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Tim Lloyd, Mark McGillivray, Oliver Morrissey and Robert Osei

Centre for Research in Economic Development and International Trade, University of Nottingham

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The Authors
Tim Lloyd, Oliver Morrissey and Robert Osei are respectively Lecturer, Senior Lecturer and Research Student in the School of Economics, University of Nottingham. Mark McGillivray is Associate Professor at RMIT University, Melbourne.

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Abstract
This paper demonstrates that an empirical link between aid and trade exist (for some donor-recipient pairs), but that the nature of this linkage is complex and can take a variety of forms. By identifying this complexity (and variability) we challenge the assertion, often made in debates regarding tied aid, that aid creates or promotes trade. The argument that aid leads to trade is usually based on anecdotal or microeconomic (project or firm-level data), but this cannot be generalised to the claim that aggregate aid flows are linked in a causal manner with aggregate trade flows. It is the latter claim we investigate. We examine data on aid and trade flows for a sample of four European donors and 26 African recipients over 1969-95. Three broad findings emerge. First, a statistical link between aid and trade, of whatever form, is the exception rather than the norm. Second, there is very little evidence that aid creates trade; this argument for tied aid is unproven on our analysis of aggregate bilateral flows. Third, France, unlike the other donors examined, does appear more likely to use trade links as a criterion in determining aid allocations.

Outline

I INTRODUCTION

Is there a link between bilateral aid and trade flows? Official aid policy statements and reports on aid appear to answer in the affirmative. Various references are made to the commercial 'return' from aid, directly by promoting donor exporters (especially tied aid which subsidises exports) or indirectly by increasing recipient growth and capacity to purchase exports (for the UK, see Morrissey *et al*, 1992; Actionaid, 1998). References are also made to linking the allocation of aid to donor commercial interests, with emphasis being given to important export markets, both current and potential. Taking the official line at face value, one could therefore be forgiven for assuming that there is indeed a causal bi-directional link between aid and trade flows: aid leads to trade and trade leads to aid. This impression is reinforced by reading much of the independent NGO literature on aid and trade (see Randal and German, 1994). It is often simply taken for granted, even by academic commentators, that such a link exists, yet there has been little research which has sought to empirically validate, in any systematic way, whether a link between aggregate aid and trade flows actually exists. This paper demonstrates that links do indeed exist but, due to the complexity of inter-linkages and difficulty inherent in empirical testing, one cannot draw simple generalisations about the form of the linkage.

This paper has a simple aim – to demonstrate that an empirical link between aid and trade may exist (for some donor-recipient pairs), but that the nature of this linkage is complex and can take a variety of forms. In fact, we demonstrate that the form of any aid-trade linkage will vary between donor-recipient pairs. We show that theoretical considerations can be used to justify a link from aid to trade, i.e. donors may use aid as a policy instrument to stimulate subsequent increased trade with the recipient. Alternatively, the link may be from trade to aid; donors will tend to grant more aid to those recipients with which they have strong established trade relations. It is quite possible that both factors may be at play, or that some third factor, such as colonial ties, determines both aid and trade relations. In such cases, one would observe the link in both directions. Indeed, there may be no empirical linkage at all; aid flows may appear unrelated to trade flows and *vice-versa*. We examine data on aid and trade flows for a sample of four European donors and 26 African recipients over 1969-95, and find evidence for all the possibilities listed above.

There are a number of important implications for empirical studies of aid. First, the claim often made by donor businesses and politicians that aid creates trade is put to the test (and found wanting in many cases). Second, studies of aid allocation are based on cross-section samples. The aim is to identify the factors determining how a donor allocates its aid budget across all recipients. A trade variable is usually included, and often found to be significant. If, however, the nature of the aid-trade relationship is different for various recipients in the sample, especially if the flows are simultaneously determined in some cases, the cross-section regression is mis-specified and the results may be unreliable. Finally, such analyses can further our understanding of the nature of donor-recipient economic relations.

There are many reasons why one might expect to observe a correlation between aid and trade flows from a donor to a particular recipient. An obvious case is tied aid, where the granting of aid is contingent on purchasing goods from the donor. More generally, aid may be given to countries, such as ex-colonies, which have strong trading ties with the donor. Alternatively, aid may engender trade dependency, where recipients purchase imports from donors granting them large amounts of aid because the aid is considered contingent on the imports. Yet another suggestion is that aid is trade creating: the aid contributes to economic growth in the recipient that generates a subsequent increase in donor exports to the recipient. Such trade creation benefits the donor, and can be a strong factor in maintaining or increasing the value of aid flows. It should be clear that the possible forms of aid-trade linkages are many and inter-related. One consequence is that empirical testing of the link is difficult.

In Section 2 we outline the various theories or explanations of why we may expect to see a relationship between aid and trade flows, and demonstrate that a number of different links are possible. Section 3 presents the existing empirical evidence on these possible linkages. Section 4 then presents our argument that one should first ‘pre-test’ the data to determine which potential linkage is most likely to prevail in a particular donor-recipient relationship. We propose Granger causality as an appropriate pre-test for aid-trade flows, and find evidence of all hypothesised linkages among donor-recipient pairs in our sample. We emphasise that we are testing causality in a purely statistical sense; our results are not to be interpreted as demonstrating that aid does *cause* trade, or *vice-versa*, in a philosophical sense. In fact, our results are to be interpreted as identifying the probable

nature of the aid-trade relationship for a donor-recipient pair, which information can be used to motivate the form of model used to study each relationship in more detail. Section V concludes and considers the directions for future research.

II POSSIBLE LINKAGES BETWEEN AID AND TRADE

The reasons why one might expect a particular link between aid and trade vary from the simple to the complex. In cases such as aid tying these links will be direct, in others they will be indirect. The fundamental requirement for a statistical link is that one flow must be greater, given the presence of the other, than it would be were the other flow absent. That is, the various impacts of aid must culminate in a higher level of donor exports to the recipient than would be the case without aid. Alternatively, the various impacts of bilateral trade (in particular donor exports) must result in a higher level of aid to the partner than would otherwise be the case. If the link is observed both from aid to trade and from trade to aid, we say it is bi-directional. The reasons why links may exist can be considered under two broad headings, those asserting a link from aid to trade and those asserting a link from trade to aid.

ARGUMENTS FOR AN AID TO TRADE RELATIONSHIP

Aid flows may induce donor exports either because of the general economic effects on the recipient, or because the aid is directly linked to trade, or because it reinforces bilateral economic and political links (or a combination of all three). However, each of these reasons linking aid to trade can operate in reverse, such that aid reduces trade.

Macroeconomic Impact of Aid

Traditional macroeconomic theories of aid impact posit that aid supplements domestic savings, leading to higher investment which contributes to higher rates of economic growth than would be the case without aid (White, 1992, provides a survey). This growth induces a greater capacity of recipient countries to absorb foreign goods and services, including those originating from donors (in line with their global competitiveness). More recently, aid is often linked to the implementation of structural economic reforms, especially the liberalisation of foreign trade regimes (Morrissey, 1995). This can have an indirect effect on donor exports, in the sense that reductions in trade barriers can increase the donors' access to markets in developing countries and/or the aid financing averts import compression. Thus there are a number of economic mechanisms through which aid

can induce donor exports by increasing recipient import capacity, notably through economic growth.

Despite the early optimism associated with aid, there is no consensus regarding its macroeconomic impact. Aid may have a negligible macroeconomic impact due to fungibility (Heller, 1975). Even if all aid is saved and invested it may crowd-out public investment and increase the price of investment goods, with the end result of lower growth than would otherwise be the case (Mosley *et al*., 1987, show that this is an empirical issue for which the evidence is mixed). A similar result can emerge if donors require counterpart funds. To raise these funds recipients must increase taxes and/or the public sector borrowing requirement, which can increase interest rates and crowd-out private sector investment (White and McGillivray, 1992).

Aid Tying

The most direct link between aid and trade is *formal tying*, where the provision of aid is dependent upon the recipient purchasing goods from the donor. In practice this usually means that aid is provided in the form of goods and services procured in the donor country, thus the aid itself is trade (donor exports). In addition to the exports directly financed, tied aid also increases recipient exposure to donor goods and services which encourages follow-on orders and expands, or at least consolidates, commercial ties; in this way aid is used as an instrument of trade policy (Morrissey, 1993a). A common variant of tied aid is mixed credits, where donors provide an export subsidy to their companies seeking contracts in developing countries (Morrissey, 1991). A less direct form of tying is *informal*, where donors direct aid towards projects, goods or countries in which its own industries have a strong competitive advantage; in practice it is difficult to distinguish resulting trade from competitive advantage. There is a related argument that aid generates political goodwill, from the recipient towards the donor, such that the recipient may feel more disposed, if not obliged, to purchase goods from the donor.

Aid tying can also retard economic growth in recipient countries and as such be counterproductive in promoting donor exports. If aid was untied, so that the recipients could choose how to spend it, they would have the opportunity to determine their own investment projects, choose the most appropriate technology and to purchase imports at world prices (Morrissey, 1993a). Empirical studies have shown that exports provided

under tied aid are overpriced, compared to prevailing world prices, by between 10 and 40 percent (Jepma, 1991). Moreover, 'the goods offered are of low priority to the recipient, are excessively capital-intensive, are highly dependent on Western technologies and are import biased' (Jepma, 1989: 10). Thus, under tied aid, recipients may experience lower growth than would otherwise be the case (as they are prevented from purchasing the most appropriate goods at the best price). There is also an argument that tying has a detrimental economic impact on donors as tied aid often supports inefficient industries (Morrissey, *et al*, 1992).

Aid-induced Trade Dependency

Even in the absence of tied aid there are ways in which aid can induce recipient dependence on donors for the supply of goods and services. For example, aid tends to fund projects that require the import of capital goods, typically produced by donors. This effect is not necessarily isolated to the life of the project; where equipment and machinery are involved, replacement parts are often only available in the original source country. Another example is food aid. It has been argued widely that food aid distorts the allocation of resources in recipient countries away from the production of food, and can exacerbate and prolong the very shortages it is intended to overcome, and can distort domestic consumption patterns (Maxwell and Singer, 1977). An outcome is prolonged dependence on donor countries not only for food aid but for food purchased on commercial terms.

ARGUMENTS FOR A TRADE TO AID RELATIONSHIP

The view that trade can lead to aid is generally attributed to effects of aid allocation policies of donors. These policies, in turn, are argued to be the result of the various pressures, exerted by domestic lobby groups, to which policy formulation is subject. Business groups and sections of the donor bureaucracy concerned with trade promotion are particularly active in this regard (Morrissey, *et al*, 1992). Trade can lead to further aid if donors give preference in the allocation of their aid to countries with which they have the greatest commercial links. With respect to the geographical allocation of aid, one would therefore expect that, *ceteris paribus*, the greater the value of donor exports to a given recipient, the greater the amount of aid it would be allocated by the donor. In these events, the donor is rewarding the recipient for purchasing its exports, or seeking to

consolidate and/or expand its market in the country through the expectation that aid will have a trade-inducing effect.

Cross section data also indicates cases where a negative relationship between aid and donor exports may be observed. A donor may well decide to pursue a more aggressive and indeed risky strategy; rather than focussing on established export markets, it could instead use aid to promote export ties in those countries which currently are lesser markets (McGillivray and Oczkowski, 1992). In this case, one would expect the geographical allocation of aid to be inversely related to export flows. Furthermore, 'life-cycle' hypotheses of aid allocation suggest trade may even be used as a indicator of the recipient's economic growth or prosperity and thus may be associated with a diminution of aid. The aid allocation literature does not provide a consensus on the impact of trade on aid flows (McGillivray and White, 1993).

HYPOTHESISED AID-TRADE LINKS

In summary, three general possibilities can be identified. The first is that trade is a determinant of aid: donors grant more aid to those recipients that import more from them. This is Case I and can be expressed as the hypothesis that 'trade causes aid' and if such (Granger) causality is found the implication is that trade flows tend to precede aid flows in time (see below for a discussion of this interpretation of causality). There is no assessment of the impact of aid on the recipient economy: Case I relates to why donors grant aid to one recipient rather than another, and says nothing about the economic merits of the aid. If we observe the reverse, that aid impacts on trade (Case II), the hypothesis is that 'aid causes trade' and the associated economic merits of aid could be positive or negative. On the one hand, the aid may engender trade dependency: aid causes trade with the donor not because the aid generates growth, but because it establishes a tie with the donor (which may in fact impair recipient growth). On the other hand, if aid contributes to growth, or relaxes a foreign exchange gap, we would expect recipient imports to increase. In this case aid creates trade through growth. When evidence for Case II is found, further testing of the relationship is required to ascertain the growth effects of aid. The implication here is that trade may be an explanatory factor in the relationship between aid and growth.

A third possibility is where evidence of both Case I and Case II is uncovered in a donor-recipient relationship (Case III). This would arise where aid and trade form parts of a mutually reinforcing cycle and would imply that the arguments relevant to Cases I and II apply simultaneously through the sample period. Reinforcement effects indicate the presence of a feedback loop between aid and trade, such that the presence of one increases the likelihood of the other. Such bi-directional causality simply means that the arguments underlying Cases I and II apply simultaneously, although neither dominates. It is also possible that no relationship exists at all, or alternatively that a third (or more) common factor is responsible for the observed temporal correlation between aid and trade. As demonstrated in the next section, the fact that many forms of relation are possible represents a problem with existing empirical work: as most studies limit attention to one (or a sub-set) of these possible cases, one cannot draw general conclusions.

III EMPIRICAL EVIDENCE ON AID AND TRADE

While there is an empirical literature on aid allocation (reviewed in McGillivray and White, 1993), and thus of the trade leading to aid hypothesis, there has been very little work on the hypothesis that aid leads to trade (thus very little on bi-directional causality). At the micro-level, there is considerable anecdotal and some empirical evidence: Morrissey *et al* (1992) review the UK literature, including reports by business groups (NERA, 1995, provides an update on the latter), while Andersson and Hellström (1994) provide a detailed study for Sweden. These studies, especially business reports, generally refer to tied aid but the evidence that tied aid leads to increased exports (which is by no means conclusive) is rarely generalisable. Whilst it may be true that particular firms benefit from aid-supported exports, this does not mean that a donor country benefits in that it exports more to a particular recipient in the presence of tied aid than it would were there no tied aid flows between the two. While the micro studies are informative, we confine attention to macro studies based on aggregate aid and trade flows. Clearly, there are problems with aggregate data, but the objective is to assess if such data offer any evidence to support the micro data; does the aid-trade link exist in the aggregate.

Econometric studies relevant to our current purposes fall into two categories. First, aid allocation studies that have attempted to identify those factors significantly influencing the geographical allocation of aid (and include trade as one of these factors). Second, studies that have specifically tested for bi-directional aid-trade links. We address each in turn.

Econometric studies of aid allocation typically estimate, using cross section data, regression models of the form:

$$A_{ij} = \alpha_0 + \alpha_1 D_i + \alpha_2 P_i + \alpha_3 C_i + \mu_i \qquad (1)$$

where A_{ij} is donor j's aid to country i, D is a vector of variables representing i's developmental requirements (e.g. humanitarian need, absorptive capacity), P is a vector of variables representing i's political and strategic importance to the donor (e.g. ex-colony, geographical location), and C is a vector of variables representing i's commercial and economic importance to the donor (e.g. trade and investment ties); α_0 is a constant, the other α's are vectors of parameters, and μ is an error term. The level (or sometimes share) of donor exports to country i is often included as an element of the vector C.

Results from a selection of some 15 studies from the aid allocation literature are shown in Table 1; these include aid and trade flows for ten individual donors and the EC as a whole. Only results relating to the link between aid and trade are shown (column 3). As can be seen, each of these studies, with the exception of Bowles (1989), finds some evidence of trade leading to aid (that is, these two variables were found to be significantly correlated after controlling for the influence on aid of other relevant variables). In some cases this is without exception (Levitt, 1968; McGillivray and Oczkowski, 1991; Tsoutsoplides, 1991; Gounder, 1994b), while in others only partial evidence is found (e.g. Wittkopf, 1972; Maizels and Nissanke, 1984). Three studies find evidence of a negative link between aid and trade (McKinlay and Little, 1978a; Bowles, 1987; McGillivray and Oczkowski, 1992).

TABLE 1 Aid Follows Trade: Results from the Aid Allocation Literature

Study	Donor	Aid follows trade?	Period	Recipient(s)
Levitt (1968)	United States	Yes (grants)	1963	Cross Section of LDCs
		Yes (loans)	1963	
Wittkopf (1972)	France	Yes	1964	Cross Section of LDCS
		No	1967	
	Germany	No	1961	
		No	1964	
		No	1967	
	United Kingdom	Yes	1964	
		Yes	1967	
	United States	Yes	1961	
		No	1964	
		No	1967	
Dudley & Montmarquette (1976)	Belgium	Yes	1970	Cross Section of LDCs
	Canada	No		
	France	Yes		
	Germany	Yes		
	Italy	Yes		
	Switzerland	No		
	United Kingdom	Yes		
	United States	Yes		
McKinlay & Little (1978a)	France	No	1968	Cross Section of LDCs
		Yes	1969-70	
		Yes (negative)	1967	Cross Section of former colonies
		Yes	1964-66	
			1968-70	
McKinlay & Little (1978b)	United Kingdom	Yes (negative)	1960	Cross Section of LDCs
		Yes	1961-70	
McKinlay & Little (1979)	United States	Yes	1962, 1970	Cross Section of LDCs
		No	1960-61, 1963-69	

Table 1 continued	**Donor**	**Aid follows trade?**	**Period**	**Recipient(s)**
Maizels & Nissanke (1984)	France	Yes	1969-70	Cross Section of LDCs
		Yes	1978-80	(excluding former colonies)
	Germany	No	1969-70	Cross Section of LDCs
		No	1978-80	
	Japan	Yes	1969-70	Cross Section of LDCs
		Yes	1978-80	
	United Kingdom	Yes	1969-70	Cross Section of LDCs
		Yes	1978-80	
	United States	Yes	1969-70	Cross Section of LDCs
		No	1978-80	
Bowles (1987)	United Kingdom	Yes (negative)	1970-81	Cross Section of LDCs
Bowles (1989)	EC (Bilateral)	No	1975-81	Cross Section of LDCs
McGillivray & Oczkowski (1991)	Australia	Yes	1980-86	Cross Section of LDCs
Tsoutsoplides (1991)	EC (Bilateral)	Yes	1975-80	Cross Section of LDCs
	EC (Multilateral)	Yes		
Grilli & Reiss (1992)	EC (Bilateral)	Yes	1971, 1980, 1988	Cross Section of Yaoundé &
	EC (Multilateral)	Yes	1971	Lomé Convention (ACP)
		No	1980, 1988	LDCs
McGillivray & Oczkowski (1992)	United Kingdom	Yes (negative)	1980, 1982, 1983	Cross Section of LDCs
		Yes	1986, 1987	
		No	1981, 1984, 1985	
Gounder (1994a)	Australia	Yes	1988, 1990	Cross Section of LDCs
		No	1987, 1989, 1991	
Gounder (1994b)	Australia	Yes	1988, 1989, 1990, 1991	Cross Section of SE Asian and South Pacific LDCs

Whilst aid allocation studies provide statistical evidence of a correlation between aid and trade, there are important caveats (McGillivray and White, 1992, provide a comprehensive review). The majority of studies test only for a contemporaneous relationship, which makes interpretation of results somewhat difficult. It may be the case that the correlation is not due to the aid allocation decisions of donors, but to the effects that aid has on trade (the direction of causality is therefore ambiguous). There are also data limitations: in using cross section data, it is implicitly assumed that trade has the same impact on aid for all countries included in the sample, i.e. the estimated coefficient on the trade variable is restricted to being the same for all recipients in the sample. This would seem an heroic assumption. Finally, and most obviously, the results refer only to correlation so inferences regarding causality cannot be drawn.

The alternative proposition that aid influences trade can be expressed in a general relationship of the form $X_{ij} = f(A_{ij}, M_i)$ where: X_{ij} are exports from donor j to recipient i, A_{ij} is aid from donor j to country i and M_i is total imports of i which is used as a measure of trade potential (other things remaining equal, j would export more to those countries that in general import more). It may be believed that countries will trade most with those countries with which they have historic links. However, if exports are related to historic links it is likely that so too will aid be related to historic links, and one would need to test alternative ways of incorporating an historic dummy into an expression of the relationship. Following this approach, and using pair-wise data for France, Germany and the UK with a sample of 36 African countries over the period 1969-92 (data for the full period was not available for all pairs), Morrissey (1993b) estimated:

$$\Delta X_{ij} = \beta_0 + \beta_1 \Delta M_i + \beta_2 \Delta A_{ij} + \varepsilon \qquad (2)$$

where

Δ indicates change in the variable (year-on-year difference),

ε a stochastic error term, with standard properties.

A dummy for historic links was excluded on the argument that historic links affect (starting) levels but need not affect year-on-year changes. This was a simplistic formulation, hence results are not reported but they are indicative (see Cnossen *et al*, 1999). In general the change in donor exports followed the trend in total recipient

imports. For both France and Germany exports were tending to increase, but only for France was there a suggestion that aid contributed to the increase in exports. For the UK, while there was evidence that aid contributed to increased exports, the trend was of declining exports to African countries. The evidence suggests that France used aid to maintain and increase its exports to Africa, the UK used aid to offset the decline in its exports, while Germany did not need to use aid to increase its export performance (the results of Nilsson, 1997, are consistent with this).

Nilsson (1997) adopted a very different approach and introduced aid flows into a gravity trade model to test for an aid-trade link between each EU donor and all recipients of EU aid (the sample of recipients thus varied from year to year). The basis of the gravity model is that exports from (donor) *j* to (recipient) *i* are determined by the size of the two countries (absolute and per capita GNP, as measures of potential supply and demand of the respective trade partners) and variables supporting or hindering trade between the two countries. The latter included the physical distance between them, bilateral aid flows from *j* to *i*, multilateral (EU) aid to *i* and dummies to pick up historic links. The study attempted to allow for the degree of tying by including a dummy for those EU donors which, on average, tied more than half of their bilateral aid (but this dummy was not found to be significant).

Nilsson (1997) found that, on average, a $10 increase in EU bilateral aid is associated with a $26 increase in EU exports, which suggests that aid is trade-creating (but could reflect co-movement of the two variables with trade having much the greater order of magnitude). The results are strongly suggestive of significant links between bilateral aid and donor exports for Belgium, France, Germany, Italy and the UK; only for France was there evidence of the effect of aid on exports increasing over time, while only for the UK was there evidence of this effect decreasing. The approach, like Morrissey (1993b), does not allow one to draw inferences about the direction of causality. Furthermore, all of the studies mentioned here use cross-section or pooled (time series for a cross-section) data. If the nature of the aid-trade links differ for donor-recipient pairs within the sample, the results may be misleading. Consequently, we propose ‘pre-testing’ for the nature of the link, and explain how Granger causality serves this purpose.

IV CAUSALITY IN AID AND TRADE FLOWS

In this section we test for Granger-causality (Granger, 1969), a widely used, if not *the* accepted, notion of causation in econometrics. Granger-causality is based on the principles that 'cause' is temporally prior to 'effect' and that the causal series contains information about the series being caused that is unavailable from any other source. In other words, a variable x_t Granger-causes another y_t if prediction of the current value of y_t is improved by knowledge of the past values of x_t and that the information is unique to x_t. While the twin principles of temporal precedence and uniqueness would seem reasonable to any concept of statistical causality, a lively and enduring debate has developed around this seemingly simple, yet apparently vexatious, issue (see *inter alia*, Sims 1972, Zellner 1979, Geweke 1982, Granger 1988, Stock and Watson 1989, Banergee *et al*. 1993, Hamilton 1994). Much of the debate has focussed upon the difficulties encountered in the practical implementation and interpretation of causality tests using aggregate time series data, particularly when the tests are conducted in a bi-variate context, as is so often the case. Here we merely highlight the issues that are of general practical significance.

First, as it is rarely the case that all possible information is available, causality statements are conditional upon some partial information set. If the information set contained *all* information at time *t*, then if temporal precedence could be established, x_{t-1} could be said to Granger-cause y_t. Where, as is usual, there is less than complete information more circumspection is warranted; other variables outside the set may be responsible for the observed correlation between x and y. Granger (1988) adopts the phrase *prima facie* Granger-causality in recognition of this, although such overt caution has yet to catch on in applied work.

Second, periodicity of the data has important implications for the results and interpretation of causality tests. Specifically, when the duration of observation exceeds the decision lag between cause and effect 'bi-directional' causality is not an unlikely outcome. Similarly, delays in recording events may confound causality inference, as in the thunder and lightning analogy. In short, some care needs to be taken that observance of an event is synchronous with its occurrence. This is of particular relevance to aid-trade

flows. Aid flows observed in year *t* are the result of decisions (on donor allocation) generally made in year *t-1* or earlier, conditioned on information available then (which does not include trade flows in year *t*). Similarly, if aid creates trade, current values of trade should be related to past values of aid. Consequently, we use lagged values of the relevant variables, as is usual in Granger-causality (see below).

However, tied aid complicates matters. The (recipient) decision on trade follows the (donor) decision on aid, but the observations of the flows will in all probability be in the same time period. Hence in some circumstances aid and trade flows in year *t* may appear to be synchronous. As a result, we also incorporate contemporaneous values of the potentially causal variable in the testing equations. Caveats notwithstanding, evidence of such 'instantaneous causation' lends support to the formal tying hypothesis.

Third, as in common with all regression, inference depends on the time series properties of the data, in particular the order of integration and presence of cointegration between variables. Importantly, if standard critical values are to be appropriate all variables should be stationary and if cointegrated these restrictions should be explicitly incorporated in to the testing framework. Not only is causality inference impaired if cointegration relations are ignored but Granger-causality (in at least one direction) automatically follows under cointegration (Engle and Granger, 1987).

So, wary of these caveats we investigate causation in aid-trade relationship by estimation of:

$$\Delta A_t = \alpha_0 + \sum_{i=1}^{m} \alpha_{1,i} \Delta A_{t-i} + \sum_{i=0}^{m} \alpha_{2,i} \Delta X_{t-i} + \delta ECM_{t-1} + \mu_t \qquad (3)$$

$$\Delta X_t = \beta_0 + \sum_{i=1}^{m} \beta_{1,i} \Delta X_{t-i} + \sum_{i=0}^{m} \beta_{2,i} \Delta A_{t-i} + \gamma ECM_{t-1} + \varepsilon_t \qquad (4)$$

where A_t is a donor's aid to a particular recipient in period *t*, X_t is exports (from the donor) to that country in *t*, both of which are assumed to be integrated of order one, and μ_t, ε_t are normally and independently distributed with constant means and diagonal

variance-covariance matrix. If cointegrated, each regression is augmented by the term ECM_{t-1} representing lagged residuals from the cointegrating regression that embodies the restrictions that cointegration implies.[1] If the variables are cointegrated, then a causal relationship exists *de facto*. Where aid and trade are not cointegrated, and *a priori* this is what one would expect, ECM_{t-1} does not appear in the testing equations. Trade is said to Granger-cause aid if one or more $\alpha_{2,i}$ $(i = 1,...m)$ are found to be significantly different from zero. Aid Granger-causes trade if one or more $\beta_{2,i}$ $(i = 1,...m)$ are significantly different from zero. Bi-directional causality, or feedback, is said to occur if at least one of each $\alpha_{2,i}$ and $\beta_{2,i}$ $(i = 1,...m)$ are significantly different from zero. Should $\alpha_{2,0}$ (or equivalently, $\beta_{2,0}$)[2] be statistically significant, then there is said to be instantaneous causation between the two, possibly signifying formal tying of aid and trade.

We test for causality in the aid-trade relation using data on exports and gross aid disbursements from France, Germany, Netherlands and the UK to a sample of 26 African countries over 1969-95. The results for each donor are summarised in Tables 2-5 (detailed econometric results are presented in the Appendix). Two general points should be noted. First, for each donor, we only tested for linkages if the time series for aid and trade had at least 22 observations. If a recipient is omitted from the donor sample, it is because there were too few observations; only Germany had a long established aid relationship with all 26 recipients. Second, the gross nature of the disbursements data may have implications for causality inference since aid to highly indebted countries may simply be used to service external debt. In cases where this occurs causal relationships are unlikely to be detected.

Before considering the results for each donor in turn, we again reiterate that this is intended as a 'pre-test' to identify broad categories that best characterise the donor-

1 The presence of cointegration also allows a further distinction to be identified, that of short and long run Granger-causality (See Granger and Lin 1995). Whilst we do not make the distinction excplicitly in the text the causality tests in the cointegrated cases also include the coefficient on the ECM term in addition to the lagged differenced terms.

2 One implies the other. This becomes clear from a comparison of the ordinary least squares estimators of the 't' ratios on the coefficients of the direct and reverse regressions.

recipient relationship. One must be careful in drawing inferences. This is especially so in a bi-variate testing framework since causality will be incorrectly attributed where there is a third variable that is common to both aid and trade. In all cases where causality is detected, third variable explanations are possible. Factors such as historical and cultural links, or a common language, would be expected to impact on the *level* rather than the *change* in aid and trade (the data used in (3) and (4)) and thus are unlikely to account for such 'spurious' causality in the aid-trade relationship as tested here. Other factors for which this does not apply could exert important effects.

Evidence of contemporaneous causality may indicate that the observation period is longer than the decision period. This would be consistent with tied aid, where the aid *is* exports and the flows are in the same year, if not simultaneous. The annual nature of the data may also mask underlying yet unobservable uni-directional (or bi-directional) causality in cases where the causal effect (and feedback) occurs completely within the observation. These data problems do not alter the validity of any actual findings of causality based on lagged (or dynamic) effects. Consequently, we look for the following results:

Case I: if we find evidence of Case I *only*. The inference is that *causality is from trade to aid*. A finding of contemporaneous causality in addition does not alter the inference (on the dynamic link); it suggests tied aid as a possibility.

Case II: if we find evidence of Case II *only*. The inference is that *causality is from aid to trade*. A finding of contemporaneous causality in addition does not alter the inference (on the dynamic link) but suggests tied aid as a possibility.

Case III: if we find evidence of both Case I *and* Case II we have *bi-directional causality* or feedback between the two. A finding of contemporaneous causality in addition does not alter the inference (on the dynamic link); again, it suggests tied aid as a possibility.

Case IV: If the *only* finding is of contemporaneous causality, this is indicative of formal tied aid where the aid come in the form of exports from the donor.

Case V: This is where there is no evidence for causality of any sort.

The overall results indicate that of the 87 donor recipient pairings, trade Granger-caused aid in 14% of the pairs, aid Granger-caused trade in 13% of the pairs and bi-directional causality was found in 8% of the pairs. Contemporaneous causality, a potential indicator of tied aid, was found in 24% of cases (but in only 8% of pairs was this Case IV). Overall, evidence for a possible link between aid and trade is present in just under half of the donor recipient pairs in the sample. Although the proportion of statistically significant aid-trade relationships was broadly the same across the four donor countries some country-specific effects are apparent. Evidence of a relationship is most common for France (55%), least common for Germany (38%) and almost 50% for the UK and Netherlands. For most of the donors no one direction of causality predominates, with the exception of France where trade causing aid was by far the most common relationship found. As expected, cointegration between aid and trade is the exception rather than the rule, although it is more common in France than the other donors in the sample.

TABLE 2 Results for France

	COINT	Case I	Case II	Bi-directional	CONTEMP
Algeria			Yes		
Burkina Faso		Yes			Yes
Burundi	Yes	Yes*			Yes
Cameroon	NR				
C.A.R	NR				
Chad	Yes	Yes			Yes
Congo	Yes	Yes			
Cote d'Ivoire	NR				
Egypt	Yes	Yes			
Gabon	NR				
Gambia	-----				
Ghana			Yes		
Kenya	-----				
Madagascar	Yes	Yes			
Malawi	-----				
Mali	NR				
Mauritania	NR				
Morocco	NR				
Niger	NR				
Nigeria	-----				
Rwanda		Yes			Yes (IV)
Senegal			Yes*		
Sierra Leone	-----				
Togo	NR				
Tunisia					Yes
Zambia	-----				
TOTAL (20)	5	7	3		5

Notes: In donor recipient pairs where a statistically significant result is found it is denoted by 'Yes' in the relevant column (* indicates the relationship appeared to be negative). Figures in TOTAL row are number of 'Yes' results (number in parentheses is sample size).

'COINT' refers to whether the variables were cointegrated,

'CONTEMP' is whether the contemporaneous value was significant (IV indicates evidence for contemporaneous causality only, Case IV).

NR no significant relationship found.

---- recipient excluded from sample due to insufficient observations.

TABLE 3 Results for Germany

	COINT	Case I	Case II	Bi-directional	CONTEMP
Algeria	NR				
Burkina Faso			Yes		Yes
Burundi	NR				
Cameroon	Yes	Yes	Yes	Yes	
C.A.R	NR				
Chad	NR				
Congo	NR				
Cote d'Ivoire	NR				
Egypt	NR				
Gabon	NR				
Gambia		Yes			
Ghana		Yes	Yes	Yes	
Kenya	NR				
Madagascar	NR				
Malawi					Yes (IV)
Mali					Yes (IV)
Mauritania	NR				
Morocco	NR				
Niger			Yes		Yes
Nigeria	Yes	Yes			Yes
Rwanda	NR				
Senegal	NR				
Sierra Leone	NR				
Togo					Yes
Tunisia	Yes	Yes	Yes	Yes	Yes (IV)
Zambia					
TOTAL (26)	3	5	5	3	7

Notes: As for Table 2.

TABLE 4 Results for the Netherlands

	COINT	Case I	Case II	Bi-directional	CONTEMP
Algeria	-----				
Burkina Faso			Yes		
Burundi		Yes*			
Cameroon	NR				
C.A.R	-----				
Chad	NR				
Congo	-----				
Cote d'Ivoire	NR				
Egypt	NR				
Gabon	-----				
Gambia	NR				
Ghana		Yes*			Yes
Kenya					Yes (IV)
Madagascar	NR				
Malawi			Yes		Yes
Mali	Yes	Yes*			Yes
Mauritania	-----				
Morocco	NR				
Niger					Yes (IV)
Nigeria					Yes (IV)
Rwanda	NR				
Senegal	NR				
Sierra Leone			Yes		
Togo	NR				
Tunisia			Yes		
Zambia	NR				
TOTAL (21)	1	3	4		6

Notes: As for Table 2.

TABLE 5 Results for the United Kingdom

	COINT	Case I	Case II	Bi-directional	CONTEMP
Algeria		Yes*			
Burkina Faso	NR				
Burundi	-----	-----			
Cameroon		Yes			
C.A.R	-----				
Chad	NR				
Congo	-----				
Cote d'Ivoire	NR				
Egypt	Yes	Yes	Yes	Yes	Yes
Gabon	-----				
Gambia	NR				
Ghana		Yes	Yes	Yes	Yes
Kenya		Yes			
Madagascar		Yes	Yes	Yes	Yes
Malawi		Yes	Yes	Yes	
Mali	NR				
Mauritania	-----				
Morocco			Yes*		
Niger	NR				
Nigeria	NR				
Rwanda	-----				
Senegal	NR				
Sierra Leone	NR				
Togo	NR				
Tunisia	NR				
Zambia			Yes		
TOTAL (20)	1	7	6	4	3

Notes: As for Table 2.

The country-specific results are as follows. Twenty of the African recipients were included in the sample for France, and significant relationships were found in eleven of these (Table 2). Evidence of unidirectional causality was found in ten cases, seven for Case I and three for Case II. There were no instances of bi-directional causality. For France, the series are cointegrated for five pairs, all of which are Case I. This suggests that France's allocation of aid to these countries is influenced by the trade flows. Burkina Faso, Burundi, Chad and Madagascar are ex-colonies of France; Congo and Rwanda were under Belgian influence (but could be considered within the Francophone sphere), and France always had an influence in Egypt. There is evidence that trade flows have followed aid (Case II) in Algeria, Ghana and Senegal. Contemporaneous causality is present in five of the recipients, all of which are Francophone. These coincided with the trade-causing-aid cases so we cannot discount the possibility of a tied aid effect.

The evidence is more mixed for Germany (Table 3), with five instances of Case I, five of Case II and three of these being bi-directional. In only three cases are the variables cointegrated; for Tunisia and Cameroon it appears bi-directional, while for Nigeria it appears that trade causes aid. There are two cases where aid causes trade only, and two where trade causes aid only. There are seven cases of contemporaneous causality, where tied aid may be a factor. As Germany is both a major donor and exporter to Africa, such mixed findings are not surprising; the possibility of informal tying is high (as Germany tends to be competitive in the products imported by African countries). There is no convincing evidence that Germany allocates aid according to trade criteria nor that it uses aid as an instrument of trade policy; significant results are found for less than 40 per cent of the sample (10/26).

The pattern is similar for the Netherlands (Table 4). In the one instance where the variables are cointegrated (Mali), it appears that trade causes aid. This would also be the conclusion for Burundi and Ghana. In all three of these cases, however, the relationship appears to be negative. If trade was increasing (decreasing), this could reflect growth (decline) so aid was decreased (increased). On the other hand, aid appears to cause trade in four countries - Burkina Faso, Malawi, Sierra Leone and Tunisia. Like France, there were no examples of bi-directional causality although contemporaneous causality occurs in six cases. As with Germany, one could not conclude that there is any consistent aid-trade relation between the

Netherlands and African aid recipients; significant results are found for ten (48%) of the 21 countries in the sample.

Like Germany, the majority of causality cases in the British pairings are bi-directional, a characteristic which is indicative of a feedback loop between aid and trade, signalling interplay between the spheres of aid disbursement and trade flows. Interestingly, there are no examples of bi-directional causality in either the French or Dutch pairings. Evidence of bi-directional causality is present for Egypt, Ghana, Madagascar and Malawi; third factors, tied aid or data measurement problems could apply (two of the countries are ex-colonies, Egypt had colonial ties, and the other is a minor partner for aid and trade). On balance, it appears that trade causes aid for Algeria, Cameroon and Kenya (the former are both Francophone, the latter a major partner), whereas aid causes trade for Morocco and Zambia. The results for the UK also indicate a lower incidence of contemporaneous causality compared to the other donors in the sample. This may be due to fewer formal ties in aid policy.

An overall summary is provided in Table 6. The only general conclusion is that France, unlike the other donors examined, is more likely to allocate aid according to trade considerations; there was evidence for Case I in seven out of twenty recipients, almost as many instances of Case I as for the other three donors combined. Combining the samples for all donors (87 pairs), there were 15 findings for Case I (17 per cent). In five of these cases the finding tended to be negative, for such cases more aid seems to be granted even though the recipients are importing less. This is especially true for the Netherlands. Such findings are also present in some aid allocation studies (Table 1), and are consistent with aid being granted primarily for developmental needs (i.e. to poorer performing countries). It could be argued that donors increase aid to countries with which trade is declining in order to boost future trade. The findings suggest such a strategy is of limited effectiveness (otherwise one should find that aid causes trade).

TABLE 6 Summary of Findings on Aid-Trade Linkages

Donor	Case I	Case II	Bi-directional
France	Burundi*	Algeria	
	Chad	Ghana	
	Congo	Senegal*	
	Egypt		
	Madagascar		
	Burkina Faso		
	Rwanda		
Germany	Nigeria	Burkina Faso	Cameroon
	Gambia	Niger	Tunisia
			Ghana
Netherlands	Mali*****	Burkina Faso	
	Burundi*	Malawi	
	Ghana*	Sierra Leone	
		Tunisia	
UK	Algeria*	Morocco*	Egypt
	Cameroon	Zambia	Ghana
	Kenya		Madagascar
			Malawi
87 cases	**15 cases**	**11 cases**	**7 cases**

Notes: Only cases were significant results were found are listed.

* Indicates that the relationship appears to be negative.

There is less evidence for the claim that aid creates trade (which, if true, would imply a finding that aid causes trade). There were 11 findings for Case II (13 per cent). There were seven findings of bi-directional causality and 17 cases (20%) of contemporaneous causality (not given in Table 6). Such results may indicate the relevance of tied aid.

It is clear from Table 6 that results are very mixed and there are no obvious common characteristics of recipients exhibiting a particular causal finding with respect to a donor. For example, in respect of France it is not the case that evidence of causality was found only for Francophone countries. One could think of a number of measures of the donor-recipient relationship that may influence findings on causality. For example, it is possible that aid is more likely to cause trade if recipients are dependent on the donor (i.e. the donor accounts for a relatively large share of aid received by the recipient). Alternatively, trade may be more likely to cause aid if the donor accounts for a large share of recipient imports. In other words, the intensity of the relationship could influence the nature of any causal relationship.

We considered a wide range of indicators. These included donor's share of imports by and aid receipts of the recipient, the ranking of recipients in terms of the amount of aid and imports from the donor, and the trends in these indicators. The latter was intended to identify if there are notable differences between those to whom aid is falling as against recipients for which aid receipts from the donor are rising. As it transpired, none of the indicators helped explain why some recipients appeared under a finding for one type of causality and other recipients appeared under a different finding. It is, however, our intention to pursue this issue further in panel data regressions.

V CONCLUSION

The literature on aid policy, and especially on donor motives for aid, abounds with assertions regarding actual (but unproven) and potential reasons as to why aid and trade flows between donors and specific recipients may be linked; this alone validates our attempt to assess the empirical basis for such assertions. The arguments were set out in Section 2, which identified three interesting cases as aid causes trade, trade causes aid, or both (bi-directional causality). The empirical evidence reviewed here offers some evidence in support of all cases, but further detailed analysis would be required to yield

any reliable conclusions (in particular regarding the magnitude of the links). Our evidence suggests that there is indeed a relationship between aid and trade, but that the specific nature of this relationship can vary between donor-recipient pairs. On account of this variability, we argue that empirical studies of aid that use trade flows (imports from donors) as an explanatory variable should pre-test the data to determine the nature of the aid-trade links for donor-recipient pairs in their sample. We propose Granger causality as an appropriate technique for such pre-testing.

Three broad findings emerge from our analysis. First, a statistical link between aid and trade, of whatever form, is not uncommon: indeed it occurs in almost half of the donor-recipient pairs. Consequently, the tying of aid and trade, (a potential but not the sole reason for the relationship) may be a common phenomenon. Second, by far the most common form of evidence found is of contemporaneous causation (detected in a quarter of the sample pairs). This is indicative of a formal tie between aid and trade, although this is not the only explanation. Certainly, the empirical evidence that aid creates trade in a dynamic sense is somewhat weaker, since aid is a Granger-cause of trade in only 14% of cases. Such a dynamic effect would have to be observed to claim that aid creates trade. The claim to this effect often heard from business and politicians arguing for tied aid is thus largely unproven from our analysis of aggregate aid and trade flows between European donors and Africa.

Third, France, unlike the other donors examined, does appear more likely to use trade links as a criterion in determining aid allocations, although in general the evidence for trade causing aid is no more common than aid causing trade. This does not imply that France uses her aid budget more strategically than the other donors. Not only is evidence of trade causing aid equally common in the UK data, but evidence of aid causing trade may itself signal strategic (tying) behaviour. What is different about the French results is that there appear to be far fewer instances of aid causing trade and hence the reinforcement effects implied by bi-directional causality.

Our findings have a number of implications for empirical studies of aid. The most important are in respect of aid allocation studies, where trade with the donor is an explanatory variable. We have shown that the nature of the relationship between aid and trade can vary between recipients in the sample. The implicit assumption in cross-section

studies that the coefficient on trade is equal for all countries will be incorrect. In the case of studies using pooled data, our approach provides a test for which countries can be pooled, or which countries should exhibit fixed effects. In general, we offer a pre-test to identify which recipients in the sample should be given particular attention (such as an interactive dummy).

Our analysis is most directly relevant to the issue of whether aid creates trade. We have found no general evidence in support of this claim, but have found instances of donor-recipient pairs where it may apply. The instances where aid caused trade appear to be random, in that there is no obvious characteristic of the recipients or donors that helps to explain a particular finding. We believe that our findings can be interpreted as evidence against the claim that aid creates trade, and thus of evidence against one of the most persistent and politically influential arguments for tied aid. Nevertheless, the issue may warrant some further investigation. If so, our pre-test is appropriate to identifying the sample for which the relationship can be tested.

APPENDIX

Table A1. STARTING DATES FOR THE AID DATA

	FRANCE	GERMANY	NETHERLANDS	UK
ALGERIA	1969	1969	******	1970
B'FASO	1969	1969	1970	1972
BURUNDI	1969	1969	1972	******
CAMEROON	1969	1969	1970	1969
CAR	1969	1969	******	******
CHAD	1969	1969	1974	1970
CONGO	1969	1969	******	******
COTE D'IVIORE	1969	1969	1970	1969
EGYPT	1969	1969	1970	1969
GABON	1969	1969	******	******
GAMBIA	******	1969	1974	1969
GHANA	1969	1969	1970	1969
KENYA	******	1969	1970	1969
MADAGASCAR	1969	1969	1971	1969
MALAWI	******	1969	1970	1969
MALI	1969	1969	1973	1972
MAURITANIA	1969	1969	******	******
MORROCCO	1969	1969	1972	1969
NIGER	1969	1969	1970	1971
NIGERIA	******	1969	1970	1969
RWANDA	1969	1969	1970	******
SENEGAL	1969	1969	1970	1969
SIERRA LEONE	******	1969	1970	1969
TOGO	1969	1969	1971	1971
TUNISIA	1969	1969	1969	1970

ZAMBIA	******	1969	1970	1969

****** Refers to cases where the series were less than 20 data points. Causality tests for those pairs

was therefore not done.

ADF AND COINTEGRATION TESTS

FRANCE			
	ADF1 (-3.612)	ADF2 (-2.997)	COINTEGRATION (-3.5805)
ALGERIA	-3.7511* -3.3566	— -7.1365**	
BURKINA FASO	-2.0277 -2.6363	-4.0703** -4.4287**	-1.9539
BURUNDI	-1.0035 -0.13297	-3.6618* -9.4580**	-3.9798
CAMEROON	-2.1182 -2.1560	-3.4672* -4.3179**	-2.2418
C.A.R.	-3.0602 -3.6954*	-5.2405** —	
CHAD	-2.2189 -1.8076	-6.1481** -3.8817**	-3.8416
CONGO	-4.5699** -1.5176	— -4.0608**	
COTE D'IVIORE	-2.2054 -2.7070	-4.4404** -4.8711**	-2.2295
EGYPT	-3.8171* -1.6478	— -6.3622**	—
GABON	-3.1711 -2.7786	-7.1922** -5.1309**	-3.3859
GAMBIA	-2.3104 -1.9614	-6.3646** -5.2190**	-2.6140
GHANA	-5.4601** -1.6206	— -5.3324**	—
KENYA	-1.6800 -3.2674	-5.2723** -7.0915**	-2.7944
MADAGASCAR	-3.1394 -2.5624	-6.7112** -5.2873**	-3.1439
MALAWI	-2.8444 -2.0922	-7.9590** -4.8069**	-3.4399
MALI	-4.1800* -3.1827	— -5.4796**	—
MAURITANIA	-2.8994 -4.4109**	-6.0217** —	—
MORROCCO	-3.9337* -1.7858	-7.3356** —	—
NIGER	-2.3724 -2.9120	-3.9074** -2.6328	-2.5108
NIGERIA	-1.8290 -2.0249	-5.7751** -3.8909**	-2.2262
RWANDA	-1.7462 -1.6168	-3.9151** -6.2152**	-3.8179
SENEGAL	-2.2924 -2.8203	-5.0518** -4.8097**	-3.5546
SIERRA LEONE	-2.5254 -3.3484	-5.5306** -3.5518*	-2.9196
TOGO	-3.6236* -1.9758	— -3.9752**	—
TUNISIA	-3.0397 -2.7272	-6.3660** -4.0660**	-3.7228

FRANCE			
	ADF1 (-3.612)	ADF2 (-2.997)	COINTEGRATION (-3.5805)
ZAMBIA	-1.0588 -4.0378*	-9.4325** —	—

ADF AND COINTEGRATION TESTS

GERMANY			
	ADF1 (-3.612)	ADF2 (-2.997)	COINTEGRATION (-3.5805)
ALGERIA	-3.1085 -2.7162	-7.8323** -3.3510*	-2.3658
BURKINA FASO	-2.6286 -4.4069**	-3.5132* —	—
BURUNDI	-2.2800 -2.1892	-4.8473** -5.5095**	-2.6010
CAMEROON	-2.7142 -3.1862	-5.4590** -6.7352**	-4.0275
C.A.R.	-2.6226 -1.6373	-5.6632** -3.7986**	-3.3048
CHAD	-1.1995 -3.1497	-6.4632** -6.4995**	-1.2493
CONGO	-4.8930** -3.6188*	— —	—
COTE D'IVIORE	-3.7571* -2.9085	— -5.0023**	—
EGYPT	-4.4638** -1.9312	— -4.9447**	
GABON	-4.6992** -4.3014*	— —	—
GAMBIA	-1.7840 -1.7701	-3.2222* -4.3648**	-2.9484
GHANA	-5.0706** -2.3046	— -6.9147**	—
KENYA	-3.1770 -2.5569	-6.3484** -5.2908**	-5.5822
MADAGASCAR	-2.6986 -2.0946	-5.0751** -4.4204**	-4.1882
MALAWI	-2.5562 -2.2474	-7.0612** -3.2928*	-4.3094
MALI	-2.8170 -1.3052	-5.8858** -6.3104**	-3.7253
MAURITANIA	-4.6100** -2.9679	— -5.2119**	—
MORROCCO	-4.9303** -1.6118	— -3.6766*	—
NIGER	-2.6621 -2.6578	-5.4868** -5.2192**	-3.1984
NIGERIA	-1.2582 -2.3358	-4.8856** -2.9325	-3.8724
RWANDA	-0.99292 -1.4116	-4.9713** -4.2935**	-2.1037
SENEGAL	-4.0344* -3.3903	— -5.9911**	—
SIERRA LEONE	-2.9335 -3.7224*	-7.3060** —	—
TOGO	-2.6155 -2.3475	-4.8446** -4.2654**	-4.5131
TUNISIA	-2.7359 -2.0008	-4.7648** -3.4826*	-5.7923

GERMANY			
	ADF1 (-3.612)	ADF2 (-2.997)	COINTEGRATION (-3.5805)
ZAMBIA	-2.3249 -3.0105	-6.0711** -6.7796**	-2.2023

ADF AND COINTEGRATION TESTS

NETHERLANDS			
	ADF1 (-3.612)	ADF2 (-2.997)	COINTEGRATION (-3.5805)
ALGERIA	-5.5009** -1.9602	— -5.6197**	—
BURKINA FASO	-1.9707 -0.82945	-4.4908** -6.6884**	-3.3935
BURUNDI	-3.0652 -3.1632	-4.3262* -5.5309**	-4.1669**
CAMEROON	-3.3693 -3.0825	-6.5700** -4.2652*	-3.6716**
C.A.R.	-2.1893 -2.6737	-4.5399** -5.1505**	-2.1389
CHAD	-3.7093 -4.3490*	-9.2640** —	—
CONGO	-4.2796* -1.6726	— -4.2469*	—
COTE D'IVIORE	-4.8720** -2.3279	— -5.3992**	—
EGYPT	-2.9646 -2.1799	-9.8419** -4.9569**	-4.1895
GABON	-4.2863* -2.8179	— -3.9162*	—
GAMBIA	-1.4810 -2.6553	-9.3158** -4.7457**	-3.3604
GHANA	-3.3859 -1.3687	-6.9431** -7.1601**	-3.2101
KENYA	-2.3142 -4.4223**	-6.8658** —	—
MADAGASCAR	-5.1590** -2.9416	— -6.7271**	—
MALAWI	-2.8931 -3.2960	-4.9562** -5.1882**	-2.9307
MALI	-2.2294 -2.3279	-5.5067** -7.0049**	-3.4957
MAURITANIA	-3.4595 -3.7278*	-8.9368** —	—
MORROCCO	-4.2726* -2.7802	— -7.1998**	—
NIGER	-3.4248 -2.9577	-5.3164** -3.7274*	-3.7056
NIGERIA	-7.1886** -2.4088	— -6.1293**	—
RWANDA	-3.9007* -2.0382	— -5.7706**	—
SENEGAL	-3.6435* -3.9342*	— —	—
SIERRA LEONE	-2.7899 -2.0038	-5.1450** -5.4720**	-3.1872
TOGO	-3.3019 -2.5548	-4.7920** -7.5996**	-4.1162

NETHERLANDS			
	ADF1 (-3.612)	ADF2 (-2.997)	COINTEGRATION (-3.5805)
TUNISIA	-2.4480 -2.3997	-5.6907** -3.9736*	-3.8009
ZAMBIA	-1.8760 -4.0550*	-5.2125** —	—

ADF AND COINTEGRATION TESTS

UK			
	ADF1 (-3.612)	ADF2 (-2.997)	COINTEGRATION (-3.5805)
ALGERIA	-2.8640 -2.3787	-5.6627** -5.0851**	-2.3725
BURKINA FASO	-4.0583* -3.0355	– -5.2471**	–
BURUNDI	-2.4463 -4.7247**	-5.8041** –	–
CAMEROON	-2.8205 -2.2921	-6.1220** -4.8281**	-3.3324
C.A.R.	-9.5232** -3.4622	– -5.7502**	–
CHAD	-6.5396** -5.6201**	– –	–
CONGO	-2.5297 -4.6700**	-4.1068** –	–
COTE D'IVIORE	-4.0595* -2.5445	– -4.8978**	–
EGYPT	-2.7542 -2.4097	-5.4976** -5.8666**	-4.0227
GABON	-23.155** -3.6375*	– –	–
GAMBIA	-3.9300* -2.3325	– -4.7893**	–
GHANA	-22.481** -3.9203*	– –	–
KENYA	-3.3351 -1.9696	-7.3590** -3.9754**	-4.1843
MADAGASCAR	-4.1757* -2.4200	– -4.9391**	–
MALAWI	-3.2726 -3.5424	-6.3773** -5.2319**	-3.8498
MALI	-3.2190 -4.5139**	-4.9646** –	–
MAURITANIA	-5.1687** -4.2390*	– –	–
MORROCCO	-4.3089* -1.4907	– -4.0721**	–
NIGER	-5.1461** -4.0105*	– –	–
NIGERIA	-3.7966* -2.0690	– -4.8080**	–
RWANDA	-3.2115 -3.4650	-4.9852** -5.5619**	-3.5161
SENEGAL	-3.9874* -1.6635	– -8.2014**	–
SIERRA LEONE	-2.8735 -2.6324	-4.2877** -4.0710**	-2.1118
TOGO	-3.6694* -1.9862	– -4.4998**	–
TUNISIA	-4.5949** -2.9983	– -5.4956**	–

UK			
	ADF1 (-3.612)	ADF2 (-2.997)	COINTEGRATION (-3.5805)
ZAMBIA	-2.3873 -2.1618	-5.2690** -5.4836**	-2.6761

ADF1 and ADF2 refer to the unit root tests for the variables in levels and in first differences respectively.

Within each cell, the first statistic is that for ‘aid’ whilst the second is for the trade series

SUMMARY RESULTS FOR UNIT ROOTS AND COINTEGRATION TESTS

	FRANCE		
	AID	IMPORTS	COINTEGRATION
ALGERIA	I(0)	I(0)	NO*
BURKINA FASO	I(1)	I(1)	NO
BURUNDI	I(1)	I(1)	YES
CAMEROON	I(1)	I(1)	NO
CAR	I(0)	I(0)	NO*
CHAD	I(1)	I(1)	YES
CONGO	I(1)	I(1)	YES
COTE D'IVIORE	I(1)	I(1)	NO
EGYPT	I(1)	I(1)	YES
GABON	I(1)	I(1)	NO
GAMBIA	----	----	----
GHANA	I(0)	I(1)	NO*
KENYA	----	----	----
MADAGASCAR	I(1)	I(1)	YES
MALAWI	----	----	----
MALI	I(0)	I(1)	NO*
MAURITANIA	I(0)	I(0)	NO*
MORROCCO	I(0)	I(1)	NO*
NIGER	I(1)	I(1)	NO
NIGERIA	----	----	----
RWANDA	I(1)	I(1)	NO
SENEGAL	I(0)	I(1)	NO*
SIERRA LEONE	----	----	----
TOGO	I(0)	I(1)	NO*
TUNISIA	I(1)	I(1)	NO
ZAMBIA	----	----	----

	FRANCE		
	AID	IMPORTS	COINTEGRATION

SUMMARY RESULTS FOR UNIT ROOTS AND COINTEGRATION TESTS

	GERMANY		
	AID	IMPORTS	COINTEGRATION
ALGERIA	I(0)	I(1)	NO*
BURKINA FASO	I(1)	I(0)	NO*
BURUNDI	I(1)	I(1)	NO
CAMEROON	I(1)	I(1)	YES
CAR	I(0)	I(1)	NO*
CHAD	I(1)	I(1)	NO
CONGO	I(0)	I(0)	NO*
COTE D'IVIORE	I(0)	I(1)	NO*
EGYPT	I(0)	I(1)	NO*
GABON	I(0)	I(0)	NO*
GAMBIA	I(0)	I(1)	NO*
GHANA	I(0)	I(1)	NO*
KENYA	I(0)	I(1)	NO*
MADAGASCAR	I(0)	I(1)	NO*
MALAWI	I(0)	I(1)	NO*
MALI	I(0)	I(1)	NO*
MAURITANIA	I(0)	I(1)	NO*
MORROCCO	I(0)	I(1)	NO*
NIGER	I(0)	I(1)	NO*
NIGERIA	I(1)	I(1)	YES
RWANDA	I(1)	I(1)	NO
SENEGAL	I(0)	I(0)	NO*
SIERRA LEONE	I(0)	I(0)	NO*
TOGO	I(0)	I(1)	NO*
TUNISIA	I(1)	I(1)	YES
ZAMBIA	I(1)	I(1)	NO

	GERMANY		
	AID	IMPORTS	COINTEGRATION

SUMMARY RESULTS FOR UNIT ROOTS AND COINTEGRATION TESTS

	NETHERLANDS		
	AID	IMPORTS	COINTEGRATION
ALGERIA	-----	-----	-----
BURKINA FASO	I(1)	I(1)	NO
BURUNDI	I(0)	I(1)	NO*
CAMEROON	I(0)	I(1)	NO*
CAR	-----	-----	-----
CHAD	I(0)	I(0)	NO*
CONGO	-----	-----	-----
COTE D'IVIORE	I(0)	I(1)	NO*
EGYPT	I(1)	I(1)	NO
GABON	-----	-----	-----
GAMBIA	I(1)	I(1)	NO
GHANA	I(0)	I(1)	NO*
KENYA	I(1)	I(0)	NO*
MADAGASCAR	I(0)	I(1)	NO*
MALAWI	I(1)	I(1)	NO
MALI	I(1)	I(1)	YES
MAURITANIA	-----	-----	-----
MORROCCO	I(0)	I(1)	NO*
NIGER	I(0)	I(1)	NO*
NIGERIA	I(0)	I(1)	NO*
RWANDA	I(0)	I(1)	NO*
SENEGAL	I(0)	I(0)	NO*

	NETHERLANDS		
	AID	IMPORTS	COINTEGRATION
SIERRA LEONE	I(1)	I(1)	NO
TOGO	I(0)	I(1)	NO*
TUNISIA	I(1)	I(1)	NO
ZAMBIA	I(1)	I(0)	NO*

SUMMARY RESULTS FOR UNIT ROOTS AND COINTEGRATION TESTS

	UK		
	AID	IMPORTS	COINTEGRATION
ALGERIA	I(1)	I(1)	NO
BURKINA FASO	I(0)	I(1)	NO*
BURUNDI	-----	-----	-----
CAMEROON	I(1)	I(1)	NO
CAR	-----	-----	-----
CHAD	I(0)	I(0)	NO*
CONGO	-----	-----	-----
COTE D'IVIORE	I(0)	I(1)	NO*
EGYPT	I(1)	I(1)	YES ?
GABON	-----	-----	-----
GAMBIA	I(0)	I(1)	NO*
GHANA	I(0)	I(0)	NO*
KENYA	I(0)	I(1)	NO*
MADAGASCAR	I(0)	I(1)	NO*
MALAWI	I(1)	I(0)	NO*
MALI	I(1)	I(0)	NO*
MAURITANIA	-----	-----	-----
MORROCCO	I(0)	I(1)	NO*
NIGER	I(0)	I(0)	NO*
NIGERIA	I(1)	I(1)	NO
RWANDA	-----	-----	-----
SENEGAL	I(0)	I(1)	NO*
SIERRA LEONE	I(1)	I(1)	NO
TOGO	I(1)	I(1)	NO
TUNISIA	I(0)	I(1)	NO*
ZAMBIA	I(1)	I(1)	NO

	UK		
	AID	IMPORTS	COINTEGRATION

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Tim Lloyd - agricultural markets, econometric modelling
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Chris Milner - trade and development
Wyn Morgan - futures markets, commodity markets
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