partners, regardless of their levels of trade.

Figure SM-1 examines common support between trade openness and the row-standardized spatial weight for trade and finds common support across terciles of trade openness, with the exception of low trade openness and high values of the spatial weight. In the right panel, I show the fitted values and 95% confidence intervals across the range of the spatial weight for trade for a relatively closed country (where trade is roughly 54% of GDP; the 25th percentile of trade openness) and a relatively open country (where trade is roughly 103% of GDP; the 75th percentile). Here, there is no difference in the effect of the spatial weight at low and high values of trade openness.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TradeParis (row-standardized)	1.01**	0.89*					4.94+	5.12+
	(0.31)	(0.36)					(2.81)	(2.85)
TradeParis (normalized by GDP)			1.16**	1.06**				
			(0.33)	(0.37)				
Trade$^{\text{EITE}}$Paris (normalized by GDP)					6.32**	5.61*		
					(2.06)	(2.29)		
TradeParis (row-standardized):Trade openness							−0.90	−0.96
							(0.64)	(0.65)
Trade openness		2.39		9.16		8.52	−9.17	−8.95
		(9.92)		(9.25)		(9.37)	(12.12)	(12.44)
Paris target	0.71**	0.70**	0.73**	0.71**	0.71**	0.69**	0.72**	0.71**
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
(Intercept)	9.58	6.05	7.49	−24.11	5.93	−22.78	52.39	60.20
	(5.82)	(48.23)	(5.35)	(43.58)	(5.33)	(44.38)	(55.54)	(60.10)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	112	112	112	112	112	112	112	112
R^2	0.50	0.51	0.51	0.52	0.50	0.51	0.51	0.52

Table SM-7: Regressions with dyadic trade flows normalized by GDP levels. Outcome variable is emissions change in 2021 NDC as a percentage of 2010 emissions levels, re-scaled so that positive values are emissions cuts. OLS regression models with standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

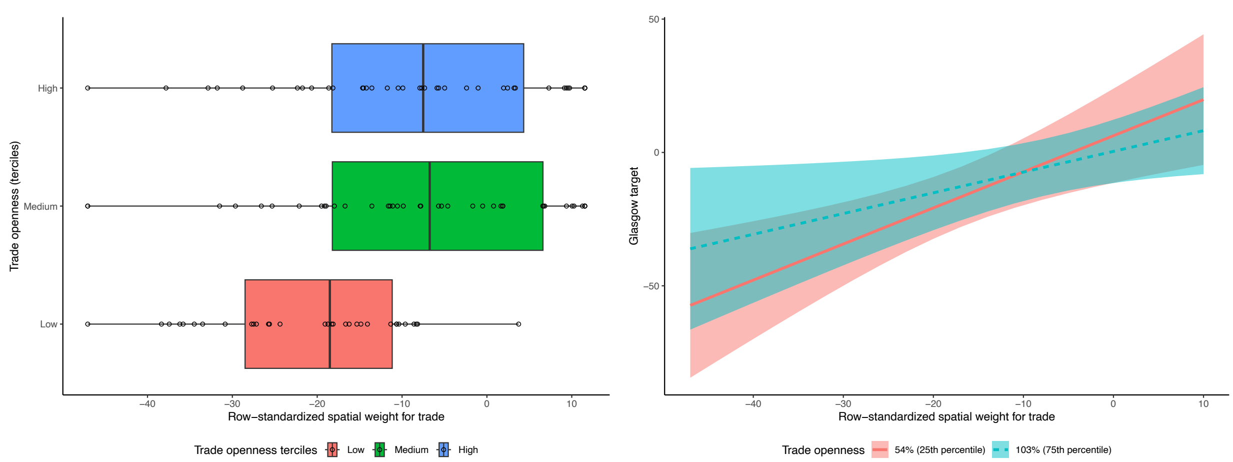

Figure SM-1: Multiplicative interaction model for row-standardized spatial weight for trade and trade openness (trade as a share of GDP). The left panel investigates common support for the spatial weight across terciles of trade openness. The right panel shows fitted values from M7.

IO memberships. The same consideration could arise with the spatial matrix for joint IO memberships, which is row-standardized in the main text. I now create a non–row-standardized matrix of joint IO ties and use this to re-create the IO-weighted measure of peers' climate targets. With this measure, states receive a larger spatial stimulus when they have more total IO memberships. The Pearson correlation coefficient between the row-standardized and the non–row-standardized spatial weight for countries in the analysis is only $r = 0.22$. I also divide the spatial weight by 1000 to place it on a similar scale as the prior measure.

Table SM-2 begins by reproducing the baseline models from table 2. Then in models 3 and 4, I use the non–row-standardized measure of IO ties. The coefficient on IO ties is not statistically significant in either model. I investigate this further by interacting the row-standardized spatial weight and the count of IO memberships in models 5 and 6. Once again, this allows the effect of IO-weighted peers' climate targets to vary at different levels of countries' total IO membership. We see a positive main effect for the spatial weight and a negative interaction effect in both models.

In figure SM-2, I investigate common support and show fitted values for model 5. First, the data lack common support, as there are no countries in the lowest tercile of IO membership whose IO-weighted peers have stronger climate targets than about -29%. Therefore, we should be attentive to not make inferences outside the range of common support. Second, the fitted values show that countries set stronger targets when their IO-weighted peers set stronger targets, but that the marginal effect of these peers' targets wanes as states have more IO memberships. The interaction effect is negative, implying that IO-weighted peers' climate targets have diminishing effects on the ratchet process as countries gain more total IO memberships. The fitted values in figure SM-2 suggest that the effect of IO ties flattens out at high counts of IO memberships rather than reversing and turning sharply negative. The effect is still positive at the 75th percentile of total IO membership and reaches zero around the 93rd percentile of membership. Combined with the positive main effect of the IO-weighted targets and the positive effect for countries in the lower tercile, there remains some empirical support for the ratchet process diffusing through shared IO memberships, though the effect appears to be diluted when states have many IO memberships.

	(1)	(2)	(3)	(4)	(5)	(6)
IOsParis (row-standardized)	4.27**	4.25**			12.33**	11.72*
	(1.11)	(1.39)			(4.39)	(4.61)
IOsParis (non–row-standardized)			0.03	−0.03		
			(0.13)	(0.15)		
IOsParis (row-standardized):IO memberships					−0.08*	−0.08*
					(0.04)	(0.04)
IO memberships					−2.34+	−2.25+
					(1.19)	(1.23)
Paris target	0.70**	0.70**	0.78**	0.73**	0.73**	0.72**
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
(Intercept)	127.68**	135.59+	5.99	−56.47	359.19*	324.37+
	(33.93)	(73.27)	(31.59)	(72.14)	(137.76)	(169.93)
Controls	No	Yes	No	Yes	No	Yes
Observations	112	112	112	112	112	112
R2	0.52	0.52	0.46	0.48	0.54	0.54

Table SM-8: Regressions with alternative spatial weights for joint IO memberships. Outcome variable is emissions change in 2021 NDC as a percentage of 2010 emissions levels, re-scaled so that positive values are emissions cuts. OLS regression models with standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

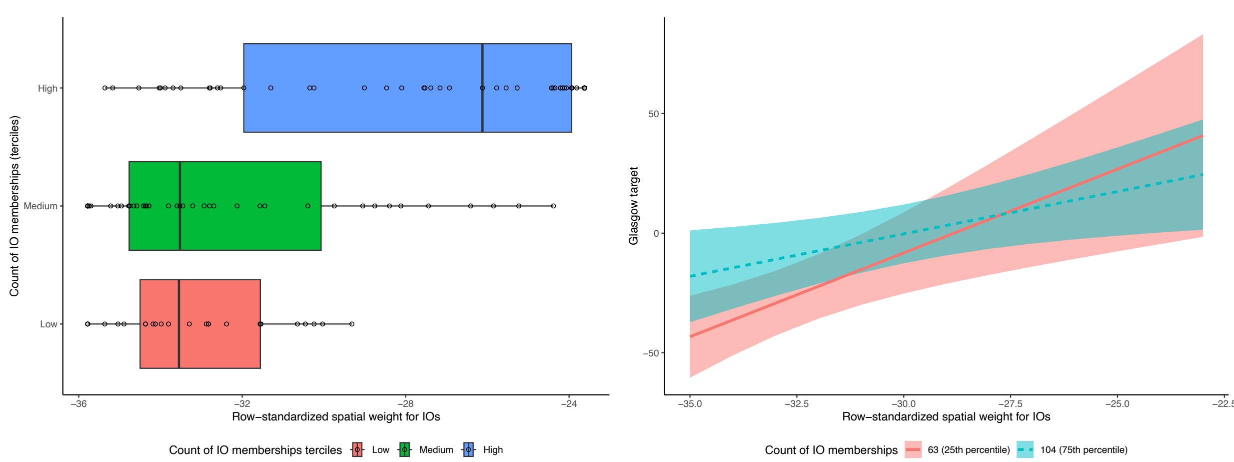

Figure SM-2: Multiplicative interaction model for row-standardized spatial weight of IO peers' climate targets and states' counts of IO memberships. The left panel investigates common support for the spatial weight across terciles of trade openness. The right panel shows fitted values from M5.

SM-3 Estimation

SM-3.1 Unobserved regional heterogeneity

Figure 3 suggests that ratcheting behavior may cluster geographically — where some world regions have ratcheted more on average, while others have ratcheted less. The spatial terms already account for regional variation to the extent that countries trade more with their geographic neighbors and share more international organizational memberships with their neighbors through regional IOs. Nonetheless, there may be further unobserved heterogeneity across regions and I now include a set of regional fixed effects to account for this. I take the World Bank's regional classifications and add these to the main regression models; East Asia and the Pacific is the reference category.

I report these results in table SM-9, where even numbered models control for trade openness, industry's share of GDP, renewable electricity generation, and fossil fuel rents. In models 1 and 2, we see that the **Trade**Paris term is no longer statistically significant when controlling for regional heterogeneity. The **IOs**Paris term remains statistically significant in models 3 and 4. Finally, in models 5 and 6, with the spatial terms for trade and IOs, neither spatial term is statistically significant at conventional thresholds. The p-values for **Trade**Paris are very large, while those for **IOs**Paris are 0.025, 0.044, 0.054, and 0.066 across models 3–6, respectively. Nonetheless, the coefficients on **IOs**Paris are very stable across models, which suggests that omitting the regional intercepts does not bias the estimates. The standard errors are larger in models 5 and 6, which make the estimates less precise. The only remaining variation in models 5 and 6 for the **IOs**Paris term to explain is within-region IO ties beyond what can be accounted for with existing trade ties *and* the lagged Paris climate target in a single cross-section. Introducing these additional region terms adds little to the model fit, as shown by the small increase in the R^2 statistic between table 2 and table SM-9. This is intuitive given that the lagged climate target already accounts for most of the underlying variation in levels across observations. As a result, these models may be over-specified, which can be inefficient and inflate standard errors.

	(1)	(2)	(3)	(4)	(5)	(6)
TradeParis (row-standardized)	0.41	0.34			−0.08	−0.14
	(0.36)	(0.39)			(0.44)	(0.47)
IOsParis (row-standardized)			4.71*	5.03*	5.00+	5.52+
			(2.07)	(2.47)	(2.56)	(2.97)
Europe and Central Asia	33.99*	35.91*	15.62	15.53	15.24	14.80
	(16.37)	(17.61)	(18.60)	(20.53)	(18.79)	(20.77)
Latin America and Caribbean	44.57*	46.00*	54.94**	57.39**	55.97**	59.53**
	(18.59)	(19.88)	(18.38)	(19.61)	(19.25)	(20.94)
Middle East and North Africa	33.45	36.08	37.34+	36.56	37.70+	37.11
	(22.63)	(24.05)	(22.25)	(23.58)	(22.43)	(23.76)
North America	44.05	44.03	32.21	33.13	32.31	33.98
	(36.92)	(38.66)	(36.75)	(38.29)	(36.92)	(38.57)
South Asia	−8.66	−6.98	−3.20	0.66	−3.65	0.31
	(25.93)	(26.72)	(25.48)	(26.53)	(25.71)	(26.68)
Sub-Saharan Africa	−0.12	1.71	13.73	14.62	14.23	15.81
	(16.60)	(17.85)	(17.67)	(18.69)	(17.95)	(19.20)
Paris target	0.71**	0.70**	0.70**	0.69**	0.70**	0.70**
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
(Intercept)	−18.91	−20.65	121.55+	126.65	129.16+	135.44
	(14.97)	(54.37)	(65.93)	(94.81)	(77.32)	(99.64)
Controls	No	Yes	No	Yes	No	Yes
Observations	112	112	112	112	112	112
R^2	0.56	0.56	0.58	0.58	0.58	0.58

Table SM-9: Alternative specification of main models with regional intercepts added; reference region is “East Asia and the Pacific”. Outcome variable is emissions change in 2021 NDC as a percentage of 2010 emissions levels, re-scaled so that positive values are emissions cuts. OLS regression models with standard errors in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

SM-3.2 Independence of trade and IO pathways

The models in text present an independent relationship between trade-weighted peers' and IO-weighted peers' climate targets — where each has an additive effect on the ratchet that can be estimated after partialling out the other's influence. However, it could also be the case that the targets of trade-weighted and IO-weighted peers interact — either through a complementary process where strong pre-existing IO ties support trade ties or a substitution process where strong IO ties can make up for weak trade ties, and vice versa. I model this as a multiplicative interaction effect in table SM-10. Models 1 and 2 reproduce the main results from 2. Models 3 and 4 add the interaction, and the interaction term is not statistically significant.

Figure SM-3 investigates common support across the interaction. Since the spatial terms for trade and IOs are positively correlated, there are no cases with high levels of IO-weighted peers' climate targets but low values of trade-weighted peers' climate targets, which restricts which inferences can be drawn from this data. In plotting the fitted values, we see that IO-weighted peers with strong climate targets can substitute for trade-weighted peers with weak targets, while the effect of IO-weighted peers on climate targets is attenuated when countries' trade partners have strong targets. Similarly, when a country's IO-weighted peers have relatively weak targets their trade-weighted peers' targets can support the ratchet when the latter are strong; but when IO-weighted peers have strong targets, trade-weighted peers' targets do not effect the ratchet. The interactions suggest that trade and IO relationships can substitute for each other when one is low, but the coefficients remain imprecisely estimated and their relationship lacks common support across the full range of the interaction.

	(1)	(2)	(3)	(4)
IOsParis (row-standardized)	3.26*	3.34*	2.55+	2.62
	(1.49)	(1.66)	(1.51)	(1.69)
TradeParis (row-standardized)	0.41	0.43	−4.84+	−4.64+
	(0.41)	(0.43)	(2.70)	(2.78)
TradeParis (row-standardized):**IO**Paris (row-standardized)			−0.17+	−0.16+
			(0.08)	(0.09)
Paris target	0.69**	0.69**	0.73**	0.72**
	(0.08)	(0.08)	(0.08)	(0.08)
(Intercept)	101.77*	121.79	86.10*	103.84
	(42.46)	(74.55)	(42.65)	(74.35)
Controls	No	Yes	No	Yes
Observations	112	112	112	112
R^2	0.53	0.53	0.54	0.54

Table SM-10: Regressions with alternative spatial weights for joint IO memberships. Outcome variable is emissions change in 2021 NDC as a percentage of 2010 emissions levels, re-scaled so that positive values are emissions cuts. OLS regression models with standard errors in parentheses. + p < 0.1, * p < 0.05, ** p < 0.01

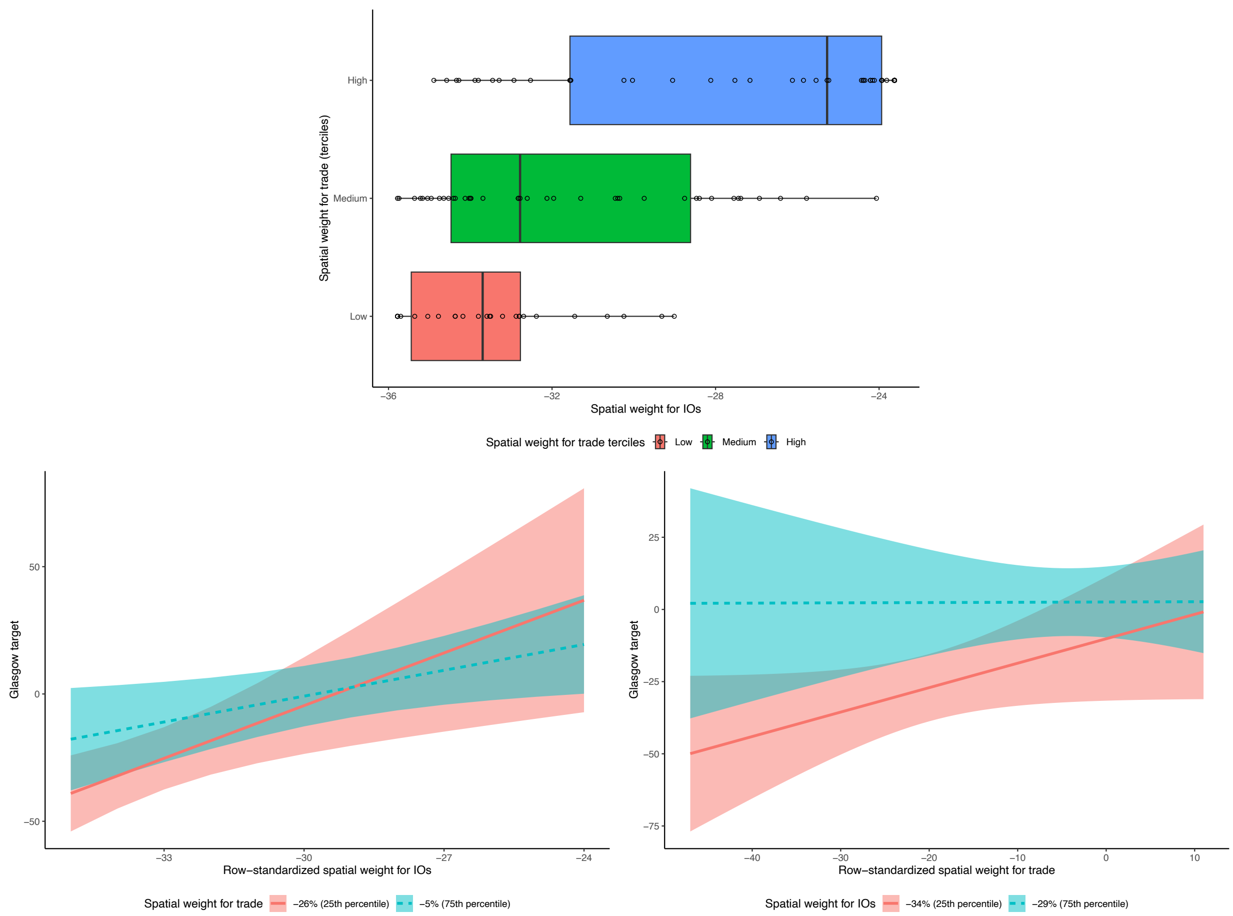

Figure SM-3: Models with a multiplicative interaction between trade-weighted peers' climate targets and IO-weighted peers' climate targets. Top panel investigates common support across levels of the interacted variables. Bottom-left panel shows fitted values across levels of IO-weighted peers' climate targets for a country whose trade-weighted peers have weak targets (red) and strong ones (blue); bottom-right panel is vice versa.

SM-3.3 Sensitivity analysis

The research design relies on no confounding to estimate the average treatment effect of peers' targets on the ratcheted targets. I have argued that the strongest source of confounding in this design are states' previous climate targets, where the Glasgow targets strongly reflect each state's pre-existing Paris target. Controlling for other observed covariates helps strengthen the argument of no confounding, but we can also assess the credibility of this assumption more rigorously using sensitivity analysis. Sensitivity analysis is a tool for understanding how strong an unobserved confounder would need to be to substantially change an observed estimate. It can be used to place bounds on an estimate that quantify how much unobserved confounding there would need to be to reduce an estimated effect size to zero.

Cinelli and Hazlett (2020) present a "robustness value" that quantifies how much of the residual variance in both the treatment and the outcome would need to be explained by a confounder to bring the estimated effect to zero.[68] The robustness value for table 2, model 3 is 0.19, implying that unobserved confounders would need to explain more than 19% of the unexplained variance in both the Glasgow targets and the climate policy of IO-weighted peers to reduce θ to zero.

I analyze the influence of a confounding variable as strong as each state's Paris target to consider the sensitivity of the main finding. The Paris targets are highly correlated with the Glasgow targets ($r = 0.68$) and also correlated with IO peers' climate policy ($r = 0.26$). It is difficult to imagine another confounder that would be more associated with the Glasgow targets than these lagged targets.

The sensitivity analysis finds that even a confounder as strong as the Paris targets that explained all of the residual variation in the Glasgow targets and that was as strongly associated with IO partners' targets would not be strong enough to reduce the effect of IO partners' climate targets to zero. The bound on $R^2_{D\sim Z|\mathbf{X}} = 0.0122$ is below the robustness value and below the partial R^2 of the treatment with the outcome $R^2_{Y\sim D|\mathbf{X}} = 0.0426$. The bound on $R^2_{Y\sim Z|\mathbf{X},D} = 0.71$ is above the robustness value; however, the bounds on each $R^2_{D\sim Z|\mathbf{X}}$ and $R^2_{Y\sim Z|\mathbf{X},D}$ would need to be greater than the robustness value to drive the estimate to zero. Note, as well, that the bound of $R^2_{D\sim Z|\mathbf{X}}$ is below

[68] Sensitivity analysis conducted in R using "sensemakr". Cinelli, Carlos and Chad Hazlett (2020). "Making sense of sensitivity: Extending omitted variable bias." *Journal of the Royal Statistical Society Series B — Statistical Methodology* 82.1, pp. 39-67.

the robustness value for the $\alpha = 0.05$ significance level ($R^2_{D\sim Z|\mathbf{X}} = 0.0122$; $RV_{q=1,\alpha=0.05} = 0.019$, which implies that this hypothetical confounder would also keep p-value below 0.05.

Outcome: *Glasgow target*						
Treatment	Estimate	Std. error	t-statistic	$R^2_{Y\sim D\|\mathbf{X}}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.05}$
IOParis	3.264	1.489	2.191	4.3%	19%	1.9%
df = 108	Bound: (1 × *Paris target*): $R^2_{Y\sim Z\|\mathbf{X},D} = 71\%$, $R^2_{D\sim Z\|\mathbf{X}} = 1.2\%$					

Table SM-11: Sensitivity analysis for table 2, model 3.

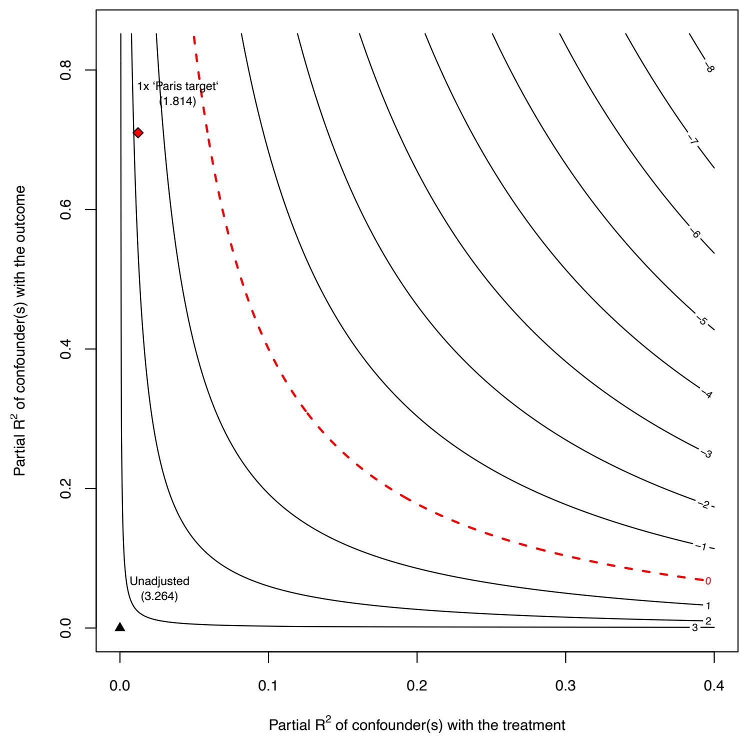

Figure SM-4: Sensitivity analysis reveals that unobserved confounding would need to explain 21–24% of the unexplained variance in both the Glasgow targets and the trade-weighted climate policy of peers to reduce the estimate for θ to zero. The bounds for a confounder as strongly correlated with these variables as the Paris targets measure would not be sufficient to confound the results.

SM-3.4 Randomization inference

There is some concern that p-values may be incorrect in small samples, especially when the main variables are not normally distributed, so I conduct randomization inference and find normally distributed estimates of θ, and calculate $p = 0.003$, smaller than the analytical $p = 0.0306$ in table 2, model 3.

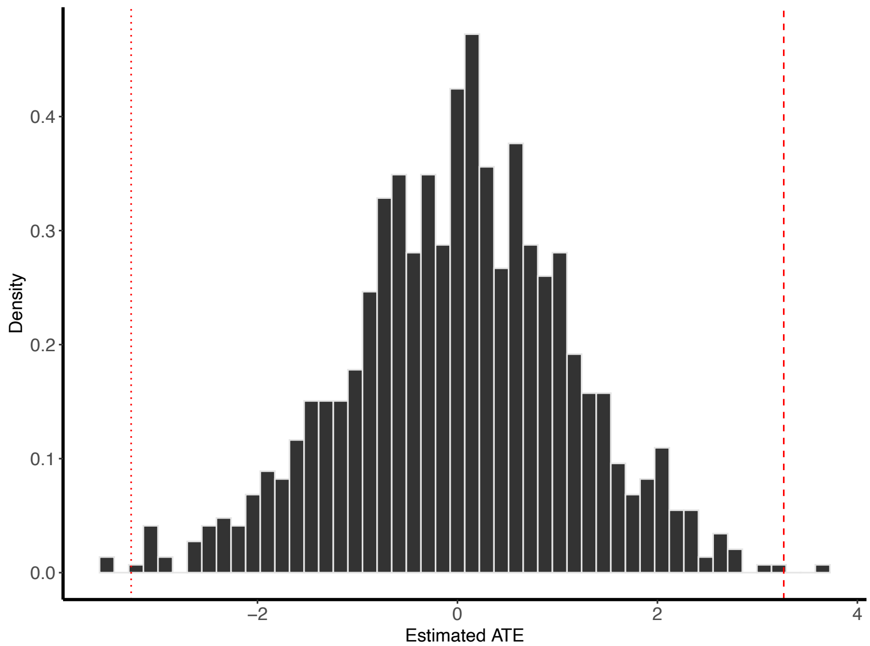

Figure SM-5: Randomization inference. Estimates of the average treatment effect in 1,000 permutations of **IO**Paris for table 2, model 3. Vertical lines indicate model ATE, $\theta = 3.26$ and the two-sided cutoff, -3.26. In 1,000 permutations, only 3 estimates were larger than $|\theta = 3.26|$, implying $p = 3/1000 = 0.003$.

SM-4 Who leads target-setting?

Table SM-12 presents results for the timing of climate target submissions. The outcome variable is a categorical indicator that measures when countries submitted their updated NDCs relative to the submissions of the EU and the US. The updated NDCs were "due" in March 2020, but very few states submitted these on time—even before the 2020 coronavirus pandemic completely halted daily activities. The slow progress in submitting NDCs on time was concerning for observers, and the UN climate secretariat eventually changed the updated NDC deadline to the end of December 2020; however, most countries missed even this second deadline. In 2020, the US was led by the Trump Administration that was in the process of formally withdrawing from the Paris Agreement. The combination of the pandemic and the American government's hostility to the Paris process led to only 6 NDCs being submitted by April 1, and only 33 NDCs throughout 2020, excluding EU states. In September 2020, the European Commission announced a new EU climate mitigation goal, and deposited its updated NDC in December. This seemed to signal a restart in multilateral climate politics, ahead of Joe Biden's presidential victory in November 2020. American participation in international climate treaties has hardly been assured historically, despite the Biden Administration's stated intentions and swift actions to rejoin the Paris Agreement.

As table SM-12 shows, the most likely countries to submit updated NDCs following the EU's announcement were countries with the most trade ties to the European Union. The EU was the first major emitter to submit their updated NDC, even if theirs was submitted after the initial due date. The Biden Administration finally presented its updated climate target at a widely-publicized virtual climate summit on Earth Day in April 2021, and pressured other countries to submit updated targets. However, as table SM-12 shows, while countries did submit new targets (as demonstrated by the positive coefficient on the intercept term), these countries were not necessarily more linked to the US by trade. One possible explanation for the lack of US pull on NDCs is the persistent domestic credibility problem in American climate policy. The Biden Administration hoped to have a major piece of domestic legislation finished by COP26 in Glasgow in November 2021, but opposition from key veto players in the Democratic Party stymied progress. If governments set climate policy conditional on the behavior of their closest trading partners, then trade ties to the EU seem to be the strongest determinant of updated NDCs.

	(1)					
	Early submission Before Sep. 2020		Post-EU, pre-US Oct. 2020–Mar. 2021		Post-US After Apr. 2021	
Term	Estimate	Std. error	Estimate	Std. error	Estimate	Std. error
(Intercept)	-0.471	0.503	-2.952*	1.000	1.004*	0.383
EU trade exposure	-0.391	1.771	4.589*	2.157	-0.474	1.320
US trade exposure	4.106	2.282	1.913	4.921	0.516	2.062

Table SM-12: Timing of updated NDC submission reflects national-level trade exposures to EU and US. Multinomial logistic regression with "Never submitted updated NDC" as the omitted reference category. $N = 161$ countries, excluding US, EU member states, and states that formally coordinate their climate policy with the EU. $*$ indicates $p < 0.05$

SM-5 Emissions growth scenarios in developing countries

Did countries that weakened their targets simply set more realistic reference levels? One consideration may be that countries that weakened their targets did so because they had unrealistic GHG emissions projections in their first NDCs and were able to update with more accurate projections in their second NDCs. This is only a consideration for countries that set baseline scenario targets that articulate their emissions reductions relative to a future "business as usual" scenario. If the first NDC's emissions projection was overly modest, then holding the target absolute emissions level fixed and updating the projection to be more realistic would imply a weakened target.

We can compare countries' GHG emissions growth rates that would be consistent with meeting their BAU reference level in both of their NDCs relative to their emissions growth rates up to the Paris Agreement. Looking only at non-high income countries, the average annual emissions growth from 2000–2015 was 2.35 percentage points (pp) per year. Within this set of countries, for countries that submitted stronger Glasgow targets, their Glasgow targets projected +1.69 pp, while their Paris targets projected +3.51 pp. Relative to developing countries' average pre-Paris growth rate, ratcheting countries projected higher BAU emissions growth in their Paris targets and then amended this to project lower BAU emissions growth in their Glasgow targets. Some of their enhanced ambition, therefore, could reflect revisions to emissions growth estimates that are more in keeping with historical averages.

For developing countries that submitted weaker Glasgow targets, their Glasgow targets projected +4.24 pp annual growth, while their Paris targets projected +2.09 pp. Relative to developing countries' average pre-Paris growth rate, these countries projected BAU emissions growth near the historical average in their Paris targets, but then revised their estimates to project much stronger BAU growth in their Glasgow targets than historical averages. Some of their observed reduced ambition, therefore, could reflect keeping percentage targets constant while updating BAU forecasts to project more future growth. Yet, this future growth is nearly double historical norms, which seems unrealistic. These targets are inflated revised growth forecasts that mask low levels of mitigation effort, rather than simply more realistic revised growth forecasts.