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INTERNATIONAL FINANCIAL REMOTENESS AND MACROECONOMIC VOLATILITY

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ABSTRACT

This paper shows that proximity to major international financial centers seems to reduce business cycle volatility. In particular, we show that countries that are further from major locations of international financial activity systematically experience more volatile growth rates in both output and consumption, even after accounting for political institutions, trade, and other controls. Our results are relatively robust in the sense that more financially remote countries are more volatile, though the results are not always statistically significant. The comparative strength of this finding is in contrast to the more ambiguous evidence found in the literature.

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1. Introduction and Motivation

This paper introduces a new stylized fact; countries that are remote from international financial activity are systematically more volatile. We interpret this fact as supportive of a joint hypothesis: 1) countries closer to major financial centers are more financially integrated; and 2) financial integration reduces macroeconomic volatility.

We motivate the first link in our joint hypothesis – that a nation's "financial remoteness" is associated with physical distance from world financial activity – based on the idea that intermediation costs increase with distance. While the cost of moving asset holdings electronically is essentially invariant to distance, a battery of empirical evidence suggests that distance exacerbates information asymmetries. Coval and Moskowitz (1999, 2001) demonstrate that fund managers in the United States invest more heavily in and earn abnormally large returns from investing in firms in close proximity, particularly from smaller firms where information asymmetries would be expected to be greater. Malloy (2005) finds that geographically proximate analysts tend to be more accurate. Petersen and Rajan (2002) find that borrower quality increases with distance, suggesting that banks are unwilling to lend to distant problem borrowers who would require more active monitoring. Berger et al (2005) find that larger banks, which are usually less intensive in the use of "soft" information in their lending decisions, lend at greater distances than small banks.[1]

The second link, the effect of financial integration on macroeconomic volatility, is ambiguous in theory. On the one hand, agents rationally respond to increased risk-sharing opportunities by raising the specialization of the production bundle (e.g. Kalemli-Ozcan, Sørensen and Yosha, 2003), leaving the national output bundle more valuable, but also more variable. On the other hand, a number of papers (e.g. Caballero and Krishnamurthy, 2001) demonstrate that poorly-developed financial sectors can exacerbate volatility, as there are fewer opportunities for firms to smooth investment shocks.

The empirical evidence is also mixed. O'Donnell (2001) finds some evidence of a positive relationship between financial openness and macroeconomic volatility for non-OECD economies, but a negative relationship among OECD countries. Buch et al (2005) find no consistent relationship. Prasad et al (2003) find higher output volatility in less financially integrated countries.

From a welfare point of view, one might be more concerned with consumption volatility. The empirical literature is again mixed. Bekaert et al (2006) find that financial liberalization is associated with reduced consumption volatility, particularly for countries with open capital accounts. Kose et al (2005) obtain mixed results: they find a negative relationship between financial liberalization and consumption volatility in their full sample, but they also find that among the more financially integrated countries, liberalizations tend to be followed by increased consumption volatility. Huizinga and Zhu (2006) find a negative relationship for non-OECD countries, but not for OECD countries. Kose et al (2003, 2007) find a negative relationship between their *de facto* proxy for financial integration and consumption smoothing among countries with poor financial market development and emerging market economies respectively. Prasad et al (2003) fail to find any measurable correlation.

One reason why these studies present weak results may be the difficulty of measuring international financial integration. *De jure* measures are usually based on the International Monetary Fund's index of capital account restrictions. While some efforts have been made to account for the magnitude and effectiveness of these restrictions (e.g. Quinn, 1997), these corrections have in practice been coarse (e.g., Edison et al, 2002). *De jure* measures are also likely to suffer from endogeneity issues, as governments might respond to macroeconomic turbulence by imposing restrictions on capital movements. *De facto* measures, such as measures of the ratio of capital flows to GDP, are also likely to have endogeneity issues, as openness may be a function of shocks that also affect volatility.

Our primary measure of international financial remoteness is the natural logarithm of the great-circle distance to the closest major financial center (London, New York, or Tokyo). We search for, and find, an effect of this measure of remoteness on volatility.[2] To check the robustness of our results, we verify our results for a number of alternative measures as well.

Our geography-based measure of financial remoteness has the advantage of plausible exogeneity. We rule out the possibility that New York, London and Tokyo have emerged as financial centers because of the superior performance of their neighbors, by conducting a robustness check in which we exclude large countries from our sample. Small countries are unlikely to influence the location of major international financial centers.

We find that the relationship between financial remoteness and volatility is robustly positive and usually statistically significant. In our default specification, a one standard deviation increase in financial remoteness (roughly equal to that between Algeria and Kiribati) results in roughly a twelve percent increase in output volatility relative to the sample mean. The significant effect of financial remoteness is reasonably insensitive to a number of checks.

We do not wish to overstate the strength and resilience of our results. Our results are not completely insensitive; for instance, dropping rich countries reduces the statistical (though not the economic) significance of the relationship. While we always find that greater remoteness is associated with more business cycle volatility, our estimates are not always significantly different from zero. This is in contrast to institutional quality, which is statistically and economically significant throughout. This makes us cautious in our interpretation. Still, our results on remoteness are stronger than the effects on volatility of other conditioning variables in our specification. Moreover, they demonstrate a stronger linkage between financial conditions and macroeconomic volatility than is typically found in the literature.

Finally, we note that the role of geography in international financial integration has already been explored in the literature on international asset trade, e.g., Martin and Rey (2004, 2006). In these models, transactions costs of exchanges of international assets exceed those for domestic assets. Financial integration is then declining in these transactions costs. When these transactions costs are posited to be increasing in physical distance, as in Portes and Rey (2005), international financial integration between two countries is decreasing in distance between them. Similarly, Rose and Spiegel (2007) introduce a model where the cost of moving assets to offshore banks is increasing in distance, and find that the share of offshore banking is decreasing in physical distance from the offshore financial center.[3,4]

2. Strategy and Methodology

The objective of our empirical work is to see if a country's geographic location "matters," and in particular to determine if countries that are further from international financial activity suffer more business cycle volatility, other things being equal. We do not use a structural theory linking the two concepts. Further, there are only imperfect measures of a number of key variables. Accordingly, our strategy is to take a reduced-form approach that encompasses existing determinants of cyclic volatility, and subject it to intense sensitivity analysis.

Our default specification is as follows:

$$Vol_{i\tau}=\beta IntFinRem_i + \gamma_1 GDP_{i\tau} + \gamma_2 Inst_{i\tau} + \gamma_3 Open_{i\tau} + \gamma_4 Govt_{i\tau} + \gamma_0 + \varepsilon_i$$

where:

- $Vol_{i\tau}$ is a measure of business cycle volatility for country i over period τ,
- $IntFinRem_i$ is a measure of international financial remoteness,
- $\{\gamma\}$ are a set of nuisance coefficients,
- GDP is a measure of country size (real GDP),
- Inst is a measure of domestic political-economy institutions,
- Open is the ratio of trade to GDP,
- Govt is the ratio of government spending to GDP, and
- ε represents other (hopefully unrelated) determinants of business cycle volatility.

The coefficient of interest to us is β, which measures the effect of international financial remoteness on business cycle volatility. A positive and significant coefficient indicates that greater international financial remoteness is associated with higher business cycle volatility, *ceteris paribus*. We estimate this cross-sectional regression with OLS, using standard errors robust to the presence of heteroskedasticity.

There are a variety of measures of business cycle volatility, none obviously superior to any other. Indeed, it is also unclear how to measure our key regressors: international financial remoteness, domestic financial depth, and institutions. Our strategy is to choose what we think of as being obvious and reasonable choices and check that our key results are robust to reasonable alternatives.

We measure business cycle volatility for country i over period τ via the standard deviation of real GDP growth (the annual first-difference of the natural logarithm of real GDP), for the eleven year period between 1994 and 2004 inclusive.[5] We also examine both longer (27-)

and shorter (5-year) periods, and pool our data across all five 11-year periods between 1950 and 2004. For further sensitivity analysis, we check both the comparable *volatility of consumption* and the *lowest* GDP growth rate during the 11-year period. Finally, we estimate our cross-sections using volatilities calculated over the entire 55 years of data available, de-trending real GDP in three different ways (deviations of growth rates from their means, and via both the Baxter-King and Hodrick-Prescott filters).

Our key regressor is international financial remoteness. As this is the novelty of the paper, the literature provides little guidance concerning its measurement.[6] We begin our analysis with a simple measure that we consider to be crude but convenient; we use the natural logarithm of the great-circle distance to the closest major financial center (London, New York, or Tokyo), and drop Japan, the UK and the US from our estimation. By this measure, Mauritius and Lesotho are the countries most remote from international financial activity (Belgium and the Netherlands are the least).

To check that our results do not depend inordinately on this precise measure, we also use three other measures of international financial remoteness (and a number of perturbations thereof). First, we use the distance from a country to the closest offshore financial center. Second, we measure the distance to countries that have large gross international stocks of international debt or assets, using the CPIS data set.[7] Third, we measure the distance to countries that have large gross capital exports on a flow basis, using IFS data.[8]

We include four additional controls to purge business cycle volatility of extraneous influences before we search for the effects of international financial remoteness. The most straightforward is real GDP, which we include simply to ensure that our results are not mechanically driven by size.

Acemoglu, Johnson, Robinson, and Thaicharoen (2003) have shown how critical political-economy institutions are in understanding volatility. For institutions, we use the popular "polity" measure from the University of Maryland's Center for International Development and Conflict Management; it ranges from -10 (strong autocracy) to +10 (strong democracy). As a check, we also use a measure of executive constraints ("xconst" from the same source), which ranges from 1 (unlimited authority) to 7 (executive parity or subordination).

We also condition for trade openness, which has been shown to have a positive effect on macroeconomic volatility in some studies [e.g. Kose et al, (2003)], but has been shown to have no measurable impact on volatility in others (Razin and Rose, 1994).[9] Finally, we condition on government expenditure as a share of GDP, which was shown by Bekaert, et al (2006) to exacerbate consumption volatility.[10]

Figure 1 contains a cross-country scatter-plot of the raw data; business cycle volatility – the dependent variable in our regression analysis – is plotted on the y-axis against international financial remoteness on the x-axis. Figure 2 is a comparable plot once the effects of the four nuisance variables have been taken out through linear regressions. Both show evidence of a positive relationship between business cycle volatility and international financial remoteness.

3. Default Specification Results

The results for our benchmark specification are in the first row of Table 1. Distance to major financial centers enters positively and significantly; financial remoteness is associated with increased output volatility. Moreover, the effect is economically important. Our coefficient point estimate indicates that a one standard deviation increase in financial remoteness would result in about a 12% increase in output volatility relative to the sample mean.

Among our other conditioning variables, the Polity2 variable enters strongly with a statistically significant effect. It is also economically large; a one standard deviation decrease in democracy (roughly a six point move for this sample), leads to a 19% decrease in output volatility relative to the sample mean. The share of GDP spent by the government enters at the 5% confidence level. The effect of real GDP is economically and statistically significant, but negative; economically large countries are more stable. The effect of trade is insignificant.

Our default specification only explains a modest amount of variation in the data, as our R-squared estimate is approximately 0.24. We do not see this as particularly troubling, given that our specification is parsimonious and includes a heterogeneous cross-section of countries.

Overall, our default specification suggests an economically and statistically significant positive relationship between financial remoteness and output volatility. Local institutions, as measured by our polity variable, also appear to have a large effect.

4. Robustness Checks

We now check that our results are reasonably insensitive to some of the many assumptions that underlie our default results. Our first checks are in the remainder of Table 1.

First, we alter the period of time (τ) over which the variables are calculated. The default period is the final (1994-2004) 11-year period; but β stays positive if either longer (27-) or shorter (5-year) periods are used, or if we use data pooled over five 11-year periods. It is typically statically significant at conventional levels (for the 27-year cross section, the coefficient is significantly different from zero at the .054 level).[11]

Our positive and significant effect of remoteness on volatility remains if we drop countries with greater than 25 million people. This is important for our maintained exogeneity assumption, as smaller countries are unlikely to have influenced which nation would emerge as the major world financial centers.

Our results are insensitive to excluding richer countries (measured as those with real GDP per capita of more than $20,000). Further, our results are insensitive to a number of other perturbations to the framework. For instance, removing outliers – defined as countries with residuals that lie more than two standard deviations from zero – only increases our key coefficient. We have added both average country population and country real GDP per capita, and our key coefficient remains statistically positive. Adding regional dummies (computed using standard World Bank groupings) also has little effect. We have also both added and changed our default measures of our control variables. Adding either the natural logarithm of a country's latitude or dummy variables for island and landlocked countries has little effect on our key result. The same is true when we measure institutions with constraint on the executive instead of polity.

Finally, we have used a different way to measure business cycle volatility. When we follow Acemoglu, Johnson, Robinson, and Thaicharoen (2003) in using the maximal drop of GDP by substituting the minimal growth rate of GDP (between 1994 and 2004) in place of the standard deviation of growth, our coefficient becomes negative and significantly so. This is consistent with our results; if remoteness raises volatility, it should make the worst year worse.[12]

We do not wish to overstate the strength and resilience of our results. While we always find that greater remoteness is associated with more business cycle volatility, our estimates are not always precisely estimated. This is in contrast to the effect of institutions on volatility, which remains negative and significant reasonably consistently. However, our results are consistently signed, and similar in magnitude across specifications. Their statistical significance is also stronger than the effects on volatility of openness or government spending. The latter variables have inconsistent and weak effects that are rarely economically or statistically significant.

5. Sensitivity Analysis

In this section, we show that reasonable variations to our methodology do not destroy our key finding, namely that remoteness raises volatility.

Our focus in this paper is the effect of international financial remoteness on business cycle volatility. Since the distance to the closest major financial centers is an imperfect measure of this remoteness, it is important to check the sensitivity of our results with respect to this key variable. Table 2 substitutes three different measures of financial remoteness into our default framework, replacing distance to the closest of the three large international financial centers (London, New York, and Tokyo). First, we use the (natural logarithm of great-circle) distance to the closest offshore financial center (OFC), using the forty OFCs tabulated in Rose and Spiegel (2007). Second, we use the distance to the (eight) countries with the largest gross stocks of foreign portfolio liabilities, measured using the CPIS data set. Alternatively, we also use the distance to the (ten) countries with the largest gross stocks of foreign portfolio assets, again using the CPIS data set.[13] These are stock measures that indicate the willingness of a country to issue to, or receive credit from foreigners. We also use the corresponding flow measures, using data from IFS. In particular, our third measure is distance to the (ten) countries with the largest capital outflows; as a check, we also use the distance to the countries with the largest capital inflows. We measure capital flows by summing flows of “direct”, “portfolio” and “other” capital flows.[14]

While we think of the distance to the *closest* countries as being most relevant, we also examine *average* distance to countries with large international financial activity in the middle panel of Table 2. Finally, in the bottom panel of Table 2, we use distance to the three major financial centers, but now weigh each of the three distances by the fraction of actual bilateral transactions between the country and the "big three." We use the CPIS data set to derive two sets of weights; the assets that are sourced from the relevant country (and hosted in Japan/UK/USA), and those that are hosted in the relevant country (from Japan/UK/USA).[15]

The results for Table 2 are similar to our benchmark results, though weaker. In particular, these different measures of financial remoteness all show a positive relationship of distance on volatility. The effect of distance to the closest country varies between .5 and .9 in size, and is typically significantly different from zero; four of the five coefficients are different from zero at the .05 level. The average distance to big international financial players also has a positive effect, but it is never significantly different from zero at conventional levels. Both of the weighted results are also positive, and the coefficient with host weights is statistically significant. Overall, we find the robustness of the results reassuring, though not overwhelming.

Table 3 is the analogue to Table 1, but uses the volatility of real consumption instead of real GDP. As discussed above, producers may respond to enhanced international risk-sharing opportunities by increasing the specialization of output, thereby increasing output volatility. However, integration also enhances the ability of consumers to hedge this increased risk; consumption volatility, which is likely to be directly relevant to welfare, may actually decrease with integration. In fact, we obtain a coefficient for consumption volatility under our default specification which is close to that for output volatility, and is statistically significant at the 1% confidence level. The sensitivity analysis in the remainder of the table indicates that this result, like that for output, is reasonably robust. For instance, our results are robust to entertaining alternate time periods. We also still obtain statistically significant results when countries over 25 million in population are omitted from our sample (albeit only at the 5% level). We no longer obtain statistically significant coefficient estimates on our variable of interest when we eliminate wealthy countries from the sample, and add either regional dummies or the log of latitude.

In summary, while theory may more strongly indicate a positive relationship between financial remoteness and consumption volatility than output volatility, our results are broadly similar for both. Since there is some sensitivity to exact model specification, we find the insensitivity to the precise concept of macroeconomic volatility reassuring.

Table 4 uses the entire sample of up to 55 years of (annual) data, instead of focusing on the last period of time. While examining the standard deviation of growth rates is a reasonable measure of business cycle volatility over an eleven-year period, de-trending over a longer period of time is more controversial. Thus we detrend real GDP in two additional ways, using both the popular Baxter-King and Hodrick-Prescott filters to extract underlying trends.[16] We then compute the standard deviation of detrended real GDP over the entire sample period, and use this as our dependent variable. We also use consumption in place of GDP. Our results are consistently correctly signed, though only one of the six coefficients is significantly different from zero at conventional levels. This is further cause for caution.

Our final set of results is in Table 5. In this table we report our benchmark equation estimated as cross-sections over different periods of time. The results for the five different eleven-year periods are in the top panel. It is interesting to note that there is no clear trend in the effect of financial remoteness on volatility, except at the very end of the sample.[17] This result is mirrored in the 5-year periods (reported at the bottom of the table). The impact of international financial remoteness might be thought to be rising over time, as technological barriers to integration seem to be falling. This topic is worth pursuing further.

We have performed a large number of robustness checks above and beyond those recorded here (a number are available in earlier versions of this paper, available on the web). For instance, we have added two size controls (population and real GDP per capita) instead of simply real GDP, we have added the ratio of domestic credit to GDP to our default specification, and we have experimented with the functional form of our default equation. None of this sensitivity analysis alters our view that the effect of financial remoteness on business cycle volatility is positive, though it is not always statistically large.

6. Conclusion

This paper uses geographic proximity as an indicator of international financial integration, and searches for its manifestations in macroeconomic volatility. We find that remoteness from financial activity, as measured by the distance to major international financial centers, increases macroeconomic volatility. We construct a number of alternative measures of both financial remoteness and volatility and demonstrate that they all appear to share this positive correlation. The size of this effect varies and is not always significant at standard levels. Still, the coefficient of interest is always positive, and is often economically large.

We do not wish to overstate the strength of our results, for a number of reasons. First, remoteness does not matter as consistently or robustly as political institutions. Second, the results are somewhat sensitive to the details of the econometrics (specification, sample, and so forth). Still, we find stronger results for our indicator of international financial integration than most previous empirical studies; the effect of remoteness seems comparable to that of openness, or government size.

While the chief purpose of this paper is to establish a stylized fact rather than to explain it, we briefly provide two thoughts. The timing of our study may be important. As demonstrated above, the strength of the relationship between financial remoteness and macroeconomic volatility appears to increase at the end of our sample. This is consistent with a growing role for international financial integration, and is consistent with weaker results for studies that rely on earlier data periods. Alternatively, our measure of financial remoteness may be a better measure of international financial integration than others, since it is more plausibly exogenous.

Finally, while we believe that the costs of intermediation increase with distance, assessing the manner in which increased costs of risk sharing affect volatility requires a more structural treatment than that which we have offered here. That is, we have only provided indirect evidence that remoteness affects volatility through its impact on integration. Thus we take a narrow interpretation of our results. While we provide evidence that geography (in the form of distance from major financial centers) matters for macroeconomic volatility, our work does not shed light on the desirability (or lack thereof) of capital flow restrictions.

There is much room for future research. One could incorporate differences in real interest rates across countries into our measure of international financial remoteness. Interest rates have the advantage of varying over time, so that a proper panel study might be possible. It would also be interesting to investigate the causes of the growing importance of financial remoteness. One possibility may be that the proliferation of non-standard financial instruments and derivatives facilitate consumption smoothing, but require greater monitoring than more conventional capital flows; this would increase the importance of geographic proximity. We leave such extensions to future work.

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Table 1: International Financial Remoteness and Business Cycle Volatility

	Remoteness	Polity2	Trade %GDP	Govt Exp %GDP	Real GDP	Obs.
Default (11-yr c/s, 1994-2004)	.85** (.26)	-.13** (.04)	.005 (.004)	.04* (.02)	-7.3e-10** (1.9e-10)	152
27-yr c/s, 1977-2003	.39 (.20)	-.17** (.03)	-.001 (.003)	.05** (.02)	-1.5e-9** (2.5e-10)	129
5-yr c/s, 2000-04	1.39** (.30)	-.07 (.04)	.013 (.007)	-.01 (.03)	-5.5e-10** (1.5e-10)	149
Pooled across 5 11-yr periods	.64** (.12)	-.11** (.01)	.006 (.004)	.04** (.01)	-9.2e-10** (2.0e-10)	521
Drop countries >25 million pop.	.81** (.31).	-.14** (.04)	.002 (.005)	.03 (.02)	-6.1e-9** (2.2e-9)	116
Drop countries >$20k GDP p/c	.91* (.41)	-.12** (.04)	.005 (.007)	.04 (.02)	-7.2e-10** (2.1e-10)	131
Drop >|2σ| outliers	.93** (.22)	-.10** (.03)	.005 (.003)	.04* (.02)	-5.7e-10** (1.7e-10)	146
Add domestic credit (% GDP)	.92** (.35)	-.13** (.04)	.005 (.005)	.05* (.02)	-8.5e-10** (3.1e-10)	143
Add regional dummies	1.04** (.37)	-.16** (.04)	.000 (.005)	.02 (.02)	-6.4e-10 (2.3e-10)	143
Add log of latitude	.77* (.33)	-.16** (.04)	.003 (.005)	.04 (.02)	-8.2e-10** (2.0e-10)	143
Add landlocked, island dummies	.95** (.29)	-.12** (.04)	.006 (.005)	.04 (.02)	-7.0e-10** (2.1e-10)	152
Substitute Exec Constraint	.72** (.26)	-.53** (.12)	.006 (.005)	.04* (.02)	-6.2e-10** (1.7e-10)	149
Substitute Min Growth Rate	-1.68** (.57)	.16 (.08)	.005 (.009)	-.05 (.05)	2.3e-9 (5.6e-10)	152

Dependent variable is country-specific standard deviation of first-difference of log real GDP (in real international $), using annual data. Default sample is final 11-year period, 1994-2004 inclusive. Regressors are means over comparable periods.
Remoteness measured as log distance to closest major financial center (London, New York, or Tokyo).
Cross-sectional (except for pooled regression) OLS estimation with robust standard errors recorded in parentheses.
Coefficients significant at .05 (.01) level marked with one (two) asterisk(s).
Intercept (for all time periods when pooled) included but not recorded.

Table 2: Different Measures of International Financial Remoteness

Distance to Closest:	**Remoteness**	**Obs.**
Offshore Financial Center	.54 (.27)	152
Eight Largest Gross Debtors (CPIS data set)	.68** (.24)	148
Ten Largest Gross Creditors (CPIS data set)	.68** (.25)	146
Ten Countries with Largest Gross Capital Outflows (IFS data set)	.64* (.28)	137
Ten Countries with Largest Gross Capital Inflows (IFS data set)	.58* (.25)	137

Average Distance to:

Eight Largest Gross Debtors (CPIS data set)	.63 (.44)	148
Ten Largest Gross Creditors (CPIS data set)	.56 (.42)	146
Eight Largest Gross Debtors (CPIS data set), Weighted by liabilities	.90 (.53)	148
Ten Largest Gross Creditors (CPIS data set), Weighted by assets	.76 (.56)	146
Ten Countries with Largest Gross Capital Outflows (IFS data set)	.55 (.43)	137
Ten Countries with Largest Gross Capital Inflows (IFS data set)	.59 (.43)	137

Weighted Distance to Major Financial Centers

Host Transactions as Weights (CPIS data set)	1.17** (.34)	120
Source Transactions as Weights (CPIS data set)	.74 (.58)	54

Dependent variable is country-specific standard deviation of first-difference of log real GDP (in real international $), using annual data for 11-year period 1994-2004 inclusive. Regressors are comparable means.
Cross-sectional OLS estimation with robust standard errors recorded in parentheses.
Controls included but not recorded: real GDP, polity2, openness (%GDP), government spending (%GDP), and intercept. Coefficients significant at .05 level marked with asterisk.
Remoteness measured as log distance.
Intercept included but not recorded.

Table 3: Consumption instead of GDP

	Remoteness	Obs.
Default (11-yr c/s, 1994-2004)	.86** (.32)	152
27-yr c/s, 1977-2003	.54* (.23)	129
5-yr c/s, 2000-04	1.49** (.34)	149
Pooled across 5 11-yr periods	.85** (.17)	522
Drop countries >25 million pop.	.75* (.36)	116
Drop countries >$20k GDP p/c	.82 (.48)	131
Drop >|2σ| outliers	.68* (.30)	146
Add domestic credit (% GDP)	.84* (.37)	143
Add regional dummies	.64 (.39)	143
Add log of latitude	.53 (.40)	143
Add landlocked, island dummies	1.09** (.34)	152
Substitute Exec Constraint	.83* (.32)	149

Dependent variable is country-specific standard deviation of first-difference of log real consumption (in real international $), using annual data. Default sample is final 11-year period, 1994-2004 inclusive. Regressors are means over comparable periods.
Remoteness measured as log distance to closest major financial center (London, New York, or Tokyo).
Cross-sectional (except for pooled regression) OLS estimation with robust standard errors recorded in parentheses.
Controls included but not recorded: real GDP, polity2, openness (%GDP), and government spending (%GDP).
Coefficients significant at .05 (.01) level marked with one (two) asterisk(s).
Intercept (for all time periods when pooled) included but not recorded.

Table 4: Full-Sample Analysis over 1950-2004

Regressand is Standard Deviation of:	Remoteness	Obs.
1^{st}- differenced GDP	.27 (.18)	72
HP-filtered GDP	.002 (.003)	72
BK-filtered GDP	.003 (.002)	72
1^{ST}-differenced consumption	.56** (.20)	72
HP-filtered consumption	.006 (.003)	72
BK-filtered consumption	.006 (.003)	72

Dependent variable computed from natural logarithms (in real international $), using annual data over 55-year period 1950-2004 inclusive. Regressors are means over same period.
Cross-sectional OLS estimation with robust standard errors recorded in parentheses.
Coefficients multiplied by 100; those significant at .05 (.01) level marked with one (two) asterisk(s).
Controls included but not recorded: real GDP, polity2, openness (%GDP), government spending (%GDP), and intercept.
Baxter-King (BK) filter use minimum/maximum oscillation time of 2/8 years, with lead-lag length of 3 years. Hodrick-Prescott (HP) filter uses smoothing weight of 6.
Remoteness measured as log distance to closest major financial center (London, New York, or Tokyo).

Table 5: Time-Variation in the Effect of International Financial Remoteness

11-year periods	**Remoteness**	**Obs.**
1950-1960	.61* (.29)	49
1961-1971	.47* (.23)	75
1972-1982	.54* (.25)	117
1983-1993	.49 (.25)	128
1994-2004	.85** (.26)	152

27-year periods

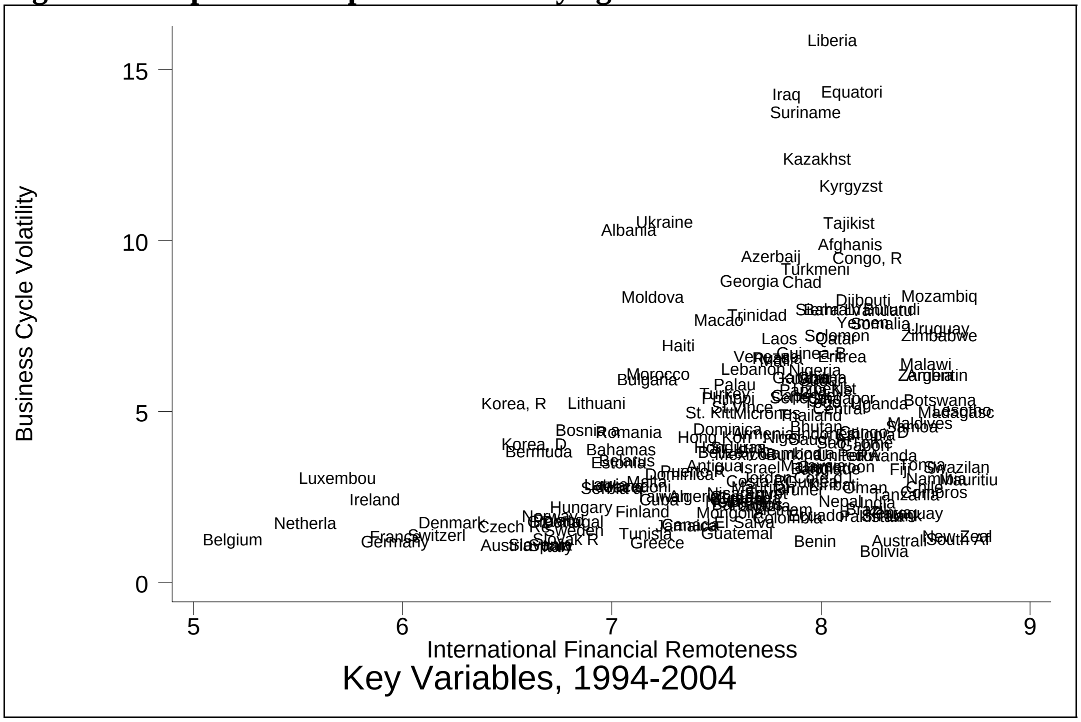
1950-1976	.72** (.24)	66
1977-2003	.39 (.20)	129

5-year periods

1950-1954	1.13* (.43)	46
1955-1959	.20 (.28)	49
1960-1964	.30 (.34)	73
1965-1969	.74* (.31)	83
1970-1974	.65* (.27)	113
1975-1979	.62 (.32)	121
1980-1984	.78* (.32)	122
1985-1989	.69** (.26)	123
1990-1994	.19 (.29)	130
1995-1999	.39 (.32)	151
2000-2004	1.39** (.30)	149

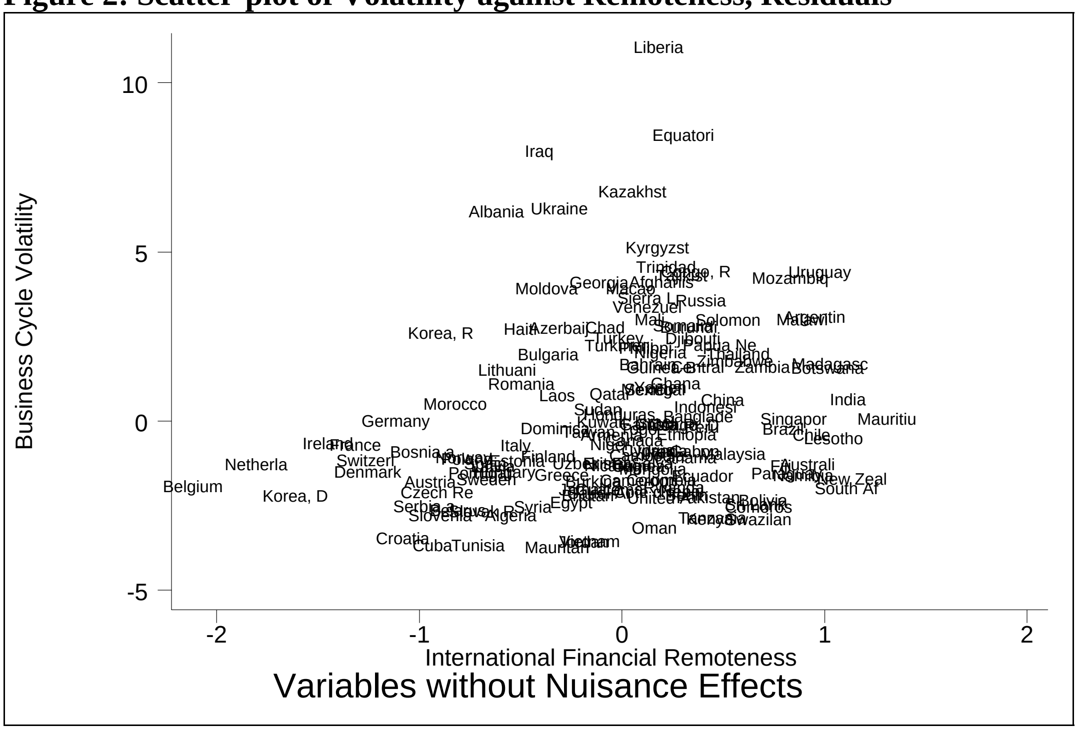
Dependent variable is country-specific standard deviation of first-difference of log real GDP (in real international $), using annual data. Regressors are means over same sample period.
Remoteness measured as log distance to closest major financial center (London, New York, or Tokyo).
Cross-Sectional OLS estimation with robust standard errors recorded in parentheses.
Coefficients significant at .05 (.01) level marked with one (two) asterisk(s).
Controls included but not recorded: real GDP, polity2, openness (%GDP), government spending (%GDP), and intercept.

Figure 1: Simple Scatter-plot of Volatility against Remoteness



International financial remoteness measured as great-circle distance to closest international financial center (New York, London or Tokyo)., scattered against standard deviation of output from 1994-2004 inclusive.

Figure 2: Scatter-plot of Volatility against Remoteness, Residuals



International financial remoteness measured as great-circle distance to closest international financial center (New York, London or Tokyo), scattered against residuals of regression of standard deviation of output (1994-2004) on default conditioning variables.

Appendix: Data Sources (Mnemonics in parentheses where available)

Penn World Table Mark 6.2 (http://pwt.econ.upenn.edu):

- Real GDP per capita, in constant international $ (rgdpl)
- Population (pop)
- Openness (i.e., exports plus imports), as percentage of GDP (openk)
- Government Spending, as percentage of GDP (kg)
- Consumption, as percentage of GDP (kc)

World Development Indicators (http://www.worldbank.org/data):

- Domestic Credit provided by banking sector, as percentage of GDP (FS.AST.DOMS.GD.ZS)
- Liquid liabilities (M3), as percentage of GDP (FS.LBL.LIQU.GD.ZS)

World Bank Country Classification (http://www.worldbank.org/data/countryclass/classgroups.htm)

- Geographic region and Income group dummies

Polity IV Project Data Set (http://www.cidcm.umd.edu/polity)

- Polity2 (polity2)
- Executive Constraints (xconst)

CIA World Factbook (http://www.cia.gov/cia/publications/factbook/index.html)

- Longitude and latitude
- Island and Landlocked status

Offshore Financial Center Location (http://faculty.haas.berkeley.edu/arose)

- Rose and Spiegel (2007)

Coordinated Portfolio Investment Survey Data set (http://www.imf.org/external/np/sta/pi/datarsl.htm)

- Aggregate portfolio assets from Table 12
- Aggregate portfolio liabilities from Table 13

International Financial Statistics (http://ifs/apdi.net/imf/about.asp)

- Capital inflows, direct (78bed)
- Capital inflows, portfolio (78bgd)
- Capital inflows, other (78bid)
- Capital outflows, direct (78bdd)
- Capital outflows, portfolio (78bfd)
- Capital outflows, other (78bhd)

Endnotes

[1] Aviat and Couerdacier (2007) explain gravity international finance models by stressing the complementarity between flows in assets and flows in goods. They demonstrate that after accounting for trade flows, the explanatory power of distance in financial flows is halved, but still not eliminated.
[2] Hong Kong may be an alternative to Tokyo, and is considered in earlier drafts of this paper.
[3] Our reduced-form specification allows geographic proximity to affect macroeconomic volatility through a variety of channels. While it can directly affect volatility by enhancing domestic consumption- or output-smoothing opportunities, access to external financial services has also been shown to affect domestic financial conditions [e.g. Rose and Spiegel (2007)], which may indirectly affect macroeconomic volatility.
[4] In an earlier version of this paper [Rose and Spiegel (2008)], we provide a formal model that links geographic remoteness to macroeconomic volatility through diminished financial integration.
[5] We choose 11-year periods because we have 55 years of annual data between 1950 and 2004 inclusive. This period is long enough to include entire business cycles. For sensitivity analysis, we also examine periodicities that are both shorter and longer.
[6] We compare our geographic-based measure of financial remoteness to a variety of more conventional measures of capital mobility in an appendix to an earlier version of this paper. The correlations are all small, indicating non-trivial measurement error in at least some indicators of capital mobility.
[7] In practice, we use the top eight debtors; there is a non-trivial gap between these and the remaining countries. Averaging available CPIS data between 1997 and 2005, these were: the USA; the UK; Germany; France; the Netherlands; Italy; Luxembourg; and Japan, all of whom had at least $50 billion in average liabilities.
[8] In practice, we use the top ten capital exporters which seem reasonable and account for most gross capital outflows. For 1994-2004, these were: the UK; the USA; Germany; France; Luxembourg; Ireland; the Netherlands; Japan; Spain; and Belgium.
[9] Kraay and Ventura (2007) find a negative relationship between trade remoteness, measured as total distance weighted by bilateral trade volumes, and volatility.
[10] The importance of domestic financial depth has been stressed by, among others, Acemoglu and Zilibotti (1997) and Bekaert, et al, (2006). In unreported sensitivity analysis, we add domestic credit provided by the banking sector, measured as a percentage of GDP, to our default equation. Its inclusion makes little difference to our results.
[11] While it is reassuring to us that the pooled coefficient is significantly positive, it turns out that there is considerable time-variation in the coefficient. We return to this issue below when we discuss Table 5.
[12] Earlier versions of the paper include a large number of other sensitivity checks which provide reassuring evidence of the robustness of our results.
[13] We choose eight and ten respectively since there seem to be obvious breaks in the series.
[14] The latter represent mostly transactions in currency and deposits, loans and trade credits.
[15] We average the CPIS data over the 2001-04 surveys inclusively.
[16] We use conventional parameter choices for both filters. For the BK filter, we use a minimum oscillation time of two years, and a maximum of eight, excluding three years at either end of our sample. For the HP filter, we use a smoothing weight of 6 for our annual data.
[17] The latter effect might be the result of the increasing sample size, but still implies that pooling the data over time is problematic.