THE EFFECT OF FOREIGN COMPETITION ON FORECASTING BIAS

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Abstract—This paper studies the effect of foreign competition on the extent of forecasting bias. I focus on two biases often described in the behavioral economics literature: overoptimism and excessive belief in trends. Using data from firm-level surveys in five African countries, I show that firms that do not face foreign competition generate forecasts of sales growth that have greater trend and optimism biases than firms that have foreign competitors. I further provide evidence that these erroneous forecasts have real effects on firms' inventory management.

I. Introduction

A number of earlier studies have found that entrepreneurs are overly optimistic and thus generate biased forecasts. This raises the question of whether there are conditions that reduce the extent of the bias. A recent survey by Glaeser (2004) describes a range of factors that may affect the biases of economic agents, and notes that market structure is surely one of them. However, there has been no empirical work to date on this question. My paper provides the first empirical contribution to examining the effects of competition on forecasting bias by studying the effect of foreign competition on the forecasts of African manufacturing firms.

I focus on a commonly cited pair of biases: overoptimism and belief in trends, which I will refer to below as optimism and trend biases respectively. There exists a large body of evidence from behavioral studies in the social sciences that provides empirical grounding for focusing on these particular biases; in the interest of brevity, I give only a couple of examples from this vast literature. On optimism bias, recent work by Malmendier and Tate (2003) studies the extent of overconfidence among CEOs, and correlates this with real economic outcomes. De Bondt (1993) provides experimental evidence on excessive belief in trends, finding that subjects overweight recent trends in predicting future observations when shown graphs generated by a well-defined process.

In this paper, I study the effects of foreign competition on these two types of bias, motivated by the long-standing claim that increased competitive pressures improve firms' operating efficiency. Using a unique data set derived from firm-level surveys in five African countries, I test for differences in expectation formation as a function of market competition. More precisely, I examine the extent of optimism and trend biases, and consider how these forecasting errors are affected by the presence of foreign competition.

The primary finding of this paper is that firms that do not face foreign competition are more prone to optimism and trend biases. First, though an optimism bias exists in all firms' forecasts in the sample, this bias is stronger in firms without foreign competitors. Further, forecasting errors are positively correlated with lagged changes in sales for firms without foreign competition (evidence of a trend bias), whereas firms that do face foreign competition exhibit no such correlation. I present some evidence that suggests that expectational errors lead to nonoptimal inventory levels for firms in my sample. Some caveats should be noted upfront: because of the discreteness of the outcome variable and the very short panel that I employ, the results of this paper should be treated with some caution, and further work will be necessary to better assess the relationship between competition and bias.

The rest of this paper proceeds as follows: in section II, I outline my estimation strategy. Section III gives some details on the data that are used in the empirical work. The main results are contained in section IV, as well as a discussion of the robustness of the results. In section V, I present evidence on forecasting errors and inventory control. I end with conclusions in section VI.

II. Empirical Specification

Consider a manager in firm f in industry i and country c who generates a forecast for future sales at time t + 1, given by S_{t+1}^{fic}. I model the forecasting error of the manager as

$$\varepsilon_{t+1}^{fic} = E(S_{t+1}^{fic} | I_t^{fic}) - S_{t+1}^{fic}, \tag{1}$$

where I_t^{fic} is the information available to the firm at time t. Under rational expectations all information contained in I_t^{fic} should be incorporated into the firm's actual forecast, $\hat{E}(S_{t+1}^{fic})$. An unbiased forecast conditional on information at time t should have the property

$$\hat{\varepsilon}_{t+1}^{fic} = \hat{E}(S_{t+1}^{fic}) - S_{t+1}^{fic} \perp I_t^{fic}. \tag{2}$$

Hence, a test of rational forecasting involves a test of $\beta = 0$ in

$$\hat{\varepsilon}_{t+1}^{fic} = \beta I_t^{fic} + \delta_{t+1}^{fic}, \qquad \text{where} \quad \delta_{t+1}^{fic} \sim N(0,\sigma). \tag{3}$$

This is the formulation of Mincer and Zarnowitz (1969) that has become standard in the macrorational expectations literature. The two biases described in the introduction imply that $\hat{\varepsilon}(S_{t+1}^{fic})$ will have two components that are contained in I_t^{fic}: a constant $\eta > 0$ (optimism bias) and a trend in sales, that is, $S_t^{fic} - S_{t-1}^{fic}$ (trend bias).[1] Hence:

$$\hat{\varepsilon}_{t+1}^{fic} = \eta + \beta_1(S_t^{fic} - S_{t-1}^{fic}) + \delta_{t+1}^{fic}. \tag{4}$$

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[1] In the empirical analysis, I effectively consider a trend bias with proportional implementation, using log (S_t^{fic}) − log (S_{t-1}^{fic}), where the extrapolation is proportional.

To evaluate the hypothesis that there are differences in the extent of bias between firms with and without foreign competition, I consider

$$\hat{\varepsilon}_{t+1}^{fic} = \eta + \beta_1(S_t^{fic} - S_{t-1}^{fic}) + \beta_2 FCOMP^{fic} + \beta_3 FCOMP^{fic}(S_t^{fic} - S_{t-1}^{fic}) + \delta_{t+1}^{fic}, \quad (5)$$

where $FCOMP^{fic}$ is an indicator variable denoting whether firm f faces foreign competition. Conditional on the existence of optimism and trend biases, the two main hypotheses of this paper are that $\beta_2 < 0$ and $\beta_3 < 0$.

Unfortunately, the forecasting data in my sample only provide the sign of the forecast, so that the actual estimating equation will be of the form

$$\text{sign}[\hat{E}(S_{t+1}^{fic}) - S_t^{fic}] - \text{sign}[S_{t+1}^{fic} - S_t^{fic}] = \eta + \beta_1(S_t^{fic} - S_{t-1}^{fic}) + \beta_3 FCOMP^{fic}(S_t^{fic} - S_{t-1}^{fic}) + \delta_{t+1}^{fic}. \quad (6)$$

The dependent variable in equation (6) is still informative with regard to forecasting error: It measures whether the *sign* of the manager's forecast change in sales is correct, and $\beta_2 < 0$ and $\beta_3 < 0$ may still be indicative of greater bias of firms without foreign competition. However, this discrete variable has two main shortcomings. First, it does not tell us anything of the magnitude of forecasting errors. I try to address this concern in the data section below by generated imputed magnitudes for forecast errors based on regional shocks. Additionally, though it is straightforward to show that if the left-hand side of equation (6) is not zero, it has the same sign as the actual forecast error, the converse is not the case. That is, forecast errors exist when $\text{sign}[\hat{E}(S_{t+1}^{fic}) - S_t^{fic}] - \text{sign}[S_{t+1}^{fic} - S_t^{fic}] = 0$, but these errors cannot be signed. If actual forecast errors for these observations are not correlated with the presence of foreign competition, this will not generate any systematic bias. Further, in order for this to generate a trend bias, the differential forecast errors must also be correlated with lagged changes in sales, a possibility that I discuss in the next paragraph.

If sales are generated by significantly different processes for firms with and without foreign competition, we may generate negative estimates of β_3 even if there is no differential trend bias for firms without foreign competition. In particular, suppose that all managers have a trend bias in the sense that I have formulated here, so that $\hat{E}(S_{t+1}^{fic}) = S_t^{fic} + \beta(S_t^{fic} - S_{t-1}^{fic})$, *regardless of the true underlying sales generating process.* If firms with foreign competition follow this trending pattern, while the sales of firms without foreign competition follow, say, a mean-reverting process, then a trend bias will only be observed in the latter group, even though both groups use identical heuristics in predicting future sales.

The discreteness of the dependent variable may also contribute to the estimation of a spurious difference in trend bias in this situation: If sales were trending downward for $FCOMP = 1$ firms, but flat for $FCOMP = 0$ firms, then the left-hand side of equation (6) would be zero for a larger range of $\hat{\varepsilon}_{t+1}^{fic}$, so that biases may appear to be different simply because fewer errors are observed for firms with foreign competition.

In the data section, I provide a tentative analysis of whether any differences in the serial correlation of sales exist between the two types of firms. However, because the data only contain three years per firm, it is not possible to properly investigate the time series properties of sales, or how this may differ across firms with and without foreign competition. Hence, the aforementioned alternative explanations imply that the results in this paper should be interpreted with some caution.

At this point, it also warrants emphasizing that the data do not allow me to identify of the precise mechanism through which competition reduces bias. The purpose of this paper is to provide evidence that there exists an effect of foreign competition, but not to identify the means by which this effect occurs. The basic channels through which this improved efficiency in forecasting *could* take place are selection, managerial behavior, and quality of feedback. Under selection-based explanations, inefficient (in this case biased) managers are unable to continue to operate profitably in the face of increased competition and are thus forced out of business, leaving only a more efficient pool of managers in operation. This is closely related to the arguments of Nelson and Winter (1982) on the selection of efficient routines and firm survival. Explanations based on managerial behavior focus either on learning induced by competitive pressures or on greater managerial effort in decision-making (see, for example, Hermalin, 1992). Finally, environmental circumstances may be such that it is simply easier to generate good forecasts in competitive markets due to better information on future market conditions, as in the models of Hart (1983) and Nalebuff and Stiglitz (1983).

III. Data

The data used in this paper come from surveys administered by the Regional Program on Enterprise Development (RPED) at the World Bank during 1992–1995, to five former British colonies in sub-Saharan Africa (Ghana, Kenya, Tanzania, Zambia, and Zimbabwe).[2] Three rounds of the survey were conducted in each country; where possible, the same firms were visited in each round. The survey instrument covered a wide variety of topics, including basic statistics on the firms' operations; the history of the firms and their owners; use of technology; competition and competitors (only in the later rounds); labor; financing and contractual relations; conflict resolution; regulation; infrastructure; and use of business support services.

[2] Two other countries (Côte d'Ivoire and Cameroon) were also covered, but were not visited for three sets of surveys.

The firms were chosen to reflect a size-weighted representative sample, by industry, of the universe of firms in each country. The industries included in the survey correspond approximately to two-digit SIC codes, and include wood and furniture, food processing, metal products, and textiles.

The data present a number of data quality concerns. Most importantly, errors may result from misreporting or mismeasurement of accounting data, such as sales, capital, and expenses. Deliberate misreporting, though often a concern, is unlikely to be driving the results, for the survey was carried out by an independent organization (as opposed to the government), so there was less incentive for managers to mislead or withhold information.

The dependent variable in most of what follows will be the difference between the sign of the expected change in sales and the sign of the actual change in sales, as in equation (6) above. The reason for this coarse measure of forecasting error is that the survey asked managers to predict whether they expected their sales to increase, decrease, or stay flat the following year. Responses to this question were used to construct the variable *EXPECTD,* which is increasing in expectations and takes on values of -1, 0, and 1.

To get a comparable measure of the realized change in sales, I begin by defining *SALES* as the firm's annual sales in local currency; I further define *DSALES* as the first difference in log(*SALES*), and *DSALES1* as the lagged value of *DSALES* (similarly, *SALES1* and *SALES2* are sales lagged 1 and 2 periods respectively). Hence, my measure of the error in expectations in equation (6) is given by $ERRORD = EXPECTD - \text{sign}(DSALES)$. Note that time subscripts will be dropped in the specifications that follow, because the data only allow for cross-sectional analyses.

As noted in the specification section above, apparent differential forecasting bias of firms with and without foreign competition could result from differences in the time series properties of sales for the two different types of firms. The main concern described in section II involves differential trends between the two groups. To investigate whether there is any evidence of differences in the serial correlation of sales changes for the two groups of firms, I consider the following regression:

$$DSALES^{fic} = \downarrow \eta^{ic} + \beta_1 FCOMP^{fic} + \beta_2 DSALES1^{fic}$$
$$\uparrow + \beta_3 FCOMP^{fic}\, DSALES1^{fic} + \delta^{fic}.$$

The empirical specification here is slightly different from equation (6) in that I include country-industry fixed effects. I first run this specification without the interaction term. The coefficient on *FCOMP* is negative, taking on a value of -0.15, but is not significantly different from 0 ($t = -1.12$). The coefficient on *DSALES1* in this regression is negative and significant, indicating that sales mean-revert on average. When the interaction term is added, I find that though β_3 is positive, it is not significant ($t = 1.02$). Hence, there is some evidence, albeit weak, that a differential sales trend could be partially responsible for my findings, and I acknowledge that the lack of significance may be due to a lack of power. That being said, I cannot reject, with available data, the hypothesis that changes in sales of firms with and without foreign competition have similar serial correlation properties.

Using *ERRORD* as a measure of forecasting errors is potentially inefficient, as valuable information is lost in transforming *DSALES* to a discrete variable. For example, a firm that expects an increase in sales and experiences a decrease in sales of 10% is making a less extreme (and potentially less costly) mistake than a firm that expects an increase in sales and experiences a decline of 90%. In order to utilize *DSALES* in formulating a measure of expectational errors, I require some continuous measure of expectations, which I will label *EXPECT.* To begin, observe that

$$EXPECT^{fic} = \begin{cases} E(DSALES^{fic} | DSALES^{fic} > 0) & \text{if } EXPECTD^{fic} = 1, \\ 0 & \text{if } EXPECTD^{fic} = 0, \\ E(DSALES^{fic}, | DSALES^{fic} < 0 & \text{if } EXPECTD^{fic} = -1. \end{cases}$$

In order to proxy for these expectations, I use the mean value of (realized) *DSALES* for all observations from the same industry-location cell of the firm that share its expectations. So, for example, for a firm in the textile industry in Kenya with positive expectations, *EXPECT* will be the average value of *DSALES* for all Kenyan textile firms with positive expectations. Thus,

$$EXPECT^{fic} = \frac{1}{N_{ic}} \sum_{g \varepsilon A^{fic}} DSALES^{gic},$$

where A^{fic} is the set of all firms with expectations in the same direction as firm *fic* in firm *fic*'s industry-location, and N_{ic} is the number of firms in *f*'s country-industry cell.[3] I may then define $ERROR = EXPECT - DSALES$.[4]

To document the real effects of potential forecasting biases, I also define a measure of inventory levels, *INVENTORY,* given by the total value of year-end raw material and finished goods inventories, deflated by *SALES.* The first difference of *INVENTORY* will be denoted by *DINVENTORY.* Unfortunately, I cannot calculate a lagged value of this variable, because very few inventory data were collected during the first round of the survey. Because of

[3] If $EXPECTD = 0$, that is, the expectation is that sales will stay flat, then $ERROR = -DSALES$.

[4] It could be argued that if the direction of expectations is correctly anticipated (that is, if $EXPECTD = 1$ and $DSALES > 0$, or $EXPECTD = -1$ and $DSALES < 0$), then the best estimate of expectations of the firm's expectations is the actual subsequent change in sales, that is, no error occurred in expectation. Replacing $ERROR = 0$ for all such firms, I obtain results that are marginally stronger than those reported below.

missing values of *INVENTORY,* regressions involving this variable have a much smaller sample size.

The presence of foreign competition is inferred from a survey question that asks firms whether one of their main competitors is foreign-owned. This is coded as an indicator variable *FCOMP* denoting whether a firm reported that at least one of its competitors for its principal product was foreign-owned. This includes foreign firms operating in the country as well as abroad. In addition to the direct effect of possibly more efficient foreign competitors, the presence of foreign competition is a strong signal that a sector has been deregulated and opened up to competition more broadly.

Note that any of several other measures of competitive pressures could potentially be correlated with the extent of forecasting bias. These include the number of competitors for a firm's principal product; the firm's market share; and entry and exit of competitors over the past year. When these measures of competition were used in place of *FCOMP* in regressions of the same form of equation (6), there was no evidence of reduced bias in forecasting. This may be attributable at least in part to shortcomings in these measures, both theoretical and practical, as indicators of competitive pressures. The number of competitors is potentially problematic in that a simple head count may not say much about the pressures created by competition: if all competitors are small and unsophisticated, there will be little pressure to drive out inefficiencies regardless of the number of competitors. Furthermore, serious data problems are associated with this measure: the questionnaire asked firms whether they had (a) no competitors; (b) one or two competitors; (c) three to five competitors; (d) six to nine competitors; (e) more than nine competitors. Given the rate at which pressures on margins have been shown to increase with the number of competitors (see Bresnahan & Reese, 1991), probably the only meaningful distinction is between the first two. However, so few firms were self-reported monopolists (3%) or duopolists or triopolists (4%) that there is very little scope for a meaningful statistical test. Market share is inappropriate because of serious endogeneity problems: efficient firms will likely garner a larger share of the market. The size of main competitors may be a useful measure, but is fraught with complications. Most importantly, competitor size needs to somehow be scaled relative to the firm's size or that of the market. I do not have data on market size, and looking at competitor size relative to firm size raises the same set of endogeneity issues as market share: a less efficient firm will remain small relative to its competitors. Furthermore, it is not clear that size alone should imply efficiency: for example, in these countries, size might equally well signal preferential treatment from government. Finally, whereas firm turnover is often used as a measure of entry barriers (see, for example, Dunne & Roberts, 1991), there is no way to calculate a turnover *rate,* because data are missing on the number of competitors for each firm.

Because foreign competition is obviously correlated with foreign ownership, it will be necessary to control carefully for this factor. I define *FOREIGN* to be a dummy variable denoting whether a firm has some foreign equity ownership.

The definitions of all variables are summarized in the data appendix.

The original sample contained 1,026, 1,031, and 944 firms in waves 1 through 3, respectively. Because I will require all three years' worth of data, firms that were not present in all years were dropped, reducing the sample to 754 firms. Firms lacking sales statistics in at least one year were also dropped[5] (71), as were firms with zero reported sales in at least one year (39); firms were also dropped that did not report their expectations about future sales (47). Finally, in as much as foreign competition is the focus of this paper, firms that did not respond to the question on the presence of foreign competitors were also dropped (97), yielding a final sample of 500 firms.

For the inventory regressions the sample size is considerably smaller, mostly because of missing data. Also, there were several very extreme observations, which likely reflected reporting errors. Seven such observations were deleted from these regressions, leaving a final sample of 276 firms for these regressions.[6]

Summary statistics for the main variables are given in table 1. Note that the mean value of *EXPECTD* is considerably higher than that of sign(*DSALES*), resulting in a mean value of *ERRORD* that is positive, that is, firms are, on average, apparently overoptimistic. Further, note that the mean of *EXPECTD* is considerably lower for firms facing foreign competition. This pessimism is justified to some degree by actual outcomes, as *DSALES* is generally lower for firms with foreign competitors. Overall, *ERRORD* is slightly lower for firms with foreign competition. I will more carefully compare the optimism bias between firms with different values of *FCOMP* in the results section below.

Also noteworthy is the high degree of dispersion in values of *DSALES1.* Given concerns about the precision of these data, it will thus be extremely important to ensure that any results are robust to considerations of measurement error.

IV. Results

Following the specification laid out in equation (6) above, the basic test of differential forecasting bias of firms with and without foreign competition takes the form

[5] Number of observations dropped in parentheses; deletions done sequentially.

[6] These all had inventory/sales ratios greater than 10. All of those remaining in the sample had values less than 2.

TABLE 1.—SUMMARY STATISTICS

(A.) Full Sample

	Mean	Std. Dev.	Obs.
SALES (US$ 000)	9,956*	1,660,497	500
EXPECTD	0.75	0.39	500
EXPECT	0.08	0.84	500
ERRORD	0.30	0.61	500
ERROR	0.21	1.10	500
DSALES1	−0.02	1.31	500
FOREIGN	13.91	31.06	372
FCOMP	0.16	0.37	500
AGE	20.60	14.84	499

B. By Presence of Foreign Competition

	FCOMP = 0			*FCOMP* = 1		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
SALES (US$ 000)	7,904*	502,035	420	35,395*	1,798,914	80
EXPECTD	0.77	0.46	420	0.62	0.38	80
EXPECT	0.15	1.10	420	−0.28	0.76	80
ERRORD	0.30	0.53	420	0.27	0.63	80
ERROR	0.21	0.49	420	0.16	1.18	80
DSALES1	−0.04	0.87	420	0.10	1.38	80
FOREIGN	12.24	37.23	305	21.53	29.34	67
AGE	19.90	16.88	419	24.30	14.33	80

*Medians reported for sales, because a few very significant outliers exist when sales figures are not expressed in logs. For variable definitions, see data appendix.

$$ERRORD^{fic} = \downarrow \eta^{ic} + \beta_1 FCOMP^{fic} \uparrow + \beta_2 DSALES1^{fic} + \beta_3 FCOMP^{fic} DSALES1^{fic} + \delta^{fic}, \quad (7)$$

where η^{ic} is a shock specific to industry i in country c, and δ^{fic} is the firm's idiosyncratic error. Note that time subscripts are omitted to emphasize that this is a cross-sectional regression, due to data limitations. The results of this basic specification are given in table 2 using an ordered logit specification. The direct effect of *DSALES1* is positive, implying the presence of a trend bias in the data overall; though the country-industry fixed effects obscures the existence of an overall optimism bias, the presence of such a bias is clear from the very large positive value of *ERRORD* in the summary statistics. Column (1) also provides evidence on differential optimism bias: the negative coefficient on *FCOMP* implies greater optimism among *FCOMP* = 0 firms. When the interaction term *FCOMP* × *DSALES1* is added in column (2), it is negative and of approximately the same magnitude as the coefficient on the direct effect of *DSALES1*. Hence, the data suggest that the trend bias is concentrated exclusively among *FCOMP* = 0 firms. These regressions are repeated using *ERROR* as the dependent variable in columns (5) and (6), yielding qualitatively very similar (though statistically weaker) results. Because this regression is in logs, the results lend themselves to a ready interpretation; the coefficient on *DSALES1* may be interpreted as an elasticity: an increase in the rate of change of past sales of 1% will result in a forecast of future sales that is more than $\frac{1}{2}$% too high for firms with no foreign competition. This effect is close to zero for firms with foreign competition.

We have several concerns regarding the robustness of the results reported above: the existence of extreme values in the sales data; the possibility of mismeasurement of sales;

TABLE 2.—THE EFFECT OF FOREIGN COMPETITION ON OPTIMISM AND TREND BIAS

	Dependent Variable: *ERRORD*				Dependent Variable: *ERROR*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DSALES1	0.567***	0.629***	0.636***		0.482***	0.519***	0.317***	
	(0.079)	(0.088)	(0.105)		(0.138)	(0.140)	(0.050)	
FCOMP	−0.483**	−0.432*	−0.422*	−0.402*	−0.169*	−0.116	−0.116	−0.122
	(0.243)	(0.241)	(0.240)	(0.237)	(0.091)	(0.082)	(0.080)	(0.085)
DSALES1 × FCOMP		−0.536***	0.552***	−0.916		−0.502***	−0.333***	−0.912***
		(0.193)	(0.201)	(0.700)		(0.167)	(0.115)	(0.289)
Instrumented DSALES1	No	No	No	Yes	No	No	No	Yes
Observations	500	500	490	500	500	500	490	500
R-squared	0.07	0.08	0.07	0.03	0.36	0.38	0.16	0.06

Robust standard errors in parentheses. Country industry fixed effects included in all regressions. See data appendix for variable definitions. *Significant at 10%; **significant at 5%; ***significant at 1%.

TABLE 3.—THE EFFECT OF FOREIGN COMPETITION ON OPTIMISM AND TREND BIAS: ROBUSTNESS TESTS

	Dependent Variable: *ERRORD*						Dependent Variable: *ERROR*					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DSALES1	0.625***	1.126***	0.583***	0.921***	0.636***	0.709***	0.500***	0.307	0.483***	0.333	0.360***	0.388***
	(0.083)	(0.420)	(0.079)	(0.248)	(0.102)	(0.116)	(0.147)	(0.446)	(0.140)	(0.263)	(0.062)	(0.065)
FCOMP	−0.586**	−0.537**	−0.501**	−0.441*	−0.531	−0.455	−0.211**	−0.149*	−0.168*	−0.121	−0.219*	−0.175
	(0.243)	(0.242)	(0.242)	(0.240)	(0.324)	(0.322)	(0.095)	(0.082)	(0.091)	(0.083)	(0.117)	(0.107)
LSALES2	0.088**	0.084*					0.032	0.022				
	(0.044)	(0.045)					(0.027)	(0.021)				
DSALES1 × *FCOMP*		−0.479**		−0.515***		−0.556***		−0.500***		−0.518***		−0.302**
		(0.196)		(0.194)		(0.216)		(0.174)		(0.178)		(0.122)
LSALES2 × *FCOMP*		−0.030						0.015				
		(0.027)						(0.036)				
log(*AGE*)			0.059	0.058					−0.032	−0.042		
			(0.115)	(0.116)					(0.057)	(0.056)		
log(*AGE*) × *FCOMP*				−0.112						0.072		
				(0.095)						(0.140)		
Foreign firms excluded?	No	No	No	No	Yes	Yes	No	No	No	No	Yes	Yes
Observations	500	500	499	499	295	295	500	500	499	499	295	295
R-squared	0.08	0.08	0.08	0.08	0.08	0.09	0.36	0.38	0.36	0.39	0.25	0.26

Robust standard errors in parentheses. Specifications (1)–(6) are ordered logit regressions. Specifications (7)–(12) are OLS regressions. Country-industry fixed effects included in all regressions. See data appendix for variable definitions. *Significant at 10%; **significant at 5%; ***significant at 1%.

unobserved differences across firms with different values of *FCOMP;* and interindustry heterogeneity. I deal with each of these issues now in turn.

As mentioned previously, there is a very high degree of dispersion in *DSALES1.* Though I do not have any theoretical justification for omitting outlying observations, it would be of some concern if the results were driven entirely by a small number of observations. In columns (3) and (7) I report results with the top and bottom 1% of observations on *DSALES1* omitted from the analyses; the results are qualitatively unchanged.

Issues of measurement error extend beyond concerns over outliers in these data. As always, attenuation bias is a concern. In the regressions that I present, mismeasurement of sales is particularly important for another reason, as I have used changes in sales as part of the left-side variable in table 2 (because it is a part of *ERROR*), and lagged changes in sales as a right-side variable. Thus, log(*SALES*) in the second year of the survey enters negatively on the left-hand side of the regression, and positively on the right. To the extent that this variable is mismeasured, this alone could account for the negative coefficient on *DSALES1*.[7]

A partial remedy for this type of problem is to use group averaging: assuming that measurement error is firm-specific, using group averages as an instrument for *DSALES1* should remove the correlation between *DSALES1* and *ERROR* that is simply due to measurement error. Group averaging should also mitigate the effects of attenuation bias. Using industry-location averages of *DSALES1* as an instrument for lagged sales results in coefficients that are qualitatively similar to those in the original regressions [see table 2, columns (4), (8)]. The resulting coefficient is significant at the 5% level in the regression with *ERROR* as the dependent variable, though not in the *ERRORD* regression, owing to a very large standard error. Moreover, in both regressions, the magnitude of the reported effect is larger, owing perhaps to attenuation bias in the original results.

The second significant concern is that of unobserved heterogeneity that may be correlated with *FCOMP.* In particular, one might imagine that firm "quality" or sophistication could be higher in markets that foreign firms choose to enter. In table 3, I try to control better for these factors by including other measures of firm sophistication as controls, both directly and interacted with *DSALES1*. As illustrated in columns (1)–(4), the inclusion of firm size (as proxied by initial sales) and age as covariates does not affect the basic results reported in table 2. In columns (5) and (6), I consider

[7] A number of factors may mitigate these concerns. First of all, as a check on the validity of the reported data, firms were asked in the third round of the survey to assess ex post whether their sales had increased over the previous year. Unfortunately, many observations are missing for this variable, because managers simply referred the surveyors to the accounting section of the survey for this information. However, for those that did respond there is a very high degree of concordance between this ex post measure of the change in sales and the actual change in sales. The two measures differ primarily because of ex post overoptimism on the part of managers (that is, many more managers believed that sales had increased than actually saw higher sales). Nonetheless, using this ex post measure yielded qualitatively identical results to those reported in the text. Furthermore, it does not appear that the correlation between *ERROR* and *DSALES1,* and more importantly, the difference in the correlation of these two variables between firms with and without foreign competition, is primarily due to the correlation of *DSALES* and *DSALES1.* Consider the following regression: $EXPECT^{fic} = \eta^{ic} + \beta_1 FCOMP^{fic} + \beta_2 DSALES1^{fic} + \beta_3 FCOMP^{fic} DSALES1^{fic} + \delta^{fic}$, that is, the same regression as in equation (7), but with *EXPECT* as the dependent variable. The coefficients obtained with this specification are similar to those obtained using *ERROR* as the dependent variable, that is, firms without foreign competition are more backward looking in forming their expectations. Because *DSALES* does not appear on the left-hand side, this regression is not vulnerable to the mismeasurement problems described above. Of course, it could be that the differences in expectations in this regression are justified by actual outcomes, that is, there is more serial correlation for firms without foreign competition. This is why the focus of this paper is on expectational *errors,* as reported in the main text. Finally, if measurement error is to account for the significance of the interaction term in table 2, one must argue that there are systematic differences in the extent of mismeasurement between firms with and without foreign competitors.

only firms that report no foreign ownership; the effect of foreign competition remains even among this subset of firms. Thus, the presence of foreigners (rather than foreignness itself) seems to attenuate bias.

Finally, there may be concerns that the coefficient on *DSALES1* may differ across industries or countries, because of differences in the predictability of future market conditions. This, combined with a different distribution of firms with foreign competitors across industry-country cells, could bias the coefficient on *DSALES1* × *FCOMP* in the preceding regressions, which implicitly assume constant slopes across industry-country cells. To assess this possibility, regressions were run with the sample split into *FCOMP* = 0 and *FCOMP* = 1 firms, yielding results that are consistent with those reported in table 2.

V. Real Effects

The preceding results are based solely on self-reported predictions. In order for these forecasts to be economically relevant, they must translate into real economic decisions.[8] Ideally, I would like to show that mistaken beliefs lead to nonoptimal investment behavior. This would be difficult even with a much more complete data set. With the data I have, it is not possible to make a convincing argument in this regard, because it is not feasible for me to make inferences about the rates of return of firms' investments. To illustrate some of the difficulties involved, consider the following: in my data set, investment *is* positively correlated with lagged changes in sales, but this does not imply nonoptimal behavior. Though sales are mean-reverting, the rate of reversion is far less than 1, so if sales increase between periods 0 and 1, sales are still expected to be higher in period 2 than in period 0 (though lower than sales in period 1). If investment is largely driven by this persistent element of changes in sales, then a firm may rationally react to an increase in sales by investing the following year.

I look at the effects of incorrect expectations on short-run investment in inventory stocks. A common assumption in modeling inventories is that the firm wishes to keep inventories at a constant fraction of sales [see, for example, Blinder (1990) for a description of such models]. Therefore, a given firm will have a desired inventory level that may be represented by (where the asterisk distinguishes desired from realized inventory levels)

$$\left(\frac{INVENTORY_t^{fic}}{SALES_t^{fic}}\right)^* = \alpha_{fic}.$$

As is also common in the inventories literature, I will assume that firms are required to make inventory orders ahead of actual production, so inventories will increase (decrease) if expectations are higher (lower) than actual sales. Therefore, a reduced-form equation based on standard inventory modeling assumptions is

$$DINVENTORY_{t+1}^{fic} = \left(\frac{INVENTORY_{t+1}^{fic}}{SALES_{t+1}^{fic}}\right) - \left(\frac{INVENTORY_t^{fic}}{SALES_t^{fic}}\right) = \beta\varepsilon_{t+1}^{fic}, \qquad (8)$$

where $\hat{\varepsilon}_{t+1}^{fic}$ is the firm's forecasting error. I am primarily interested in the component of the change in inventories that may be tied directly to differential trend bias across firms with and without foreign competition. To capture the component of $\hat{\varepsilon}_{t+1}^{fic}$ that is specific to the differential bias of foreign competition, I use equation (7) as the first stage to predict $\hat{\varepsilon}_{t+1}^{fic}$, and run equation (8) as an instrumental variable regression with both *FCOMP* and *DSALES1* as controls. In this approach, the instrumented error captures *only* the component that is driven by differential bias. Note once again that because actual analyses will be run using a cross section, time subscripts do not appear in the specifications below. To summarize, the instrumental variables specification that I utilize is the following:

First stage:

$$ERROR^{fic} = \eta^{ic} + \beta_1 FCOMP^{fic} + \beta_2 DSALES1^{fic} + \beta_3 FCOMP^{fic} DSALES1^{fic} + \delta^{fic}.$$

Second stage:

$$DINVENTORY^{fic} = \eta^{ic} + \beta_1 FCOMP^{fic} + \beta_2 DSALES1^{fic} + \beta_3 ERRORIV^{fic} + \delta^{fic},$$

where *ERRORIV* is the instrumented error from the first-stage regression.

In table 4, column (1) shows the reduced-form results of the inventory regressions, where *DINVENTORY* replaces *ERRORD* as the dependent variable in equation (7). The coefficient on the interaction term *DSALES1*×*FCOMP* is negative and significant at the 10% level, implying a weaker sensitivity of changes in inventories to past sales trends. In column (2), multivariate outliers on *DINVENTORY* and *DSALES1,* determined by the method of Hadi (1992), are dropped from the sample; this does not substantially change the results. Columns (3) and (4) repeat the same regressions, this time using *DSALES1*×*FCOMP* to instrument for *ERRORD.* This generates similar results to the reduced-form regressions.

Thus, backward-looking expectations apparently have a significant effect on real outcomes, in the form of inventory levels. To the extent that these "mistakes" lead to nonoptimal inventory levels, this is one potential channel through which biased expectations may be costly.

[8] A serious problem with looking for "real" effects in general is that it is generally possible to account for the observed patterns in the data within the context of rational expectations if individuals' loss functions are asymmetrical. See, for example, Zellner (1986).

TABLE 4.—THE EFFECT OF FORECASTING BIAS ON CHANGES IN INVENTORIES

	(1)	(2)	(3)	(4)
DSALES1	0.069***	0.056**	0.001	−0.003
	(0.025)	(0.024)	(0.031)	(0.024)
FCOMP	0.124***	0.040	0.160***	0.081*
	(0.047)	(0.032)	(0.053)	(0.045)
DSALES × *FCOMP*	−0.078*	−0.067*		
	(0.042)	(0.036)		
ERRORD			0.288*	0.256*
			(0.150)	(0.134)
Outliers excluded?	No	Yes	No	Yes
Observations	276	266	276	266
R-squared	0.24	0.37	0.24	0.30

Robust standard errors in parentheses. Industry-country fixed effects included in all regressions. Dependent variable in all regressions is *DINVENTORY. ERRORD* is instrumented using the interaction *FCOMP* × *DSALES1* in columns (3) and (4). For details on the specification, please see section V of the text; for variable definitions, see the data appendix. *Significant at 10%; **significant at 5%; ***significant at 1%.

VI. Conclusions

In this paper, I present evidence suggesting that there may be greater optimism and trend biases in firms that do not face foreign competition. Furthermore, these differential errors in expectations translate into larger errors in inventory decisions for firms without foreign competition. My findings suggest a potential bridge between neoclassical and behavioral economics. The standard economic reasoning that managers with costly heuristics will be competed out of existence does seem to hold, but only if competition allows for this to take place. Though the results presented in this paper must be treated with some caution due to data limitations, they suggest that the broader agenda of understanding the effects of market competition on forecasting behaviors is indeed a promising direction for future work.

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DATA APPENDIX

SALES total firm sales, in local currency

SALES1 one-year lag of *SALES*

SALES2 two-year lag of *SALES*

DSALES first difference of log(*SALES*)

DSALES1 one-year lag of *DSALES*

EXPECTD variable that takes on values −1, 0, or 1 in response to the question: "Do you expect that your sales will increase, decrease, or stay the same next year?"

EXPECT a constructed proxy for expectations, based on industry-location averages for firms with common values of *EXPECTD*. So

$$EXPECT = \frac{1}{N_A}\sum_A DSALES,$$

where A is the set of observations in the firm's industry-location cell that share the same value of expectations as the firm.

ERRORD error in expectations, defined as *EXPECTD* − sign(*DSALES*)

ERROR error in expectations, defined as *EXPECT* − *DSALES*

FORCOMP dummy variable denoting whether the firm reported that one of its main competitors is foreign-owned

FOREIGN dummy variable taking on a value of 1 if the firm is at least 10% foreign-owned.

INVENTORY value of year-end raw-material and finished-goods inventories, deflated by sales.

DINVENTORY First difference of *INVENTORY.*