On-Line Appendix:

This appendix provides a series of robustness checks designed to support the results presented in our research note. These statistical robustness checks are presented together in Table 3, and the discussion in this appendix follows the sequence of models in that table.

Measuring US AfT on a Per Capita Basis

In model 3.1, we simply re-estimated our base model (model 1.1 in Table 1), but with *USAfT* measured on a per capita basis (i.e. the natural log of US aid for trade per capita lagged two years). The results show that when US aid for trade is deflated by recipient population, its coefficient remains positively signed and statistically significant. Although this result is certainly favorable to our hypothesis (*US AfT has been associated with export growth in recipient national economies*), we have some concerns about what the *USAfT* coefficient in model 3.1 is actually measuring since it picks up on both numerator (US aid for trade) and denominator (recipient population) effects with regard to recipient exports. Our argument most directly concerns the former and not the latter, and it is hard to disentangle one effect from the other.

Indeed, there is reason to believe that the denominator effect tends to work against the numerator effect, raising the risk of a Type II error. To understand why this is true, consider the very likely possibility that a developing country's exports come from its comparative advantage in labor, which implies that a larger population should be positively associated with exports at least for most of the countries in our statistical sample. This relationship means that the inverse of population (i.e. 1/population) should be negatively correlated with exports, thus working against the positive export effect expected for aid for trade, the variable in the numerator of an aid per capita ratio. This understanding helps explain why measuring US aid for trade on a per capita basis should

produce a statistically weaker export effect than when simply using the US aid for trade level as done in model 1.1 in Table 1.

Adding More Control Variables

In model 3.2, we add four more control variables to our base model specification. One might be concerned that the positive export effect that we attributed to US aid for trade in our base model was being driven, at least in part, by traditional foreign aid from the United States and from other Western donors. To account for this possibility, we add two more independent variables, the first measures the official development assistance from the US government (*USODA*) and the second measures the same from other Western sources (*NonUSODA*), using information from the PLAID database (Nielson et al. 2010). Both of these variables will be logged and lagged two years so that they are specified on the same terms as our *USAfT* variable.

One might also be concerned that there are certain omitted variables that potentially explain both recipient exports and their US aid for trade allocations. In this regard, two possibilities come to mind. First, certain countries are wealthy in terms of natural resources, which may lead them to have both greater exports and receive more US aid for trade. Likewise, a country in a post conflict situation might be associated with both export growth and increased amounts of US aid for trade. To the extent that these third factors are not picked up indirectly through our use of fixed effects (both cross-sectional and temporal), they could create a positive bias for the *USAfT* coefficient. Thus, we directly control for natural resource wealth by including the logged value of *EnergyProduction*, a natural resource of particular interest to the US government, and *PostConflict*, a dummy variable indicating whether five years have passed since the country/year observation experienced an armed conflict. More specifically, *EnergyProduction* measures primary energy production (petroleum, natural gas, coal, and electric power) in quadrillion British thermal units

using data from the U.S. Energy Information Administration (2011), and the *PostConflict* variable comes from PRIO (2009).

The results in model 3.2 show that the *USAfT* coefficient does not change with these additional controls (in this regard, the relevant comparison is model 1.1 in Table 1 and not model 3.1 in Table 3). Furthermore, among these additional independent variables, only *EnergyProduction* is statistically significant, suggesting that our fixed effects specification was effectively controlling for most potentially omitted third factors. The statistical *in*significance of *USODA* and *NonUSODA* also provides an interesting false specification check since official development assistance has not been typically targeted towards increasing recipient country exports, unlike aid for trade. Thus, one would not expect to observe that ODA (from the United States or other Western donors) has a similar effect in promoting exports. In this regard, both *USODA* and *NonUSODA* remain statistically insignificant even when country fixed effects are dropped from the model.

Removing the Observations with No US Aid for Trade

In model 3.3 in Table 3, we estimate our base model specification for only the sample of countries (N=722) that actually received any US aid for trade (i.e. *USAfT*>0) in order to ascertain whether our larger sample which included all potential recipient country/years was associated with any endogeneity/selection bias. The results in model 3.3 suggest that this was not the case as the *USAfT* coefficient remains positively signed and statistically significant (0.014**) even in this smaller sample. Indeed, to the extent the larger sample used in model 1.1 was associated with any bias, the slightly greater positive coefficient for USAfT in the smaller sample suggests that it would have worked *against* finding support for our hypothesis and not in favor of it.

In model 3.4 in Table 3, we directly model the selection process into this restricted sample, which contains only the country/year observations that actually received any American aid for trade.

To this end, we use two of the new variables described above: *EnergyProduction* and *PostConflict*. In a first stage selection model where the dependent variables is a dichotomous indicator for whether or not the country/year observation received any US AfT (*GetUSAfT*), both *EnergyProduction* and *PostConflict* are positively signed and statistically significant, and together they offer a strong F statistic (45.92***). We then use the predicted value from this *GetUSAfT* model to calculate the *Inverse Mills Ratio* as a selection parameter, which is included as an additional regressor in the second stage export equation. The results show once again that the *USAfT* coefficient remains positively signed and statistically significant. As further evidence that no "unmodeled" selection problem exists in our full sample base model specification, note that the *Inverse Mills Ratio* coefficient is statistically insignificant in the second stage reduced sample export model.

Dropping the Observations with Large USAfT Values

Having truncated our sample at the bottom by excluding the observations that received no American aid for trade, we can also truncate our sample at the top by excluding the observations that received large amounts of US AfT in order to ascertain whether our results might be driven by particular observations at the high end of the aid for trade distribution. Thus, in model 3.5 in Table 3, we estimate our base model specification excluding the sample of countries that received *USAfT* above the 75th percentile. The results show the aid for trade coefficient to be virtually unchanged relative to the full sample comparison model, suggesting no or little bias coming from the set of country/years that received the most American aid for trade.

Removing Fixed Effects

Having shown that our main result is robust to changes in the statistical sample, readers may also be interested in seeing how the *USAfT* coefficient changes when we estimate our model

without any fixed effects. The obvious risk in dropping these fixed effects is that our model could become mis-specified, or subject to omitted variable bias. To reduce this possibility, our model without any fixed effects adds four control variables, including GDP per capita (*GDPpc*), *Distance*, a *Landlocked* dummy, and an *Island* dummy (these latter three variables were, in fact, part of our original specification but dropped due to perfect collinearity with the country fixed effects). This equation is estimated as model 3.6 in Table 3, and the *US AfT* coefficient remains positively signed and statistically significant, but its substantive effect becomes much weaker dropping from 0.010*** with fixed effects (see model 1.1 in Table 1) to 0.003** without them.

In model 3.7 in Table 3, we re-estimate this equation as a random effects model to show another specification that does not include fixed effects, and the *US AfT* coefficient remains at 0.003**. The utility of this random effects specification is that it allows us to compare the coefficients from this model (without country fixed effects) to a model with them, using the Hausman test. This X^2 test effectively considers whether the random effects are uncorrelated with other regressors in the model, with the null hypothesis being no such correlation. To the extent that one observes a statistically significant Hausman X^2, this would be a strong indication that the random effects specification is biased and that a model with fixed effects, despite any inefficiencies, represents the preferred specification. As shown in model 3.7, the Hausman X^2 is 173.48 and statistically different from zero with greater than 99 percent confidence.

Reverse Causality?

The last three models in Table 3 are designed to test for endogeneity bias in the form of reverse causality. Despite the two year lag on *US AfT*, its coefficient may be biased by the effect of a country's export growth on its US aid for trade allocations. This should not be much of a concern for our hypothesis if the causality from recipient exports to US aid for trade runs in a negative

direction (i.e. less export growth results in greater AfT allocations) because this would mean that the positive *US AfT* coefficient in model 1.1 in Table 1 was likely understated and, thus, US aid for trade has been even more export effective than this statistical estimate would suggest. But if the causality from recipient export growth to US aid for trade runs in a positive direction (i.e. the United States allocates more aid for trade to countries already experiencing greater export growth, or to the easier cases), then our *US AfT* coefficient may be overstated, or too large in a positive direction.

To ascertain the direction and extent of endogeneity, we begin by eliminating the lag on *US AfT*, thus estimating a model where exports are regressed on US aid for trade in the same year that it was appropriated (rather than implemented). If reverse causality runs in a positive direction, then one should be able to observe that *US AfT* coefficient gets even larger in a positive direction when the two year lag is eliminated. The results in model 3.8 in Table 3 show that this is not case: with no lag, *US AfT* becomes statistically *in*significant. We interpret this as a favorable result with regards to endogeneity: there is a very low probability that the positive *US AfT* coefficient in model 1.1 was supported by a positive relationship that runs from country/year exports to its appropriation of aid for trade from the U.S. government.

In model 3.9 in Table 3, we directly estimate a reverse causality model with *US AfT* as the dependent variable and *Exports* lagged two years as the independent variable. In this model, the lagged value of country/year exports returns a negative coefficient that is statistically significant with greater than 99 percent confidence, strongly suggesting that US aid for trade policy has not been targeting the easier cases. Just the opposite appears to be the case: the US government has been allocating more aid for trade to countries that export less, consistent with our results reported in Table 2.

In model 3.10, we also examine the possibility of endogeneity using a System Generalized Method of Moments estimator (Blundell and Bond 1998), which combines a regression in

differences using lagged levels as internal instruments with a regression in levels using lagged differences as internal instruments. This System GMM specification treats both *USAfT* and *GDP* as potentially endogenous regressors since a Keynesian production function would put GDP on the left hand side of exports, and it uses only two lagged levels for the difference equation and two lagged differences for the level equation in order to reduce the instrument count. When estimated in two steps with robust standard errors, the *USAfT* coefficients grows to 0.019**, consistent with the results in model 3.9 showing that if there was any reverse causality from exports to US aid for trade in our base model (model 1.1 in Table 1), it tended to depress (not inflate) the *USAfT* coefficient.

References:

Blundell, Richard and Stephen Bond. 1998. "Initial Conditions and Moment Restrictions in Dynamic Panel Data Models." Journal of Econometrics 87 (August): 115-43.

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U.S. Energy Information Administration. 2011. "Total Primary Energy Production (Quadrillion Btu)". Available from http://www.eia.gov/cfapps/ipdbproject/IEDIndex3.cfm .

Table 3: Robustness Checks

	3.1	3.2	3.3	3.4	3.5
Dependent Variable:	ln*Exports*	ln*Exports*	ln*Exports*	ln*Exports*	ln*Exports*
Constant	7.37*** (2.14)	9.73*** (1.72)	6.72*** (1.98)	6.66*** (1.99)	9.01*** (2.27)
Lagged Dependent Variable	0.53*** (0.07)	0.48*** (0.06)	0.40*** (0.08)	0.40*** (0.08)	0.50*** (0.08)
ln*USAfT* (lagged two years)	0.060* (0.035)	0.010*** (0.003)	0.014** (0.006)	0.014** (0.006)	0.011*** (0.003)
ln*GDP*	0.12 (0.09)	0.04 (0.07)	0.28*** (0.08)	0.28*** (0.08)	0.07 (0.09)
ln*USODA* (lagged two years)		-0.002 (0.002)			
ln*NonUSODA* (lagged two years)		-0.004 (0.005)			
ln*EnergyProduction*		0.89*** (0.20)			
PostConflict		-0.03 (0.03)			
Inverse Mills Ratio from *GetUSAfT* eq.				0.07 (0.39)	
R^2	0.98	0.90	0.97	0.97	0.97
N	1180	1180	722	722	835
1st stage F				45.92***	
Note:	US AfT measured on a per capita basis	More controls	Non-zero US AfT sample	Non-zero US AfT sample with selection control	Sample excludes US AfT obs. above 75th percentile

Table 3 continued

	3.6	3.7	3.8	3.9	3.10
Dependent Variable:	ln*Exports*	ln*Exports*	ln*Exports*	ln*USAfT*	ln*Exports*
Constant	0.05 (0.14)	0.05 (0.11)	7.43*** (2.11)	29.77 (21.17)	0.65 (0.46)
Lagged Dependent Variable	0.95*** (0.02)	0.95*** (0.01)	0.53*** (0.07)	0.09* (0.05)	1.03*** (0.06)
ln*USAfT* (lagged two years)	0.003** (0.001)	0.003** (0.001)	0.001 (0.002)		0.019** (0.009)
ln*GDP*	0.04** (0.02)	0.04*** (0.01)	0.12 (0.08)	0.76 (0.87)	-0.06 (0.07)
ln*GDPpc*	0.03*** (0.01)	0.03*** (0.01)			
ln*Distance*	-0.015* (0.009)	-0.015* (0.009)			
Landlocked	0.01 (0.02)	0.01 (0.02)			
Island	-0.02 (0.02)	-0.02 (0.02)			
ln*Exports* (lagged two years)				-1.87*** (0.51)	
R^2	0.99	0.99	0.98	0.76	
N	1180	1180	1180	1180	1180
Hausman X^2		173.48***			
AR(2) test (p value)					-0.01 (0.992)
Hansen test (p value)					15.01 (0.595)
Note:	More controls, but no country or year fixed effects	Random effects model	No lag on US AfT	Reverse Causality	System GMM