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What's in a Name? Mutual Fund Flows When Managers Have Foreign-Sounding Names

Alok Kumar
University of Miami

Alexandra Niessen-Ruenzi
University of Mannheim

Oliver G. Spalt
Tilburg University

We show that name-induced stereotypes affect the investment choices of U.S. mutual fund investors. Managers with foreign-sounding names have about 10% lower annual fund flows, and this effect is stronger among funds with investor clienteles more likely to be suspicious of foreigners. Foreign-named managers experience lower appreciation (greater decline) in flows following good (bad) performance. Following 9/11, flows to funds with managers with Middle-Eastern-sounding names declined abnormally. In an experimental setting in which skill differences are absent, individuals allocate 11% less money to an index fund managed by a foreign-named manager. This gap widens following the Boston marathon bombings. (*JEL* G23, J71)

> *And I got my middle name from somebody who obviously didn't think I'd ever run for president.*
>
> – Barack Hussein Obama
> Al Smith Dinner, New York, October 16, 2008

Upon hearing a person's name, individuals typically assign, either consciously or subconsciously, a host of attributes to the person; the attributes are often related to the "group" (i.e., country of origin, religion, ethnicity, culture, etc.)

associated with the name. Often, name-related stereotypes are activated almost spontaneously without much conscious effort (Kunda 1999). These stereotypes consequently may color the initial impressions of the person. For example, names such as "Zaheer Sitabjhan" or "Toshihiko Tsuyusaki" are likely to invoke a different reaction than American-sounding names, such as "Matthew Brown" or "Robert Stevens."

One of the main reasons for this perception is that a person with a less familiar, foreign-sounding name may trigger a strong sense of in-group bias. People systematically adopt favorable opinions about members of their own group and might be indifferent or have lower opinions about members who are outside of their group (e.g., Tajfel 1982; Hewstone, Rubin, and Willis 2002). As a result, out-of-group individuals may be trusted less, could undergo closer scrutiny, and may even experience some form of discrimination. If these biases take an extreme form, they may generate emotions of fear or hostility and even induce xenophobic feelings (e.g., Allport 1954).

Using a field experiment in which names are randomly assigned to resumes, Bertrand and Mullainathan (2004) show that employers discriminated against candidates based on their name. Individuals with names that are distinctively African-American, such as "Lakisha" and "Jamal," are less likely to be invited for a job interview. While in that study there are no differences among candidates' resumes (by design), it is still possible that names proxy for unobservable differences in productivity.[1] Because job productivity is largely unobservable, it is very hard to distinguish between social bias-induced taste-based discrimination and pure statistical discrimination in most labor market settings.

In this paper, we analyze whether potential stereotypes associated with a person's name affect the investment choices of mutual fund investors. Specifically, we examine whether investors are less likely to invest in mutual funds that are managed by individuals with foreign-sounding names. The mutual fund setting is particularly attractive for studying social bias because fund management is one of the very few occupations in which good measures of job performance are available. In particular, the performance of fund managers is observable from fund returns, allowing us to overcome challenges in the prior literature where it has been difficult to account for productivity differences that might be correlated with observable individual attributes.

Our key conjecture is that funds managed by individuals with foreign-sounding names will have lower fund flows even if managers of those funds do not have inferior investment skill. In addition, those managers may be "punished" more after bad performance and "rewarded" less after good past performance; that is compared to funds managed by individuals with non-foreign-sounding names, flows will decline more following poor performance and may appreciate less following good performance. We also posit that name-induced

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[1] In a related study, Fryer and Levitt (2004) show that distinctively African-American names are correlated with parental behavior, which in turn is negatively related to the child's productivity.

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stereotypes are likely to be stronger among individuals who are more suspicious of foreigners. Consequently, name-induced flow patterns would be stronger for mutual funds that have a higher fraction of these individuals as investors.

We test our conjectures using a novel, hand-collected dataset that contains measures of foreignness of a large sample of mutual fund managers from the perspective of a U.S. resident. We are not particularly interested in differentiating between fund managers who are U.S. citizens and those who are foreigners. Instead, we want to capture whether a name *sounds* foreign when heard, read in a fund prospectus, or is found on a mutual fund Web site. Since information on nationality is rarely disclosed, the name of the fund manager is the only personal information that is available to most investors. The perceived foreignness of a name might trigger social biases and influence the investment decisions of mutual fund investors. For brevity, we refer to funds managed by individuals with foreign-sounding names as foreign funds.

One of our key findings is that flows in funds managed by individuals with foreign-sounding names are around ten percentage points lower, compared with funds managed by individuals with typical American names. Further, compared to an otherwise identical fund managed by an individual with an American name, managers with foreign-sounding names experience 14.2 percentage points higher outflows and 46.7 percentage points lower inflows when their recent performance is in the bottom or the top decile of all mutual funds, respectively. We observe these flow differences even though funds managed by individuals with foreign-sounding names are very similar to other funds in terms of performance.

The results from several additional tests support the taste-based discrimination conjecture. In particular, we find evidence of taste-based discrimination using the 9/11 terrorist attacks as an exogenous event that may have exacerbated negative stereotypes against individuals with names of South Asian and Middle Eastern origin. Following the 9/11 terrorist attacks, fund managers with Middle-Eastern and South-Asian-sounding names experience a drop in fund flows relative to other managers with foreign-sounding names. We also exploit a change in the law governing disclosure of individual names for team-managed funds and demonstrate that fund management companies were more likely to assign managers with foreign-sounding names to teams before the change, presumably because they were then effectively invisible to fund investors.

To better establish the causal relation between the foreignness of fund manager names and fund flows, we conduct an online experiment, in which we randomly assign fund manager names to funds. We find that subjects allocate 10.8% less money to an index fund managed by an individual with a foreign-sounding name. The size of the effect is similar to the results we obtain using actual flow data. We conduct two additional experiments around the Boston bombings in April 2013 and find that the allocation gap widens substantially following the event. In contrast, in a placebo test in which both fund managers are assigned a typical American name, we find no significant differences in allocations.

In the next set of tests, we examine directly whether the foreignness of fund manager names influences the mutual fund investment decisions of individual investors. Using a small sample of brokerage customers at a large U.S. discount brokerage house, we show that individuals who live in regions with a greater proportion of foreign-born individuals invest more in foreign funds. This evidence is consistent with our conjecture that in-group bias affects the investment choices of individual mutual fund investors.

Overall, these findings suggest that the foreignness of fund manager names cannot be used for effective statistical discrimination in situations in which an investor could use it to distinguish between skilled and unskilled fund managers. Our evidence is more consistent with the view that taste-based discrimination among fund investors is likely to be an important driver of the differential flow patterns.

One potential alternative explanation for our findings is screening discrimination. Cornell and Welch (1996) present a rational theory of discrimination in which discrimination arises even when individuals do not have an innate preference for others who are like them. This discrimination equilibrium arises because people are able to evaluate the unobservable qualities of others more effectively when they have similar cultural backgrounds. This type of screening discrimination is unlikely to explain our findings. We find stronger results during periods following the 9/11 and Boston marathon bombing events, and it is not obvious why investors would be able to evaluate the unobservable qualities of fund managers more accurately following these two events.

One of the key implicit assumptions in our empirical analysis is that investors are aware of the identities of their fund managers. To examine whether this assumption is appropriate, we conduct an online survey in the United States and ask individuals whether they are aware of the fund manager when they pick a mutual fund. About 64% of survey respondents mention that fund manager identity is an important determinant of their fund choices, and 57% of respondents report that they knew their fund managers when they made their fund choices.

This evidence suggests that a sizeable proportion of mutual fund investors are likely to be aware of fund manager identities when they make their investment decisions. Thus, it is likely that fund manager names influence people's investment decisions.

Our finding that taste-based discrimination affects fund flows raises an intriguing equilibrium question. Why then would mutual funds employ individuals with foreign-sounding names? Typically, foreign-sounding names lead to a loss of income to the fund management company because fund companies are usually compensated based on assets under management. Although we do not have data on individual compensation contracts, our rough cost estimates indicate that the typical fund with a size of about $195 million can incur an average annual cost of $133,000 due to lower flows into foreign-managed funds. In view of these "gross" costs, what potential benefits offset those costs?

In the last part of the paper we provide some preliminary evidence toward answering this question. One potential benefit of hiring managers with

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foreign-sounding names may be that those managers are more skilled on average, so that their better performance compensates for the lower flows per unit of performance. Or, it is likely that fund management companies are simply unaware of the adverse effects of foreign names on fund flows. A final possibility is that foreign-named individuals have special skills to manage certain special funds associated with lower flows.

While all these mechanisms are plausible, we find that none of these explanations is fully consistent with our data. This leaves a number of plausible, but hard to measure, mechanisms that would yield positive effects from employing foreign-named managers. These include lower compensation paid to foreign-named managers, side benefits of being a diverse employer in the labor market, a decreased risk of being sued for discrimination in the hiring process, or benefits from additional business with entities such as governmental agencies that desire their business partners to be diverse employers. We find some evidence consistent with the last channel, but identifying the main equilibrium mechanism is left for future research.

These empirical findings contribute to a growing literature in behavioral finance that examines the effects of social biases, such as discrimination and stereotyping, on financial markets (e.g., Wolfers 2006; Kumar 2010; Niessen-Ruenzi and Ruenzi 2013). We demonstrate that social biases, such as in-group bias and stereotyping, can aggregate and have the potential to influence aggregate-level variables like fund flows. Several previous papers have examined the impact of psychological biases, such as overconfidence and the disposition effect on aggregate-level variables, such as turnover, liquidity, and returns. However, none of these studies investigates the impact of social biases on portfolio decisions and aggregate-level variables, which is the main focus of our study.

Our results also contribute to a broader literature in economics that provides evidence of in-group bias and discrimination in a variety of settings. Specifically, this literature documents that employers discriminate against minority groups in the labor market (e.g., Becker 1957; Bertrand and Mullainathan 2004). Similarly, consumers discriminate against products sold by or associated with individuals of other races (e.g., Nardinelli and Simon 1990; Ouellet 2007). Race-based discrimination influences decisions in sporting events (e.g., Price and Wolfers 2010; Parsons et al. 2011). Even in courtrooms, studies consistently find that race matters (e.g., Abrams, Bertrand, and Mullainathan 2012). In this paper, we provide evidence of name-induced taste-based discrimination and demonstrate that social biases, such as in-group bias and stereotyping, affect capital allocations in the mutual fund industry, which is one of the most liquid and competitive capital markets in the United States.

Previous mutual fund studies have shown that names of funds or fund managers could influence the investment choices of mutual fund investors. For example, Cooper, Gulen, and Rau (2006) demonstrate that fund inflows increase when mutual funds pick names that are associated with hot investment styles. Similarly, supporting our underlying premise that investors care about

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who is managing their fund, Massa, Reuter, and Zitzewitz (2010) show that fund management companies may strategically assign certain fund managers to anonymous management teams. In contrast to these studies, we relate fund manager names to social biases.

1. Data and Summary Statistics

We use data from multiple sources. This section provides a brief summary of all those datasets. In the Appendix, we define all variables used in the empirical analysis.

1.1 Mutual fund data
Our main data are from the Center for Research on Security Prices (CRSP) survivorship bias-free mutual fund database from 1993 to 2011. Although we consider both individual- and team-managed funds, our main focus is on the set of individual-managed U.S. equity funds.

The main variable of interest in most of our tests is the net inflow ("fund flow") for fund i in year t defined as

$$\text{Fund Flow} = \frac{TNA_{i,t} - TNA_{i,t-1}}{TNA_{i,t-1}} - r_{i,t},$$

where $TNA_{i,t}$ denotes fund i's total net assets at the end of year t and r_t denotes fund i's return (net of fees) in year t as reported in CRSP. To eliminate the impact of outliers, we drop the smallest and largest 1% of flow observations from our sample. Our main results are unaffected if, instead, we use raw flows, winsorize the observations, or drop all funds with TNA below $1 million.

1.2 Identifying managers with foreign-sounding names
We use Morningstar as the main source to obtain the names of fund managers. This choice is motivated by the findings of Massa, Reuter, and Zitzewitz (2010), who show that Morningstar name data are more accurate than the name data from CRSP. Morningstar is also a more likely source of information for mutual fund investors. In instances in which a Morningstar manager name could not be identified (about 4% of observations), we use the CRSP manager names instead.

To identify fund managers with foreign-sounding names, we survey a random sample of the U.S. population via the electronic Amazon Mechanical Turk (AMT) platform.[2] We asked individuals to indicate for each fund manager name whether it sounds foreign from the perspective of a U.S. resident by answering "Yes", "No", or "Unsure." In total we recruited 150 individuals

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[2] A growing number of studies in economics and psychology use AMT (e.g., Horton, Rand, and Zeckhauser 2011; Amir, Rand, and Gal 2012; Chandler and Kapelner 2013; Goodman, Cryder, and Cheema 2013). To our knowledge, our study is the first to use AMT in the finance literature.

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located in the United States to each classify 1,000 names. In line with Amazon's recommendations on hourly wages, we paid $7 for their work. We thus obtained thirty foreign classification scores for each fund manager. Our main variable of interest is Foreign75, which is a dummy variable that is set to one for names for which at least 75% of individuals that did not answer "Unsure" indicated the name as foreign sounding.

In our full sample, 17.60% of fund companies have at least one of their domestic equity funds managed by a manager with a foreign-sounding name. However, in most fund families, the fraction of domestic equity funds managed by managers with foreign-sounding names is small (= 4.77%). About 4.50% of fund-year observations in our sample are classified as foreign funds. Altogether, there are 4,805 individual U.S. equity funds and 3,784 unique managers in the full sample. Within this set, 364 of the funds (7.58% of total) are managed by 200 unique individuals (5.29% of total) with foreign-sounding names.

1.3 Attributes of funds by managers with foreign-sounding names

Table 1 presents univariate sorting results. The most striking feature of the table is that funds managed by individuals with foreign-sounding names have 10.9 percentage points lower annual fund flows. This difference is highly significant with a t-statistic of 4.5. We find this evidence not only in the pooled sample but also in a year-by-year comparison. Figure 1 shows annual fund flows for foreign-named managers and other managers separately. In 17 out of 19 years in our sample, flows to funds with foreign-named managers are lower than flows to other funds. Since we only consider funds that invest predominantly in U.S. domestic equities, lower flows into foreign funds cannot be explained by a potentially greater willingness of foreign-named managers to invest in foreign markets.

While flows are different for fund managers with foreign-sounding names, there is no economically or statistically significant difference between any of the other fund characteristics, including return ranks, size, turnover, fund risk, expense ratios, and fund age. Further, there is virtually no difference in the marketing expenses, the Morningstar rating, or the load versus no load feature of funds in the two groups. Focusing on fund manager characteristics, in most cases, we find no statistically significant difference between foreign and nonforeign funds. The sole exception is that foreign-named managers are mentioned less often in the media. However, we show later that while media cites influence flows, this difference in media coverage does not drive the relation between fund foreignness and fund flows.

2. Foreign Names and Fund Flows

In this section, we present our first set of main results. Our main objective is to investigate whether foreign-sounding fund manager names are related to mutual fund flows.

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Table 1
Fund manager names and fund characteristics

Variable	Foreign funds (1)	Other funds (2)	Difference (3)	t-statistic (4)
Fund attributes				
Fund flow	0.096	0.205	−0.109	−4.50
PRank	0.453	0.470	−0.017	−1.16
PRank^2	0.279	0.290	−0.011	−0.82
Fund size	5.278	5.392	−0.114	−0.61
Turnover	1.004	0.886	0.118	1.38
Fund risk	0.051	0.048	0.003	1.34
Expense ratio	0.016	0.013	0.003	1.09
Fund age	2.397	2.330	0.067	0.95
Segment flow	0.065	0.075	−0.010	−1.56
Family flow	0.428	0.261	0.167	1.58
Marketing expenses (NSAR-B)	0.106	0.109	−0.003	0.27
Morningstar rating	1.800	1.843	−0.043	−0.26
No load fund	0.480	0.478	0.002	0.05
Fund manager attributes				
Age	46.81	47.32	−0.51	−0.28
Gender	0.099	0.098	0.001	0.03
Bachelor	1.000	0.999	0.001	1.44
MBA	0.580	0.607	−0.027	−0.34
PhD	0.134	0.045	0.089	1.36
Fund tenure	5.227	5.771	−0.544	−1.09
Media coverage	0.538	2.119	−1.581	−4.24

This table reports the mean fund and fund manager characteristics sorted by our main variable Foreign75. Foreign75 is a dummy variable that takes on the value of one if at least 75% of AMT workers indicate that the name of the manager is foreign-sounding, and zero otherwise. The differences between the group means and the corresponding t-statistics, clustered by fund for fund attributes and clustered by manager for manager attributes, are reported in Columns (3) and (4), respectively. Fund Flow is the net inflow into the fund in the current year defined as $(TNA_{i,t} - TNA_{i,t-1})/TNA_{i,t-1} - r_{i,t}$, where $TNA_{i,t}$ denotes fund i's total net assets in year t, and r_t denotes fund i's return in year t as reported in CRSP. PRank is the performance rank of the fund in the previous year relative to all other funds in the same market segment, scaled to lie between zero (lowest performance) and one (highest performance). Fund Size is the lagged natural logarithm of the fund's size in million USD. Turnover is the fund's lagged turnover rate. Fund Risk is the lagged return time series standard deviation of the fund return using the past twelve monthly return observations. Fund Age is the natural logarithm of fund age in years at the beginning of year t. Segment Flow is the growth rate of fund i's market segment due to flows in year t, excluding flows in fund i. Family Flow is the growth rate of fund i's fund family due to flows in year t, excluding flows in fund i. Marketing expenses is the fraction of a fund's marketing expenses by its total expenses according to the fund's NSAR-B filing. No Load Fund is a dummy variable equal to one (zero) if the fund does (not) have load fees. Tenure is the difference of the current year to the start year of the fund manager at a given fund as reported in CRSP. Media coverage is the number of articles on a fund manager in a given year in all U.S. newspapers.

2.1 Flow regression estimates: Baseline results

Our key conjecture is that social biases, such as discrimination, stereotyping, and in-group bias, likely affect the investment decisions of mutual fund investors. As a result, the foreignness of a fund manager's name could steer capital away from those funds. Specifically, either consciously or subconsciously, investors may skew their investments more toward funds that are managed by individuals with names that sound more familiar to them. To test this conjecture, we start by examining the relation between the foreignness of fund manager names and fund flows. Specifically, we estimate flow regressions in which annual net fund flow is the dependent variable.

The main independent variable is the Foreign75 dummy variable, which indicates whether a fund manager's name sounds foreign.

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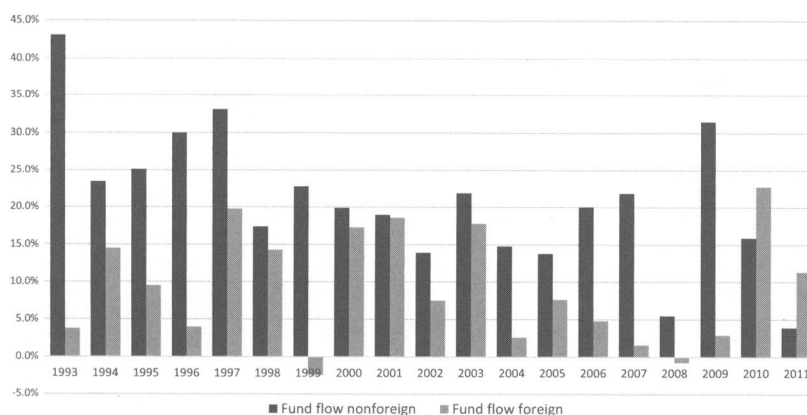
Figure 1
Annual fund flows, 1993 to 2011
This figure shows the average annual net fund flow for nonforeign and foreign funds. Fund flow is computed as:
Fund Flow$_t$ = $(TNA_t - TNA_{t-1})/TNA_{t-1} - r_t$. Foreign funds are funds managed by an individual with a foreign-sounding name according to the Foreign75 classification. Nonforeign funds are all other funds in the sample. Only funds that invest predominantly in U.S. domestic equities are included in the sample.

Our primary set of control variables includes fund size, turnover, fund risk, expense ratio, fund age, segment flow (i.e., the aggregate flow to funds that are in the same market segment during the year), fund family flow, fund manager tenure, and lagged fund flows. In addition, we control for fund returns by including performance rank, defined as the rank of the fund in the previous year relative to all other funds in the same market segment, as well as the squared performance rank measure.[3] We lag all control variables by one year. We estimate the flow regressions as a panel and include year, segment, segment × year, and fund family fixed effects, depending on the specification. We cluster standard errors at the fund level. For robustness, we also estimate Fama and MacBeth-type (1973) regressions.

Table 2, panel A, presents the flow regression estimates. Consistent with the view that investors discriminate among funds based on fund manager names, we find that the Foreign75 variable is significantly negatively related to fund flow across all specifications. The Fama and MacBeth (1973) and panel regression estimates are quite similar, and they are all economically significant. All else equal, fund flows are between 7.6 to 11.0 percentage points lower for a fund that is managed by an individual with a foreign-sounding name. Compared to the mean annual fund flow of 20%, these estimates are economically meaningful.

We include year and segment fixed effects in specifications (1) to (3). In addition, we control for segment × year fixed effects in specification (5) to show that even within the same segment and year, foreign funds receive lower

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[3] We find similar results when we control for other measures of past performance, including raw returns and performance quintiles.

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Table 2
Fund flow regression estimates

Panel A: Main results

	OLS	OLS	OLS	FMB	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign75	−0.098	−0.087	−0.089	−0.076	−0.085	−0.110
	(−4.10)	(−4.01)	(−4.06)	(−2.98)	(−3.87)	(−3.71)
PRank	0.732	0.604	0.052	−0.038	0.025	−0.027
	(18.52)	(15.34)	(0.41)	(−0.17)	(0.19)	(−0.20)
PRank^2			0.583	0.670	0.607	0.658
			(3.98)	(2.37)	(4.04)	(4.47)
Fund size	−0.062	−0.061	−0.060	−0.070	−0.060	−0.124
	(−9.85)	(−10.18)	(−10.07)	(−8.11)	(−9.98)	(−11.67)
Turnover	0.043	0.039	0.037	0.035	0.037	0.043
	(2.52)	(2.27)	(2.15)	(1.54)	(2.15)	(2.22)
Fund risk	0.841	0.426	0.017	0.489	−0.095	0.607
	(2.12)	(1.06)	(0.04)	(0.59)	(−0.22)	(1.33)
Expense ratio	−1.621	−1.757	−2.106	−6.641	−1.966	0.257
	(−1.48)	(−1.67)	(−1.90)	(−1.66)	(−1.82)	(0.23)
Fund age	−0.150	−0.093	−0.094	−0.087	−0.092	−0.064
	(−14.48)	(−9.07)	(−9.16)	(−5.33)	(−9.02)	(−4.98)
Segment flow	0.138	0.095	0.092	−1.738	−0.332	0.052
	(2.39)	(1.75)	(1.70)	(−2.19)	(−3.25)	(0.91)
Family flow	0.021	0.014	0.014	0.017	0.013	0.007
	(2.76)	(2.21)	(2.22)	(2.76)	(2.14)	(1.22)
Tenure	−0.002	−0.001	−0.001	−0.001	−0.002	0.007
	(−1.33)	(−0.73)	(−0.96)	(−0.59)	(−1.16)	(3.92)
Lagged fund flow		0.100	0.098	0.111	0.095	0.076
		(9.19)	(9.05)	(5.91)	(8.89)	(6.21)
Year FE	yes	yes	yes	no	no	yes
Segment FE	yes	yes	yes	yes	no	yes
Segment × year FE	no	no	no	no	yes	no
Fund family FE	no	no	no	no	no	yes
Adj/Avg. R^2	0.074	0.087	0.102	0.164	0.107	0.166
Observations	14,502	13,091	13,091	13,091	13,091	13,086

Panel B: Results using matched samples

	Coefficient	t-statistic	Observations
Baseline (Table 5, panel A, specification (3))	−0.089	−4.06	13,091
Matching attributes			
Year, and segment, and size	−0.085	−3.60	7,800
Year, segment, size, and fund age	−0.106	−3.40	2,583
Year, segment, size, and manager age	−0.069	−2.19	2,828
Year, segment, size, and performance	−0.086	−2.34	1,848
Year, segment, size, and MSA	−0.137	−3.31	1,965
Year, size, and fund family	−0.177	−4.20	1,925
Year, segment, size, and fund family	−0.164	−3.39	1,090
Year, segment, size, and education	−0.065	−1.77	2,757

(continued)

flows. This evidence indicates that our results are unlikely to be induced by some unknown factor that varies at the segment level. Finally, we consider fund family fixed effects in specification (6) and find that the effect of the Foreign75 indicator becomes even more pronounced. This finding suggests that time-invariant unobserved heterogeneity at the fund family level cannot explain our results. Overall, we conclude that fund manager names are correlated with fund flows.

Table 2
Continued

Panel C: Robustness checks

		Coefficient	t-value	Observations
(1)	Alternative foreignness measure (Foreign85)	−0.062	−2.56	13,091
(2)	Including international funds	−0.066	−3.30	16,143
(3)	Name fluency instead of foreignness	−0.047	−2.46	13,091
(4)	Control for name fluency			
	Coefficient on foreign75	−0.083	−3.78	13,091
	Coefficient on fluency measure	−0.045	−2.34	
(5)	Nonparametric size control	−0.088	−3.91	13,091
(6)	Exclude index funds	−0.089	−3.97	12,863
(7)	Control for other managerial attributes			
	Gender	−0.095	−4.33	13,071
	Gender and age	−0.073	−2.96	10,062
	Gender, age, and education	−0.079	−3.06	9,799
(8)	Control for marketing expenses (NSAR-B)			
	Coefficient estimate of Foreign75	−0.097	−2.84	3,453
	Coefficient estimate of marketing expenses	0.313	2.36	
(9)	Exclude mixed names	−0.068	−2.40	3,663
(10)	Distribution channels			
	Coefficient estimate of Foreign75	−0.081	−2.50	11,692
	Coefficient estimate of Foreign75 × No load	0.005	0.11	
	Coefficient estimate of no load dummy	−0.034	−1.99	
(11)	Industry experience			
	Coefficient on Foreign75	−0.090	−4.07	13,091
	Coefficient on industry tenure	−0.001	−0.44	
(12)	Control for Morningstar rating			
	Coefficient on Foreign75	−0.078	−3.33	11,692
	Coefficient on Morningstar rating 1	0.110	2.92	
	Coefficient on Morningstar rating 2	0.098	4.33	
	Coefficient on Morningstar rating 3	0.140	5.82	
	Coefficient on Morningstar rating 4	0.208	8.07	
	Coefficient on Morningstar rating 5	0.205	3.78	
(13)	Control for media coverage			
	Coefficient on Foreign75	−0.098	−3.45	9,165
	Coefficient on media coverage	0.038	3.18	

This table shows the estimates of percentage fund flows regressed on the Foreign75 indicator and various control variables. All independent variables, except for segment and family flows, are lagged by one year and have been defined in Table 1. In panel A, the model is estimated by pooled OLS in all columns, except for Column (4), which uses the Fama and MacBeth (1973) estimation method. Panel B presents results from estimating specification (3) from panel A on a sample of matched funds. We construct the matched fund sample by keeping for each fund with a manager with foreign-sounding name only the subset of funds with the same set of matching criteria in a given year. The following matching attributes are used: fund size, fund segment, fund family, fund location, manager age, fund performance, and education. MSA denotes funds headquartered in the same metropolitan statistical area. Panel C presents robustness checks. The t-statistics are displayed in parentheses below the coefficient estimates. The standard errors are clustered at the fund level.

2.2 Flow regression estimates using a matched sample

One potential concern with the flow regression results may be that our findings are spuriously induced by unobservable fund or managerial attributes. Because the set of foreign funds is small, our regressions might implicitly compare very different types of funds that would then also differ on those unobservable attributes. To mitigate these potential concerns, we identify a matched sample of funds for which we match the subset of foreign funds with similar nonforeign funds at the same point in time. As the foreign funds and matched funds are more similar on observable characteristics, they are also likely to be more similar on the unobservable characteristics. We then re-estimate the baseline regressions

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using only the sample of foreign funds plus matched funds. If our results are truly due to the foreignness of the manager's name, then restricting the analysis to more similar funds should not alter the results reported in Table 2, panel A.

We perform the matched sample analysis as follows. First, we identify a set of matching attributes, such as fund size, fund location, and manager education. Then, each year, for each foreign fund in the sample, we identify all nonforeign funds that match the foreign fund on the chosen dimensions. We keep only these nonforeign funds in the sample and drop all other nonforeign funds that do not have a matching foreign fund in the chosen year. This matching procedure yields a set of nonforeign funds that closely resembles our sample of foreign funds.

Table 2, panel B, presents the fund flow regression results for several alternative ways of performing the match. The results are based on specification (3) in panel A, where for brevity we only report the estimates of the Foreign75 dummy. The matched-sample results indicate that in many cases restricting the sample to better matches in terms of observable attributes increases the point estimates of the Foreign75 indicator variable. Specifically, when we match funds on year, fund segment, fund size, or lagged performance, we continue to find lower flows in funds managed by individuals with foreign-sounding names.

Our results remain essentially unchanged when we additionally match on fund age and manager age. Although the sample size shrinks significantly, statistical significance remains high. Further, the point estimates increase substantially when we focus only on funds that are headquartered in the same MSA and funds in the same fund family. Last, we show that even though managers with foreign-sounding names are slightly more educated (see Table 1), we do not find that the negative relation between the Foreign75 indicator and fund flows becomes insignificant when we consider a subsample of matched funds that are managed by individuals with similar education (i.e., with either a PhD, or an MBA).

Overall, the flow regression estimates in Table 2 indicate that fund flows are significantly lower when fund managers have foreign-sounding names. The matched sample analysis should attenuate concerns that unobservable fund and managerial attributes somehow spuriously generate these results.

2.3 Robustness checks and alternative explanations

We conduct an extensive set of checks to ensure that fund flow regression estimates are robust to sensible alterations of our main setup and we examine several potential alternative explanations. The results from these tests are summarized in panel C of Table 2. In all regressions, we use specification (3) of panel A as the benchmark.

We find that our results are essentially unchanged if we classify a name as foreign-sounding if at least 85% of AMT workers classify it as foreign. The key results also hold when we extend the sample of domestic equity funds and

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include international funds. We do not find that our results are driven by name fluency rather than the foreignness of a name.[4] We replace our fund size control variable with nonparametric controls for fund size, and we exclude index funds from the sample. Both alterations do not materially affect our results. When we include gender, age, and education as additional controls, we find they do not influence the results significantly.

To investigate whether our results are driven by fund management companies that spend less on advertising funds managed by individuals with foreign-sounding names, we collect data on a fund's total expenses, as well as its marketing expenses from the SEC annual NSAR-B filings. Then we compute marketing expenses as a fraction of total expenses and include this variable in our main regression. The results show that our main findings are not driven by differences in marketing expenses between foreign and nonforeign funds, which is consistent with the univariate results presented in Table 1.

In additional tests, we find that our main result is virtually unaffected if we drop "mixed" names (i.e., names that are classified as foreign by fewer than 75% AMT workers) from the sample. They are also unaffected by a fund's distribution channel as proxied by load versus no-load status. Controlling for a manager's industry experience or a fund's Morningstar rating also does not affect our results.

Finally, we control for a fund manager's media coverage. We include the logarithm of the annual number of newspaper articles about each fund manager as an additional control variable in our flow regression specification.[5] Consistent with previous evidence (Kaniel, Starks, and Vasudevan 2007), we find that media coverage has a significantly positive impact on fund flows. More importantly, we still obtain a significantly negative and essentially unchanged coefficient on our Foreign75 indicator variable (coefficient estimate $= -0.098$; t-statistic $= -3.45$).

Based on these findings from various additional tests, we conclude that our baseline results are robust to several reasonable alterations to the main empirical specification.

3. Evidence of Taste-Based Discrimination

In this section, we examine whether the observed flow patterns are consistent with the view that taste-based discrimination affects the decisions of U.S. mutual fund investors.

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[4] Motivated by Green and Jame (2013), we define a name fluency dummy that is equal to one if the number of characters in the name is below the median number of characters or the name does not appear in the 10,000 most commonly used names in the United States, and zero otherwise.

[5] We obtain data on media mentions of fund managers from Niessen-Ruenzi and Ruenzi (2013) and refer readers to their paper for details about the data collection procedure.

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3.1 Performance of foreign funds

We start by taking a closer look at the performance of foreign funds.

We compute the return of a portfolio that assumes a long position in all foreign funds and a short position in all nonforeign funds in our sample. We assign funds to the foreign and nonforeign fund portfolios at the beginning of every month. Table 3, panel A, reports the alpha estimates from this long–short strategy. We find that there is no detectable performance difference in any of the specifications considered in panel A. Regardless of the factor model used to obtain risk-adjusted performance, alpha estimates are always close to zero and never even close to being statistically significant. For example, the alpha estimate in specification (1) translates into a difference of only 23 basis points per year ($-0.019\% \times 12$) and is statistically insignificant (t-statistic $= -0.58$).

Panel B of Table 3 presents results using an alternative approach, where we regress individual fund alphas on our main Foreign75 indicator variable as well as year and segment fixed effects. We use one year of daily returns (Columns (1) to (3)) and three years of monthly returns (Columns (4) to (6)) to compute the alphas in these tests. We cluster standard errors at the fund level. We repeat this analysis in panel C using gross returns, computed for the daily and monthly specifications by adding the appropriate fraction of the expense ratio to the net returns. This test would capture performance differences better if higher skill were reflected in higher expense ratios. The results in both panels portray a picture consistent with what we find in panel A. There are no significant differences in the performance estimates of foreign funds, regardless of the method or benchmark used.[6]

As an additional performance test, we compare fund performance using the matched sample approach from Section 2. The results in panel D of Table 3 do not show any statistically significant performance differences between foreign and nonforeign funds, and the economic magnitudes of the point estimates are small.

In the last set of performance tests, we follow the Massa, Reuter, and Zitzewitz (2010) approach and regress various fund performance variables on a large set of controls. Panel D presents those results. We consider net returns, CAPM-adjusted returns, four-factor adjusted returns, and the expense ratio as dependent variables. Lagged controls include an indicator for no-load funds, the expense ratio and 12b-1 fees (except in specification (4)), fund and fund family size, fund age, turnover, fund flow, past return, and the standard deviation of the past return measure. We cluster standard errors at the fund level. We again find no trace of a performance difference between foreign and other funds. In additional tests, we analyze differences in total risk, idiosyncratic risk, turnover, and tracking error. All estimates (unreported) are statistically indistinguishable between the two groups.

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[6] Other performance measures, such as the raw returns, Sharpe ratios, or appraisal ratios, also yield similar results.

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Table 3
Performance of foreign funds and other funds

Panel A: Fund performance: Portfolio evidence (foreign − nonforeign)

	Equal-weighted (1-3)			Value-weighted (4-6)		
	CAPM	3-Factor	4-Factor	CAPM	3-Factor	4-Factor
	(1)	(2)	(3)	(4)	(5)	(6)
Alpha$_t$	−0.019	−0.009	−0.002	−0.001	0.027	0.016
	(−0.58)	(−0.29)	(−0.07)	(−0.01)	(0.61)	(0.36)
Mkt-RF$_t$	−0.008	−0.021	−0.025	0.024	0.011	0.016
	(−1.03)	(−2.82)	(−2.95)	(2.14)	(1.06)	(1.58)
SMB$_t$		0.032	0.034		−0.002	−0.003
		(2.09)	(2.10)		(−0.12)	(−0.25)
HML$_t$		−0.040	−0.043		−0.073	−0.069
		(−3.19)	(−3.50)		(−4.13)	(−3.85)
MOM$_t$			−0.009			0.013
			(−0.92)			(1.37)
Adj. R^2	0.001	0.116	0.119	0.018	0.110	0.114
Observations	256	256	256	256	256	256

Panel B: Fund performance: Net returns

	1 year of daily returns (1-3)			3 years of monthly returns (4-6)		
	CAPM	3-Factor	4-Factor	CAPM	3-Factor	4-Factor
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign75	0.001	−0.001	−0.001	−0.006	−0.003	−0.002
	(0.24)	(−0.43)	(−0.37)	(−1.04)	(−0.64)	(−0.41)
Year FE	yes	yes	yes	yes	yes	yes
Segment FE	yes	yes	yes	yes	yes	yes
Adj. R^2	0.055	0.054	0.054	0.048	0.036	0.049
Observations	16,134	16,134	16,134	18,003	18,003	18,003

Panel C: Fund performance: Gross returns

	1 year of daily returns (1-3)			3 years of monthly returns (4-6)		
	CAPM	3-Factor	4-Factor	CAPM	3-Factor	4-Factor
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign75	0.003	0.002	0.003	−0.031	−0.013	−0.005
	(1.06)	(0.82)	(0.92)	(−0.64)	(−0.31)	(−0.12)
Year FE	yes	yes	yes	yes	yes	yes
Segment FE	yes	yes	yes	yes	yes	yes
Adj. R^2	0.052	0.047	0.048	0.111	0.064	0.065
Observations	15,222	15,222	15,222	17,611	17,611	17,611

Panel D: Matched sample 4-factor results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Net returns								
Foreign75	0.000	−0.000	−0.001	−0.000	−0.005	−0.002	−0.000	−0.003
	(0.05)	(−0.14)	(−0.42)	(−0.07)	(−0.99)	(−0.98)	(−0.03)	(−0.83)
Gross returns								
Foreign75	0.004	0.004	0.002	0.000	0.001	0.004	0.006	−0.002
	(1.20)	(1.16)	(0.62)	(0.03)	(0.13)	(1.15)	(1.44)	(−0.56)

(continued)

Taken together, the results from Table 3 suggest that a foreign-sounding name is not associated with lower risk-adjusted performance. Hence, if investors prefer funds with nonforeign-sounding names, "statistical discrimination" is unlikely to drive this behavior.

Table 3
Continued

Panel E: Alternative specifications

	Net return	CAPM	4-Factor	Expense ratio
	(1)	(2)	(3)	(4)
Foreign75	−0.004	0.001	−0.000	0.002
	(−0.38)	(0.37)	(−0.15)	(0.94)
No load	0.001	0.001	0.000	−0.002
	(0.30)	(0.61)	(0.36)	(−2.59)
Expense ratio	0.094	−0.002	−0.002	
	(0.27)	(−0.90)	(−1.09)	
12b-1 fee	−0.014	−0.002	−0.003	
	(−1.97)	(−0.61)	(−0.96)	
Fund size	−0.001	−0.001	−0.000	−0.002
	(−0.53)	(−3.38)	(−1.41)	(−3.96)
Company size	0.002	0.001	0.000	−0.000
	(3.04)	(2.18)	(1.35)	(−6.79)
Turnover	0.008	−0.003	−0.003	0.001
	(2.84)	(−3.58)	(−4.19)	(2.05)
Fund age	−0.004	−0.001	−0.001	0.004
	(−1.93)	(−1.19)	(−2.35)	(2.74)
Lagged fund flow	−0.004	−0.001	−0.000	0.000
	(−3.21)	(−2.37)	(−0.91)	(2.22)
Lagged return measure	0.116	0.035	0.089	−0.013
	(6.60)	(1.76)	(4.33)	(−2.30)
Fund risk	−0.457	−0.389	−0.304	0.123
	(−3.59)	(−8.11)	(−6.45)	(2.27)
Year FE	yes	yes	yes	yes
Segment FE	yes	yes	yes	yes
Adj. R^2	0.612	0.262	0.275	0.100
Observations	10,698	5,710	5,710	14,600

This table presents fund performance estimates. In panel A, we regress the monthly difference in the returns of a portfolio that is long in funds of managers with foreign-sounding names and short in funds of managers with nonforeign-sounding names on the market, SMB, HML, and UMD factors. Panels B and C show the estimates of fund performance, measured using the CAPM alpha (Columns (1) and (4)), the Fama and French 1993 three-factor alpha (Columns (2) and (5)), and its four-factor extension (Columns (3) and (6)). In panel B, performance alphas are obtained using raw returns, while in panel C, we add a fund's expense ratio to its returns before estimating the alphas. In panel D, we run the same performance regressions as in Column (3) of panels B (net) and C (gross), based on the different matched samples as in Table 2, panel B. In panel E, estimates of fund performance are regressed on a foreign fund manager dummy (Foreign75) and a set of fund characteristics. All control variables are lagged by one year and have been defined in Table 1. The t-statistics are displayed in parentheses below the coefficient estimates. The standard errors are clustered at the fund level.

3.2 Foreign-sounding names and the flow-performance relationship

Our evidence so far establishes that, all else equal, fund flows are lower when fund managers have foreign-sounding names. In this section, we gather support for our more specific conjecture, which posits that managers with foreign-sounding names are likely to be "punished" more after bad performance and "rewarded" less after good performance. This conjecture is based on the observation that investors react more strongly to both extreme positive and negative performance than to average performance (e.g., Chevalier and Ellison 1997).

To test our conjecture, we re-estimate the baseline flow regressions (see Table 2) after adding interactions between the Foreign75 dummy variable and measures of past performance. We also include an interaction term between fund size and the Foreign75 variable, as well as fund size and the performance

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Table 4
Flow-performance relationship among foreign funds

	OLS	OLS	OLS	FMB	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Foreign75	−0.077	−0.102	−0.275	−0.248	−0.267	−0.386
	(−1.00)	(−1.64)	(−4.14)	(−3.53)	(−3.97)	(−3.97)
Foreign75 × PRank	−0.329	−0.263	0.813	0.695	0.798	0.902
	(−3.95)	(−3.40)	(2.67)	(1.65)	(2.40)	(2.77)
Foreign75 × PRank^2			−1.132	−0.865	−1.129	−1.183
			(−3.48)	(−1.86)	(−3.08)	(−3.57)
PRank^2			2.054	1.903	2.099	2.407
			(3.27)	(2.28)	(3.29)	(3.71)
PRank	1.210	1.015	−0.883	−0.707	−0.911	−1.202
	(8.29)	(6.87)	(−1.65)	(−1.07)	(−1.67)	(−2.12)
Foreign75 × fund size	0.026	0.027	0.026	0.019	0.026	0.037
	(1.91)	(2.60)	(2.38)	(1.19)	(2.37)	(2.49)
Fund size	−0.019	−0.025	−0.062	−0.061	−0.060	−0.133
	(−1.74)	(−2.26)	(−4.07)	(−4.26)	(−3.91)	(−6.83)
PRank × fund size	−0.090	−0.076	0.164	0.116	0.164	0.206
	(−4.12)	(−3.49)	(1.97)	(1.21)	(1.94)	(2.33)
PRank^2 × fund size			−0.261	−0.222	−0.264	−0.309
			(−2.72)	(−1.88)	(−2.73)	(−3.10)
Controls	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	no	no	yes
Segment FE	yes	yes	yes	yes	no	yes
Segment × year FE	no	no	no	no	yes	no
Fund family FE	no	no	no	no	no	yes
Adj/Avg. R^2	0.091	0.102	0.105	0.175	0.111	0.170
Observations	14,502	13,091	13,091	13,091	13,091	13,086

This table shows the estimates of percentage fund flows regressed on the Foreign75 interacted with lagged performance indicators and fund size. We use the same specifications as in Table 2 and add interaction terms of the Foreign75 with the respective performance and fund size variables. All control variables, except for segment and family flows, are lagged by one year and have been defined in Table 1. The standard errors are clustered at the fund level. The corresponding t-statistics are shown in parentheses below the coefficient estimates.

rank variables. These additional interaction variables allow us to rule out the possibility that we are merely picking up differences in the flow-performance relationship due to fund size rather than to the foreignness of the fund manager name.

The regression results are presented in Table 4. The first two specifications interact the Foreign75 dummy with fund returns, expressed by performance rank (PRank) in the past year. We find a negative and significant interaction effect, whether (specification (2)) or not (specification (1)) we account for lagged fund flows. These estimates indicate that the largest differences in flows are due to lower inflows into funds of managers with foreign-sounding names following good performance.

The linear specification for the interaction will not be appropriate if the true flow-performance relationship is nonlinear. We therefore add squared performance rank to our previous specification to capture the potential nonlinearity. The results presented in Column (3) of Table 4 show that the flow-performance relationship is indeed nonlinear and the difference between
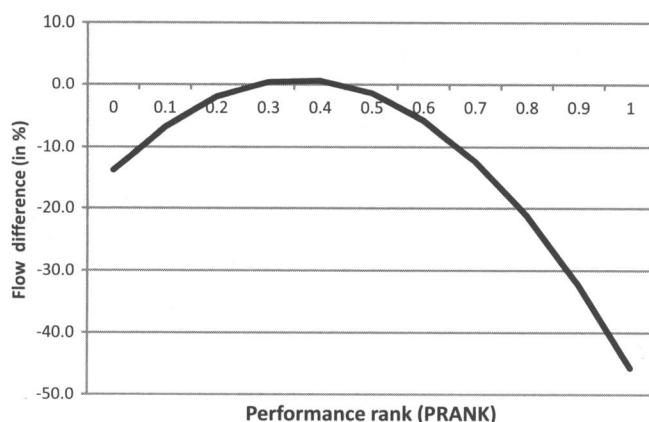
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Figure 2
Differences in fund flow conditional on performance
This figure shows the predicted difference (in percentage points) in fund flow conditional on lagged fund performance between funds with and without managers with foreign-sounding names. The graph is based on specification (3) in Table 4. The negative values indicate lower flows into funds managed by individuals with foreign-sounding names.

foreign and non-foreign-managed funds is more pronounced for the extreme return rank quintiles.

Figure 2 displays the difference in fund flows between foreign and nonforeign funds, conditional on performance ranks implied by the estimates in specification (3). The plot shows that the relation between flow difference and performance has an inverse U shape. This finding is consistent with our hypothesis that, following good performance, investors do not reward managers with foreign-sounding names as much as they reward nonforeign managers through additional inflows, but investors do punish them more severely after bad performance by withdrawing more capital. The economic magnitudes of these effects are large. The fund flow differential due to the foreignness of the manager's name is -14.2% for the lowest performance rank and -46.7% for the most extreme outperformers.

The last three columns in Table 4 present evidence from additional robustness checks. First, we estimate the corresponding flow regressions using the Fama and MacBeth (1973) method. The results are slightly weaker statistically, but their economic significance remains similar. As before, we find that investors react most to the two extreme return deciles, where the estimates imply a 14.4% flow difference for the worst performing funds and 28.2% for the best performing funds. Specification (5) replaces the segment and year fixed effects with segment $\times$ year fixed effects to rule out the possibility that flow dynamics at the segment-level drive our results. Last, specification (6) includes fund family fixed effects. Our findings are largely unaffected by the presence of these additional control variables.

3.3 Evidence from two natural experiments

3.3.1 The 9/11 terrorist attacks. In this section, we use the terrorist attacks of September 11, 2001, as a natural experiment to provide support for a taste-based explanation for our findings. Specifically, we exploit the fact that the attacks plausibly generated an exogenous shock to the negative stereotypes against the subset of the foreign names sample that could be associated with Islam. These are funds managed by individuals with names that could be associated with the Middle East (e.g., Saudi Arabia, Syria, Iraq, or Iran) and South-East Asia (e.g., Afghanistan, India, or Pakistan). For brevity, we refer to these names as "Middle Eastern" names. We test whether flows into funds with Middle-Eastern-named managers decline abnormally after 9/11 as compared with flows into funds managed by other managers with foreign-sounding names.

We conduct the 9/11 tests by manually identifying those manager names that could be perceived as "Middle Eastern." About one-third of managers with foreign-sounding names are identified as "Middle Eastern." As before, our focus here is on the *perception* that a person is Middle Eastern, or associated with Islam, rather than on fact of whether the individual is truly from one of those countries or is a Muslim. We use monthly data to capture the flow shifts around September 2001 and only include control variables with monthly variation in the flow regressions. The overall sample consists of 279,734 monthly fund observations, out of which 5,403 observations (1.93% of the sample) are associated with managers with Middle Eastern names, which correspond to 71 unique Middle Eastern managers in our sample.

Table 5 presents the estimation results. Columns (1) and (3) focus only on the subsample of managers with foreign-sounding names. In these specifications we are comparing Middle-Eastern-named managers (e.g., Zaheer Sitabjhan or Ajit Dayal) with non-Middle-Eastern foreigners (e.g., Klaus Kaldemorgen or Taizo Ishida). The first column shows the results when we regress flows on a Middle Eastern dummy interacted with a dummy that is set to one for all months including and after September 2001. The estimates show that flows for funds managed by individuals with Middle Eastern names experience an abnormal 2.0 percentage point monthly decline after 9/11. This effect is economically large relative to the average 3.1% fund flow for funds with Middle Eastern managers.

It is important to note that we are using only U.S. equity funds in our analysis. Therefore, these findings are inconsistent with an alternative fundamentals-based hypothesis, which posits that managers with Middle Eastern names manage portfolios that invest predominantly in the Middle East and that 9/11 adversely affected the economic outlook for that region. Specification (3) shows that the chance of observing an outflow is 13 percentage points higher for Middle Eastern managers after 9/11. Compared with the average chance of an outflow of 50.6%, this effect is economically significant.

Figure 3 provides additional evidence by presenting average flows for the two groups around September 2001. Consistent with the regression results, flows

Table 5
Fund flow regressions around 9/11 terrorist attacks

	Fund flow		Prob(Outflow)		Family flow	
	Foreign funds	All funds	Foreign funds	All funds	Foreign families	All families
	(1)	(2)	(3)	(4)	(5)	(6)
MidEast	0.011	0.009	−0.065	−0.069	0.015	0.015
	(1.45)	(1.44)	(−1.23)	(−1.51)	(3.63)	(4.12)
MidEast × post-9/11	−0.020	−0.018	0.133	0.097	−0.020	−0.013
	(−2.47)	(−2.69)	(2.28)	(2.07)	(−4.32)	(−3.10)
Non-MidEast-Foreigner		−0.006		0.023		0.002
		(−2.33)		(0.86)		(0.86)
Non-MidEast-Foreigner × post-9/11		0.008		−0.070		0.003
		(1.95)		(−1.90)		(1.25)
PRank	0.026	0.011	−0.212	−0.223	0.070	0.009
	(1.86)	(3.03)	(−1.88)	(−8.92)	(4.18)	(1.73)
PRank^2	−0.007	0.012	0.079	0.072	−0.053	0.009
	(−0.50)	(3.30)	(0.73)	(2.91)	(−3.29)	(1.82)
Size	−0.004	−0.005	0.028	0.022	−0.002	−0.003
	(−5.23)	(−21.99)	(3.71)	(13.08)	(−5.44)	(−19.45)
Segment flow	0.066	0.048	−0.334	−0.241		
	(1.47)	(4.88)	(−2.04)	(−5.87)		
Family flow	0.084	0.084	−0.417	−0.397	0.051	0.143
	(5.16)	(15.67)	(−5.27)	(−21.78)	(2.88)	(20.05)
Time FE	yes	yes	yes	yes	yes	yes
Segment FE	yes	yes	yes	yes	no	no
Adj. R^2	0.046	0.034	0.059	0.050	0.026	0.052
Observations	12,704	279,734	12,704	279,734	8,699	77,305

This table reports estimates from flow-related regressions around the 9/11 terrorist attacks. The dependent variable in specifications (1) and (2) is Fund Flow, defined as the net inflow into the fund in the current month: $(TNA_{i,m} - TNA_{i,m-1})/TNA_{i,m-1} - r_{i,m}$, where $TNA_{i,m}$ denotes fund i's total net assets in month m and r_m denotes the return of fund i in month m, as reported in CRSP. In specifications (3) and (4), the dependent variable is a dummy variable that is equal to one if the monthly flow is negative. The MidEast dummy is one for managers with a Middle Eastern name. Post-9/11 is a dummy variable that takes the value of one for September 2001 and all subsequent months ending in December 2011, and zero otherwise. Specifications (1) and (3) look only within funds of managers with foreign-sounding names. Specifications (2) and (4) use the full sample and include MidEast managers, Non-MidEast foreigners, and managers with nonforeign-sounding names (the omitted category). All other variables are lagged by one year and have been defined in Table 1. Specifications (5) and (6) repeat the analysis of Columns (1) and (2) at the fund company level. Correspondingly, in Columns (5) and (6), PRank (based on a fund company's value-weighted return), PRank^2, and Size are aggregated at the monthly fund company-level. The standard errors are clustered at the fund-level in Columns (1) to (4) and at the fund company level in Columns (5) and (6). The corresponding t-statistics are shown in parentheses below the coefficient estimates.

into funds managed by individuals with Middle Eastern names are substantially lower starting in September 2001, while the flows into non-Middle-Eastern foreign funds are essentially unaffected. The month of September 2001 is the only month in the five months surrounding it in which there were actual outflows from funds with Middle-Eastern-named managers. By contrast, this was never the case for funds managed by managers whose names sounded foreign, but not Middle Eastern.[7]

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[7] Our full sample results are not driven by September 2001 alone. In fact, our results are most consistent with a permanent negative effect on the perceptions of Middle Eastern foreigners. This finding is consistent with survey evidence from the Gallup poll as reported by Margulies (2013), suggesting that U.S. citizens have a progressively less favorable view of Islam from 2001 until today.
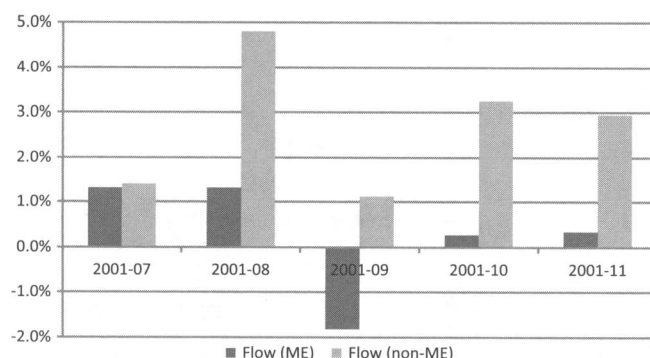
Figure 3
Monthly fund flows around terrorist attacks in September 2001
This figure shows the average monthly net inflows into funds that are managed by individuals with Middle-Eastern-sounding names (Flow (ME)) and net inflows into funds managed by individuals with other foreign-sounding names (Flow (NME)). The fund flows are measured at the last day of each month.

While the above tests focus on differences within the set of foreign-named managers, specifications (2) and (4) use the full sample of managers. In these tests, we compare the two sets of foreigners (Middle Eastern and non-Middle-Eastern) to nonforeigners before and after 9/11. Our results show that Middle Eastern named managers experience a 1.8 percentage point decline in flows relative to nonforeigners and that the chance of seeing an outflow increases by 9.7 percentage points following 9/11. The evidence also indicates that non-Middle-Eastern foreigners experienced the opposite pattern, supporting our underlying conjecture that 9/11 had a differential impact on how names associated with the Middle East and other foreign countries were perceived.

Specifications (5) and (6) repeat the analysis at the fund company level. Consistent with the fund-level evidence, we find that fund companies that employ at least one Middle Eastern foreigner experience abnormal outflows following 9/11, relative to other fund families. Taken together, the results from our 9/11 tests show that after an exogenous event that likely reinforced negative stereotypes toward individuals with Middle Eastern names, funds managed by those individuals experience an abnormal decrease in fund flows. This evidence is hard to reconcile with a fundamentals-based explanation and provides support for the view that fund flows are affected by taste-based discrimination.

3.3.2 The 2004 disclosure rule change.   Our next test uses data on foreign-named managers in team-managed funds. We exploit the introduction of the Securities and Exchange Commission (SEC) rule that required mutual funds to report individual fund manager names for team-managed funds starting in October 2004 (SEC release 33-8458). Before this regulatory change, fund companies could classify individual funds as "team managed" or "anonymous" instead of providing the individual fund manager names in a team. If at least some fund management companies strategically put foreign-named managers
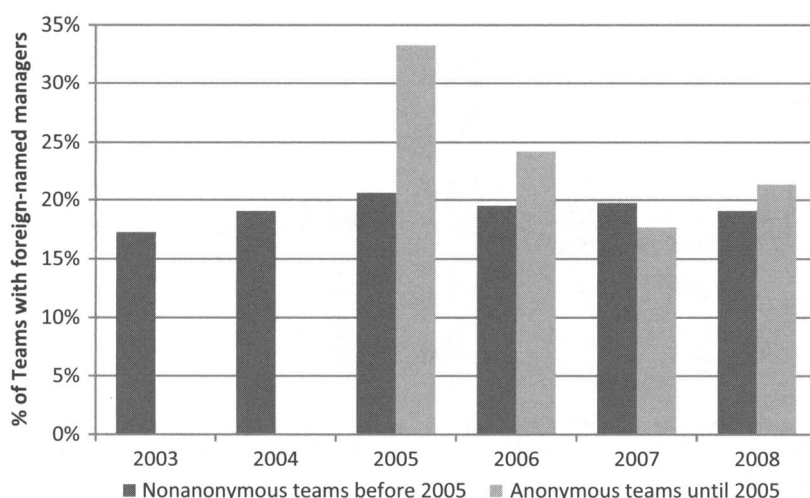
Figure 4
Foreign-named managers in teams, 2003 to 2008
This figure shows the fraction of teams that have at least one manager with a foreign-sounding name. Teams that voluntarily reported team members before 2005 (dark bars) and teams that did not disclose team-member names before 2005 (light bars) are reported separately.

into teams to avoid lower flows, then, following the regulatory change, we should observe a higher fraction of managers with foreign-sounding names in the group of team-managed funds that previously chose not to disclose fund manager names, compared with the group of team-managed funds that voluntarily chose to disclose fund manager information even before the regulatory change. Thus, following the regulatory change in SEC file N1-A in October 2004, we should observe a higher fraction of managers with foreign-sounding names in funds that were previously anonymous and team managed.

Figure 4 shows that foreign-named managers are indeed much more likely to be in team-managed funds that were managed anonymously before 2005 than in team-managed funds that disclosed the identity of all team members before 2005. This evidence is consistent with the conjecture that mutual fund companies strategically assigned managers with foreign-sounding names to anonymous team-managed funds. The difference between the two proportions in 2005 is twelve percentage points, which is statistically significant at the 10% level. Importantly, the number of foreign-named managers in anonymous teams drops after 2005 and converges toward the proportion of foreign-named managers in team-managed funds with disclosed team members. This finding suggests that the observed difference between the proportion of managers with foreign-sounding names in previously anonymous and nonanonymous team-managed funds in 2005 is not due to any unobserved variable that generates a stable difference in the incidence of foreign-named managers between the two groups of funds.

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Because disclosure altered the information set of investors, who now observe when a foreign-named manager is in a team, the above results are consistent with the hypothesis that disclosure increased the cost of employing a foreign-named manager and that fund management companies were responding to this change by hiring foreign-named managers less frequently. The results therefore are consistent with the existence of taste-based discrimination among mutual fund investors.

3.4 Evidence based on fund headquarters' locations

In this section, we exploit the well-documented local bias in equity investments among both institutions and retail investors (e.g., Coval and Moskowitz 1999; Coval and Moskowitz 2001; Ivković and Weisbenner 2005), especially the local bias of retail investors in their mutual fund investments (Bailey, Kumar, and Ng 2011). Given the existence of fund-level local bias, we conjecture that the foreignness-fund flow relationship will be more pronounced for funds headquartered in regions in which social biases are stronger.

To conduct these tests, we use CRSP to obtain data on the headquarters location for all mutual funds in our sample. Our econometric approach is to augment the baseline specification in Column (3) of Table 2, panel A, with an interaction term between the Foreign75 indicator and regional characteristics. We cluster standard errors by ZIP code in all regressions. The estimates from these extended regression specifications are presented in Table 6.

In the first set of tests, we examine whether the strength of the foreignness-fund flow relationship correlates with measures of demographic characteristics that are likely to be associated with the degree of suspicion toward outsiders. We start by looking at more rural areas and regions with higher average age, as investors in those regions may on average hold less favorable views about fund managers with foreign-sounding names (e.g., Massey 2010). We obtain data on the fraction of the county population that lives in rural areas from the U.S. Census and define a dummy variable that is set to one for counties with above-median rural population in our sample, and zero otherwise. We define a similar dummy variable using county-level age.

The results reported in specification (1) of Table 6 show that the coefficient estimate of the Foreign75 and the rural interaction term is negative, indicating that fund flows are lower for foreign funds when these funds are located in rural areas. Similarly, specification (2) shows that funds located in counties with above-median average age have lower flows when they are managed by individuals with foreign-sounding names than otherwise identical funds managed by individuals with more familiar American names.

Next, we use two religion-based measures. We first obtain data on church attendance from the General Social Survey (GSS) and define a Church Attendance dummy variable that is set to one for regions in which the share of residents who attend church at least once a week is higher than the sample median. The second data source is the American Religious Data

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Table 6
Regional characteristics around fund headquarters and fund flows

Panel A: Fund location and fund flows

	Rural area	Old population	Church attendance	PCRATIO	Stereotypes
	(1)	(2)	(3)	(4)	(5)
Foreign75	−0.044	−0.045	−0.036	−0.023	0.058
	(−1.10)	(−1.60)	(−0.99)	(−0.57)	(0.69)
Foreign75 × location variable	−0.085	−0.097	−0.105	−0.107	−0.176
	(−1.81)	(−2.50)	(−2.21)	(−2.34)	(−1.99)
Location variable	0.075	0.012	0.029	−0.016	0.084
	(2.12)	(0.44)	(0.92)	(−0.38)	(2.42)
Controls	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Segment FE	yes	yes	yes	yes	yes
Adj. R^2	0.096	0.096	0.106	0.096	0.094
Observations	14,099	14,099	10,582	14,099	12,897

Panel B: Fund location and incidence of foreign-named funds

	Rural area	Old population	Church attendance	PCRATIO	Stereotypes
	(1)	(2)	(3)	(4)	(5)
Location variable	−0.421	−0.036	−0.042	−0.143	−0.432
	(−4.31)	(−2.38)	(−0.48)	(−5.65)	(−6.08)
Controls	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Segment FE	yes	yes	yes	yes	yes
Pseudo R^2	0.021	0.020	0.016	0.023	0.025
Observations	20,596	20,596	15,761	20,596	18,678

This table shows fund flows regressed on the Foreign75 indicator interacted with regional characteristics at the fund company's headquarters location in panel A and logit regressions with the Foreign75 indicator as the dependent variable in panel B. Rural Area is the proportion of the county population that lives in rural areas. Old Population is a dummy variable equal to one if the average household age in the county of the fund headquarters is larger than the median age across all counties in our sample. Church Attendance is a dummy equal to one if the share of residents in a state who go to church at least once a week based on the General Social Survey is higher than the sample median. PCRATIO is one if the ratio of Protestants to Catholics in a county is above the sample median. Stereotypes is a state-level dummy variable for the prevalence of stereotypes of African Americans and Asian Americans as defined in the Appendix. We use the same set of control variables as in our baseline specification in panel A of Table 2, column (3). In panel B, we control for fund age, fund size, expense ratio, and fund company size (logarithm of a fund company's assets under management). In both panels we control also for the fraction of minority (none white) households in the county in which the fund is headquartered and a top10 MSA dummy equal to one for funds headquartered in one of the ten largest MSAs (New York, Los Angeles, Chicago, Dallas, Houston, Philadelphia, Washington, Miami, Atlanta, and Boston). The standard errors are clustered at the ZIP code level. t-statistics are displayed in parentheses below the coefficient estimates.

Archive (ARDA), which provides county-level data on the fraction of the population that adheres to Protestant and Catholic faiths.[8] The former variable is motivated by Allport and Ross (1967), who suggest a positive link between churchgoing and prejudice against ethnic minorities.[9] The latter variable is motivated by evidence of Kumar, Page, and Spalt (2011), who show that

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[8] Since both the GSS and the ARDA denominations surveys are not conducted every year, we extrapolate the yearly values between the different survey waves before matching it to our mutual fund dataset.

[9] More specifically, their findings suggest that the overall positive relation between churchgoing and strength of ethnic prejudice is driven mainly by to those churchgoers with an *extrinsic motivation*, that is individuals "disposed to use religion for their own ends." Their evidence also suggests that *intrinsically motivated* churchgoers, that is individuals who "find their master motive in religion," are often less prejudiced.

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investors in Protestant dominated areas are less likely than Catholics to engage in speculative trading activities.

If investing in a fund managed by an individual with an unfamiliar name is perceived to be riskier, a higher ratio of Protestants to Catholics in a county (we label this ratio PCRATIO) would predict greater reluctance to invest in such funds. The results in specifications (3) and (4) show that foreign funds have lower flows when they are located in areas in which church attendance is higher or the local tendency to speculate is lower.

In the final location-based test, we use a more direct measure of racial and ethnic stereotypes in a region. The 2008 wave of the American National Election Studies (ANES) asks a representative sample of the U.S. population about whether or not they rate Whites, African Americans, or Asian Americans as intelligent on a scale between one (intelligent) and seven (unintelligent). We construct a state-level stereotype measure by dividing the average rating assigned to African Americans and Asian Americans by the average number assigned to Whites. This yields a state-level measure of negative stereotypes against non-Whites by the local population, which should proxy for the tendency of the local population to shy away from managers with foreign-sounding names.

Our evidence indicates that funds managed by individuals with foreign-sounding names have flows that are about 12 percentage points lower when stereotypes among the local investor population are more pronounced by the ANES measure. As that measure quite directly captures local stereotypes through surveys of the local population, these results support our interpretation that the observed flow differences between managers with (non-)foreign-sounding names are likely to be due to in-group bias and taste-based discrimination.

The results in panel B of Table 6 provide additional evidence for strategic behavior by fund management companies. Here, we estimate the impact of regional variables plausibly associated with greater discrimination tendencies on the probability of having a foreign fund in this region. Our results show that managers with foreign-sounding names are less likely to be employed by fund companies located in rural regions, in regions with relatively older residents, in regions with higher church attendance, in regions with higher PCRATIOs, and in regions in which surveys indicate individuals exhibit greater discrimination tendencies against non-Whites on average.

Overall, the results from our location-based tests are consistent with the view that the differences in fund flows are driven by taste-based discrimination of mutual fund investors.[10]

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[10] One potential alternative interpretation of our location-based results is that the geographical patterns we observe reflect taste-based discrimination at the fund management company level. Such a management company discrimination channel, if it exists, would further strengthen our argument that flow differences are induced by taste-based discrimination and complement the investor channel. However, because the flow results are robust to including fund family fixed effects, it is not entirely obvious how an alternative channel would operate.

4. Microlevel Evidence of Taste-Based Discrimination

Our results so far are consistent with taste-based discrimination by U.S. mutual fund investors. In this section, we use microlevel data from online surveys and experiments, as well as direct holdings of a sample of U.S. brokerage investors, to further examine this conjecture.

4.1 Sensitivity to fund manager names: Survey evidence

Our first online survey is designed to examine whether mutual fund investors are aware of their fund managers at the time they select their funds. Investors are often directly exposed to manager names when they search for product descriptions on fund web sites. Thus, it is quite possible that investors are aware of fund manager names at the time they choose the funds. We now provide supporting survey evidence.

We survey 100 individuals on the AMT platform and find that 64% of the survey respondents think that it is important to know their fund manager, and 57% said they actually knew the identities of the fund managers when making their fund choices. Only 13% of respondents say that the fund manager is of little importance or that they seldom or never read about the fund manager. This evidence indicates that many investors are keen to know about their fund managers and suggests that the identity of the fund manager may be an important determinant of their decision to buy a fund. Our finding is consistent with the findings of Massa, Reuter, and Zitzewitz (2010), who show that funds with named managers have greater inflows than funds with anonymous management, and with those of Choi, Kahraman, and Mukherjee (2013), who show that there are flow spillovers within the funds of the same manager. These mechanisms can only work if the name of the manager is relevant to at least some investors.

It is important to note that we are not trying to determine whether investors currently know the name of the fund manager. Most investors would probably not recall the fund manager names for most of their funds, just as most investors would not recall the exact expense ratios or other key attributes of the funds they have invested in. This issue is not important in our setting. What is important is whether investors look at the names of fund managers at the time they make their investment decisions, which is what we ask directly in our survey questions.

Collectively, the online survey results are consistent with our key assumption that a substantial fraction of investors is likely to be sensitive to the names of fund managers when they make their investment decisions.

4.2 Evidence from online experiments

In the next microlevel test, we conduct online experiments to identify the causal impact of the foreignness of fund manager names on the investment choices of fund investors. We again recruit individuals at Amazon Mechanical Turk to complete a hypothetical fund investment task in which they are required to split an investment of 100 dollars between two S&P500 index funds, which we label "fund A" and "fund B."

4.2.1 Experimental evidence using randomly assigned names. Our first fund investment experiment is run on a total of 400 individuals.[11] For each index fund A and B, we provide subjects with information about fund segment, size, inception date, expense ratio, annual turnover, a short description of the investment objective, and the top five holdings. Both funds have identical or very similar fund attributes and those remain unchanged throughout the experiment. The key feature of the experiment is that half of the individuals observe that fund A is managed by "Mustafa Sagun," whereas fund B is managed by "William R. Andersen." The other half observes that fund A is managed by "William R. Andersen," whereas fund B is managed by "Mustafa Sagun."

We randomly assign fund manager names to funds and funds to individuals. Hence, finding lower investments to a given fund when the fund manager has a foreign-sounding name strongly provides evidence for a causal link between fund manager names and fund flows. Since statistical discrimination in this experimental setting is not very plausible, such results would support a taste-based discrimination channel.

Table 7, panel A, presents the results of this experiment. Column (1) shows that AMT workers allocate 10.8 dollars less to the index fund whose manager has a foreign-sounding name. This result gets even stronger when we exclude from our sample AMT workers who spent less than one (Column 2) or two minutes (Column 3) on the experiment, with a difference of 15.4 dollars in the latter case. Spending little time on the task could indicate that the workers did not carefully look at all the available information. These findings show that our results are not spuriously induced by AMT workers who did not take the task seriously. In Column (4), we use several interaction terms and show that our results are stronger among older AMT workers.

Overall, across all specifications, we find that the effect of a name on the investment amount allocated to a fund is economically meaningful and—interestingly—similar in magnitude to the effects we document in our CRSP/Morningstar sample. Because names are randomly assigned to AMT workers, these results suggest a causal impact of the foreignness of fund manager names on fund flows.

4.2.2 A placebo test. To ensure that our experimental results are robust, we perform a placebo test and rerun the index fund experiment with 100 workers using "Thomas Smith" as the name of the second fund manager instead of "Mustafa Sagun."

Consistent with our taste-based discrimination conjecture, in untabulated results, we find that investment choices do not depend on fund manager names when both funds are managed by individuals with typical American names. This

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[11] The experiment was conducted in three different batches of 100, 100, and 200 individuals. The first group was contacted in April 2012, and the second and third groups were surveyed in April 2013.

Table 7
Fund allocation regression estimates using experimental data

Panel A: Mustafa Sagun versus William R. Andersen

	Full sample	Work time > 1 min	Work time > 2 mins	Inter- actions
	(1)	(2)	(3)	(4)
Fund manager characteristic				
Foreign fund	−10.840	−13.111	−15.444	−6.681
	(−4.12)	(−4.56)	(−3.90)	(−1.61)
Interactions				
Female × foreign fund				−5.861
				(−1.07)
Old investor × foreign fund				−9.596
				(−1.71)
MF investor × foreign fund				6.687
				(1.19)
Controls	yes	yes	yes	yes
Time FE	yes	yes	yes	yes
Adj. R^2	0.057	0.084	0.091	0.069
Observations	397	323	160	397

Panel B: Changes around Boston bombings

	Full sample	Work time > 1 min	Work time > 2 mins	First time
	(1)	(2)	(3)	(4)
After bombings× foreign	−10.292	−15.030	−17.029	−17.099
	(−2.61)	(−3.18)	(−2.27)	(−3.02)
Before bombings× foreign	−9.721	−10.661	−12.618	−10.117
	(−2.01)	(−2.12)	(−1.84)	(−2.04)
Controls	yes	yes	yes	yes
Adj. R^2	0.033	0.074	0.083	0.078
Observations	297	225	94	194

This table shows estimates of experimental monetary units allocated to an index fund regressed on fund characteristics and Amazon Mechanical Turk (AMT) worker characteristics. The dependent variable is the fraction of 100 monetary units that AMT workers allocated to a randomly assigned S&P500 index fund in an online experiment. Foreign Fund is a dummy variable equal to one if the manager name of the index fund is "Mustafa Sagun" and zero if the manager name of the otherwise identical fund is "William R. Andersen." The results in panel A are based on all experimental observations. Control variables are gender, a dummy for old investors (above median age), a dummy variable equal to one if the AMT worker indicated that she invested in a mutual fund before. Column (1) presents results for the full sample of AMT workers. Columns (2) and (3) exclude AMT workers who completed the experiment in less than one and two minutes, respectively. Results in panel B are based on two experiments run around the Boston bombings in April 2013. "Before" ("After") is a dummy equal to one for the sample of AMT workers that completed the task after the bombings but before (after) the FBI released photographs of the suspects. The after dummy is included as an additional control variable in panel B. t-statistics are reported in parentheses below the coefficient estimates.

evidence from the placebo test provides additional support for our conjecture that fund manager names would affect the investment decisions of mutual fund investors.

4.2.3 Experimental evidence from the Boston bombings. In our next experimental test, we use the terror attacks during the Boston marathon on April 15, 2013, as an exogenous source of amplification of discriminatory tendencies against foreigners. Specifically, we conducted the fund investment experiment with 100 individuals on April 18, 2013, that is immediately after the bombings took place, but before there was any public announcement as to who could be

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potential suspects.[12] Then, after it was revealed that the attack was most likely related to Islamic fundamentalists, we conducted the same experiment on April 23 and 26 with 100 individuals each. As in our 9/11 tests already described, we conjectured that, due to its association with Islam, the event would amplify discriminatory tendencies against foreign names, such as "Mustafa Sagun."

The results reported in Table 7, panel B, show that this conjecture is supported by our experimental data. While the "before" group invests less in a fund if the manager's name sounded foreign, the "after" group, which had just learned of the Islamic background of the Boston bombings suspects, shows an even stronger tendency to tilt investment away from the manager with a foreign-sounding name. Examining the estimates in specifications (2) and (3), we find that the level of underinvestment in the foreign fund increases by 50% and 42%, respectively.

The final specification considers only individuals who are not in both the "after" and "before" groups. We want to guard against the potential concern that the money allocation of the first experiment would influence the allocation in the second experiment, which involved the same task and was done only a week later. We find that the difference in the investment allocation to the fund with the non-foreign-named manager increases even more among individuals for whom this anchoring effect is absent. While our sample sizes are too small to find strong statistical significance for the difference between the coefficients, we nevertheless interpret the findings in panel B as supportive of our taste-based discrimination hypothesis. An exogenous event that amplifies negative stereotypes against foreign names induces substantial shifts in investment behavior.

Overall, these experimental results provide further evidence of a causal impact of the foreignness of fund manager names on investment decisions of fund investors.

4.3 Evidence using investor-level data

In this section, we analyze whether the foreignness of fund manager names influences the mutual fund investment decisions of retail investors. We obtain a sample of brokerage customers from a large U.S. discount brokerage house, allowing us to directly examine their investment choices.[13] Specifically, we observe the fund-level investment allocations of each household at the end of each month. Unlike the CRSP mutual fund database that only allows us to observe net flows, the brokerage data allow us to separate inflows (the amount of

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[12] The FBI released photographs of the suspects at 5:00 p.m. EDT on April 18. Source: http://www.fbi.gov/boston/press-releases/2013/remarks-of-special-agent-in-charge-richard-deslauriers-at-press-conference-on-bombing-investigation-1. All subjects in this group completed their AMT task well before 5:00 p.m. EDT on April 18.

[13] Additional details about the individual investor database are available from Barber and Odean (Barber and Odean 2000, Barber and Odean 2001). See Bailey, Kumar, and Ng (2011) for additional information about the mutual funds sample.

fund shares a household bought in a particular quarter) and outflows (the amount sold). While the brokerage sample overlaps with our CRSP/Morningstar data for only a short time period from 1993 to 1996, it provides microlevel evidence consistent with our main taste-based discrimination hypothesis and complements the results obtained using aggregate fund flows.

In the investor-level tests, we have to use a slightly different definition of foreignness of fund manager names since only very few funds in this sample are managed by a manager with a foreign-sounding name. We classify a fund as foreign-managed if at least half of AMT workers identify the manager name as foreign sounding, and denote the associated dummy variable as Foreign50. With this classification, 12.29% of fund managers and 7.35% of fund-year observations are identified as foreign.[14] We control for several fund characteristics, such as the fund's return, size, turnover, risk, expense ratio, and age. These variables are defined as in the previous regressions. We cluster standard errors at the ZIP code level.

The inflow and outflow regression results are presented in Table 8, panel A. The evidence reported in Column (1) suggests that households invest significantly less in a mutual fund if the fund is managed by an individual with a foreign-sounding name. This result is also economically significant. Foreign-managed funds receive about $e^{5.982} - e^{(5.982-0.725)} = \$204$ lower inflows per household per quarter. Relative to the mean inflow of $e^{5.982} = \$396$, this reflects a difference of about 52%. We also find a significant impact of foreign fund manager names on a household's decision to withdraw money from a mutual fund (see Column (2)).

Last, in Column (3), we aggregate inflows and outflows into a mutual fund as net flows and find that net flows are lower if a fund is managed by a manager with a foreign-sounding name. These results, based on the brokerage data, jointly indicate that, all else equal, retail investors shy away from mutual funds that are managed by individuals with foreign-sounding names.

Next, we investigate whether certain household characteristics influence the sensitivity of mutual fund investment choices to foreignness of fund manager names. The goal of this exercise is to determine whether investor attributes that might be associated with the degree of suspicion toward outsiders and in-group bias more strongly affect the their mutual fund choices. Specifically, for each household, we focus on the subset of foreign funds and analyze the level of flow into those funds. Table 8, panel B, presents the regression results. The set of

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[14] If we require 75% of all workers to define a fund as foreign, as in our main flow-based tests, only 1.64% fund managers and 0.85% of all observations in the brokerage sample are associated with foreign funds. Even with this small sample of observations associated with managers with foreign-sounding names, we find that the coefficient estimates of the Foreign75 variable in Table 8, panel A, remain significant. The Foreign75 coefficient estimates in the inflow, outflow, and net flow columns are -0.802 (t-statistic $= -4.64$), 1.312 (t-statistic $= 6.82$), and -1.677 (t-statistic $= -3.07$), respectively. In panel B, the main coefficient estimates have similar signs, but they lose their statistical significance.

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Table 8
Investor-level regression estimates

Panel A: Flows into funds by foreign-name managers

	Ln(inflows)	Ln(outflows)	Net flow
Foreign50	−0.725	0.668	−1.944
	(−11.23)	(9.22)	(−9.19)
Controls	yes	yes	yes
Year FE	yes	yes	yes
Segment FE	yes	yes	yes
Adj. R^2	0.051	0.026	0.021
Observations	48,568	48,568	48,568

Panel B: Foreign fund inflows and retail investor characteristics

	(1)	(2)	(3)	(4)	(5)
HHAge	0.003				0.006
	(0.34)				(0.82)
HHEducation		−0.005			−0.005
		(−0.93)			(−0.73)
HHMale			−0.598		−0.714
			(−3.05)		(−3.01)
HHForeign				0.015	0.020
				(2.06)	(2.24)
Additional controls	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Segment FE	yes	yes	yes	yes	yes
Adj. R^2	0.220	0.239	0.237	0.240	0.230
Observations	2,094	3,355	3,143	3,355	1,990

This table presents estimates from investor-level regressions. Foreign50 is a dummy variable that is equal to one if at least 50% of AMT workers mark the name of a fund manager as foreign sounding. Panel A regresses flow measures on Foreign50 and controls. Inflows (outflows) are buys (sells) of fund shares aggregated by household, fund, and quarter, in the brokerage dataset. Net Flow is defined as Inflow minus Outflow. Panel B regresses inflows into foreign funds on HHAge, HHEducation, HHMale, and HHForeign, which are average demographic characteristics on the household level. Control variables in both panels are fund size, fund age, fund turnover, expense ratio, lagged fund return, fund risk, and time and segment fixed effects. The standard errors are clustered at the ZIP code level. The corresponding t-statistics (z-statistics) are reported in parentheses below the coefficient estimates.

control variables used in these regressions is identical to those used previously in the flow regressions, and standard errors are clustered at the ZIP code level.

We find that male investors invest significantly less in foreign funds than do female investors. We also find a significantly positive coefficient on foreignness of households; that is, households with members more likely to be born outside of the United States invest significantly more in foreign mutual funds than do other households. To the extent that investors in these regions are themselves more likely to be foreign, this evidence is consistent with the existence of in-group bias among individual investors. Alternatively, this evidence may indicate that investors who are more exposed to foreign-born individuals exhibit lower sensitivity to the foreignness of manager names. These estimates remain significant when we include all household characteristics in one regression (see Column (5)).[15]

In sum, the results using investor-level data are consistent with taste-based discrimination. Our evidence using the brokerage sample alone is not

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[15] We find similar results when we estimate the probability of investing in a foreign fund.

very strong due to the short sample period and various data approximations. Nevertheless, these results are informative because we can directly show that the decisions of retail investors are influenced by the foreignness of funds and that they are likely to exhibit in-group bias.

5. Potential Equilibrium Outcomes

Our empirical results suggest that fund management companies are likely to incur considerable direct costs when they employ foreign-named managers because those managers are associated with lower fund flows. This finding raises an intriguing equilibrium question: why do some fund companies employ foreign-named managers in the first place? In this section, we discuss several possible equilibrium outcomes.

5.1 Potential cost of discrimination: Preliminary estimates

Most mutual fund advisory contracts are based on assets under management, so lower flows translate directly into lower fee income. To get some sense of the potential economic consequences of discrimination, we obtain rough estimates of "gross" costs of taste-based discrimination. We explicitly consider gross costs, because the "net" costs, that is the net effect for mutual fund companies after taking into account potential benefits from employing foreign-named managers, would depend upon several other factors, many of which we cannot observe.

Our rough estimates of discrimination-induced gross costs are based on the "marginal compensation rate" of 0.767% across all equity funds (see Deli 2002). This number means that for every additional $100 million in fund assets, the fund advisor (e.g., Fidelity) receives on average an additional $767,000 in fees. Given these numbers, our baseline estimates in Table 2, specification (3), indicate that the difference in fund flows between foreign and other funds translates into $133,386 (= 0.089 × 195.4 million × 0.767%) lower fees per fund and year if a manager has a foreign-sounding name. This calculation assumes that the fund size is equal to the mean fund size in the sample (= $195.4 million).

The flow-performance regressions in Table 4 allow us to take a closer look at how these cost estimates change with the level of fund performance. Because the differentials in flows are greatest for funds that have either very good or particularly poor performance, the actual costs for these funds are larger. In contrast, for a fund with an average performance, the costs are economically small. Table 9 presents estimates of costs associated with managers with foreign-sounding names, conditional on their performance levels. The cost estimates are obtained as follows. First, we calculate the flow differential for various levels of performance from specification (3) in Table 4. Second, we multiply this difference by $195.4 million, which is the average size of funds in our sample, and subsequently by the marginal compensation rate of 0.767%.

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Table 9
Rough estimates of the gross cost of discrimination

Performance rank	Δ Flow (%)	Estimated cost ($)
0.00	−14.24	−213,407
0.10	−6.91	−103,586
0.20	−1.93	−28,894
0.30	0.71	10,670
0.40	1.01	15,106
0.50	−1.04	−15,586
0.60	−5.43	−81,406
0.70	−12.17	−182,355
0.80	−21.25	−318,432
0.90	−32.67	−489,637
1.00	−46.44	−695,971

This table provides estimates of costs incurred by the mutual fund company due to lower flows into funds that have managers with foreign-sounding names. These estimates are based on the implied flow difference (Δ Flow) from specification (3), Table 4. We assume that the fund size equals $195.4 million, which is the average fund size in our sample. The calculations are based on the estimate of 0.767% for the marginal compensation rate in Deli (2002).

As shown in Table 9, for a foreign fund with median performance level, there would be almost no economic cost of discrimination because our nonlinear estimates suggest virtually no difference in fund flows between foreign and nonforeign funds. For the worst-performing funds the cost increases to $213,407 because the difference in flows increases. Similarly, among the best performing funds, the economic cost is substantial. For example, a fund at the 80th percentile of the performance distribution would experience $318,432 higher advisory fees, whereas for the extreme outperformers this number can amount to almost $700,000. All these estimates are based on the mean fund size in our sample, and for larger funds, these differentials would widen further.

Since we do not directly observe advisory contracts, the above cost estimates are necessarily rough and must be interpreted with caution. Nevertheless, they suggest that name-induced biases are likely to generate economically meaningful gross costs for mutual fund companies that employ foreign-named fund managers.

5.2 Potential benefits of hiring foreign-named managers

In light of these economically significant costs of discrimination, what could be the potential benefits that induce some fund companies to continue to employ foreign-named managers? In this section, we investigate this issue. While we cannot fully quantify the economic benefits, we are able to rule out three plausible equilibrium mechanisms.

One possible reason that fund management companies employ managers with foreign-sounding names is that they are simply not aware of the bias we uncover in this paper. While this is possible for some funds, the results in Section 3.3.2 indicate that many fund management companies act strategically. They were more likely to assign foreign-named managers to teams before the disclosure rule change, suggesting that fund companies were potentially aware of the relationship between flows and names.

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A second possible equilibrium outcome could be that fund management companies recognize that investors are biased against foreign-named managers and they only recruit foreign-named managers who are on average better than other managers. However, when we examine performance differences in Table 3, we do not find evidence to support this conjecture. Managers with foreign-sounding names do not exhibit superior abilities. Further, since we control for past performance in our flow regressions, our results are unlikely to reflect skill differences between foreign and domestic managers.

A third alternative equilibrium scenario is one in which foreign-named managers have special skills to manage specialized funds, such as funds that invest in South Asia or the Middle East. While the data do show that foreign-named managers are more likely to work for "regional," "global," or "emerging markets" funds, our results cannot be due to this kind of specialization because we conduct all of our main tests using a sample of funds that primarily invest in U.S. equities.

What are other potential benefits of having a manager with a foreign-sounding name? We believe that there are a number of plausible candidates, but these mechanisms are harder to establish empirically. For example, foreign-named managers might still be hired if fund companies can pay them less. Unfortunately, because we lack detailed compensation data for fund managers, we cannot directly test this conjecture. Another possibility is that mutual fund companies hire non-White foreign managers (who would often be managers with foreign-sounding names) because they are afraid of potential lawsuits claiming discriminatory hiring policies. The fund companies would then trade off the expected cost of a lawsuit with the expected loss in fees due to the lower flows, and we might see some managers with foreign names in equilibrium.[16]

Alternatively, mutual fund companies might be able to attract better employees in the future if they credibly signal that they are a nondiscriminating employer, which seems particularly relevant in light of increasing numbers of university graduates of foreign origin. Further, mutual fund companies might be able to attract additional business by showing that they are a diverse employer.[17] They would then trade off the expected value of additional business with the expected loss in fees due to the lower flows.

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[16] The threat of a lawsuit is more than a theoretical possibility. For example, in 2008, Texas-based American State Bank agreed to pay $225,000 to settle allegations of the Department of Labor that the bank engaged in hiring discrimination against minority applicants. American State Bank claims that the Department of Labor's decision was based solely on a "statistical shortfall in hiring of minority applicants for office and clerical positions" even though "[...] no employee, applicant, or any other person actually complained of discrimination or wrongdoing by American State Bank." Source: http://www.kcbd.com/Global/story.asp?S=8373627&nav=menu69_1_4.

[17] As an example, the Dodd-Frank Wall Street Reform and Consumer Protection Act (P.L. 111-203, §342) contains new diversity requirements for federal agencies involved in the financial sector, as well as the businesses that deal with those agencies. Specifically, each contractor doing business with an agency is required to submit a written statement indicating that it has "ensure[d], to the maximum extent possible, the fair inclusion of women and minorities in [its] workforce ... and, as applicable, [the workforce of] subcontractors."

Table 10
Impact of diversity on flows into nonforeign funds

	Foreign dummy	# of foreign funds in fund family	# of foreign managers in fund family
	(1)	(2)	(3)
Diversity	0.035	0.012	0.015
	(1.90)	(2.83)	(2.96)
Controls	yes	yes	yes
Year FE	yes	yes	yes
Segment FE	yes	yes	yes
Adj. R^2	0.102	0.102	0.102
Observations	12,494	12,494	12,494

This table shows the estimates of percentage fund flows regressed on variables indicating the presence of foreign fund managers in the same fund family and year, as well as control variables. The sample is restricted to funds managed by managers with non-foreign-sounding names. In Column (1), *Diversity* is a dummy variable equal to one, if at least one fund in the same fund company is managed by an individual with a foreign-sounding name in a given year, and zero otherwise. In Column (2), *Diversity* is the number of funds in the same fund company that are managed by individuals with a foreign-sounding name in a given year. In Column (3), *Diversity* is the number of foreign-named managers present at the fund company in a given year. Controls are PRANK, PRANK^2, Fund Size, Turnover, Fund Risk, Expense Ratio, Fund Age, Segment Flow, Fund Flow, Tenure, and Lagged Fund Flow. t-statistics are displayed in parentheses below the coefficient estimates. The standard errors are clustered at the fund level.

Our conversations with practitioners suggest that several of these channels may be operating in parallel. However, establishing the existence of these channels empirically and determining their relative importance is much harder, since data needed for the empirical analysis are currently unavailable. In Table 10, however, we provide one piece of evidence that is consistent with the last mechanism: acquiring additional business by employing foreign-named managers.

Specifically, for each fund managed by an individual with an American-sounding name, we regress fund flows in a given year on variables that capture whether there are managers with foreign-sounding names in the same fund family in that year. The evidence in Table 10 shows that flows to nonforeign-managed funds are higher for funds in families with a foreign-named manager during the year (specification (1)), with more foreign-managed funds (specification (2)) and with more foreign-named managers in the fund family and year (specification (3)). We interpret these results as suggestive evidence that employing at least some foreign fund managers might be beneficial for the fund company as a whole, even though foreign funds attract lower flows individually.

5.3 Dismissals and transfers after bad performance

In our last test, we consider a specific equilibrium outcome: firms could have a lower threshold for dismissing managers with foreign-sounding names after bad performance or for converting underperforming foreign funds to team-managed funds. Eliminating underperforming managers with foreign-sounding names quickly can be a benefit to the fund company. For the manager, a greater likelihood of termination is a direct cost. In addition, being assigned to a

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Table 11
Dismissals and fund conversions after bad performance

	Manager dismissed in $t+1$		Conversion to team in $t+1$	
	(1)	(2)	(3)	(4)
Foreign75	0.050	0.026	−0.564	−0.568
	(0.32)	(0.16)	(−2.65)	(−2.65)
Foreign75 × low PRank	0.490	0.440	0.725	0.708
	(2.31)	(2.06)	(2.46)	(2.35)
Low PRank	0.297	0.315	−0.004	0.003
	(5.38)	(5.48)	(−0.07)	(0.04)
Controls	yes	yes	yes	yes
Year FE	yes	no	yes	no
Segment FE	yes	no	yes	no
Segment × year FE	no	yes	no	yes
Pseudo R^2	0.028	0.041	0.019	0.031
Observations	12,425	12,341	15,759	15,702

This table presents estimates of logit regressions. In Columns (1) and (2), the dependent variable is a dummy variable equal to one if a manager disappears from the sample in the following year, and zero otherwise. The dummy variable is also equal to zero if the manager appears in a team-managed fund the following year. The last year of the sample (=2011) is dropped as all managers mechanically disappear from the sample in the following year. In Columns (3) and (4), the dependent variable is a dummy variable equal to one if a fund converts to a team management structure in the following year, and zero otherwise. Low PRank is a dummy variable equal to one if a fund's performance rank is below the median PRank in a given year, and zero otherwise. Controls are Fund Size, Fund Age, Turnover, and Tenure, all lagged by one year. The corresponding t-statistics are obtained using standard errors clustered at the fund level.

management team is costly to the manager because it reduces outside visibility and associated managerial "brand value" (Massa, Reuter, and Zitzewitz 2010).

Specifications (1) and (2) of Table 11 show that managers with foreign-sounding names have a greater likelihood of being dismissed if they underperform. The estimates imply a 7.2% higher firing probability for underperforming managers with foreign-sounding names than for underperforming managers with non-foreign-sounding names, which is large relative to a baseline firing probability of 3.2% for an underperforming fund. Specifications (3) and (4) show that underperforming managers with foreign-sounding names who previously were the sole manager of a fund are more likely to be moved into a team-managed structure than their nonforeign, underperforming peers. Interestingly, managers with foreign-sounding names who do not underperform have a lower likelihood of being transferred into a team-based structure. This evidence is consistent with the signaling value of employing managers with foreign-sounding names to customers and future employees.

Collectively, the results in this section suggest a number of reasons that fund companies employ foreign-named managers in equilibrium. We can rule out some plausible alternatives and narrow the set of potential explanations. Establishing the precise economic mechanisms that generate the observed flow patterns requires more detailed data than are currently available.

6. Summary and Conclusion

In this paper, we study whether social biases induced by a fund manager's name affect the investment choices of U.S. mutual fund investors. Specifically, we

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examine whether investors are less likely to invest in mutual funds that are managed by individuals with foreign-sounding names. Our key finding is that funds of managers with foreign-sounding names experience 10% lower annual fund flows, even though their performance is similar to funds of other managers who have typical American names. Funds of managers with foreign-sounding names also experience lower appreciation in flows following good performance and greater decline in flows following poor performance.

While the absence of a performance difference makes statistical discrimination unlikely, we provide results from several tests that all support an interpretation based on taste-based discrimination against managers with foreign-sounding names. First, we show that the flow effects are stronger among funds with investor clienteles that are more likely to be suspicious towards foreigners. Further, following the 9/11 terrorist attacks, fund managers with Middle Eastern and South Asian names experience a drop in fund flows relative to other managers with foreign-sounding names.

In an experimental setting in which managerial skill differences do not exist by construction, individuals allocate 11% less money to an index fund managed by an individual with foreign-sounding name. This average American-foreign allocation gap widens further following the Boston Marathon bombings in April 2013. In contrast, in a placebo test in which both fund managers are assigned a typical American name, we find no significant allocation differences. Last, using investor-level data, we document that investors who live in regions with high concentration of foreign-born individuals invest less in funds managed by individuals with more familiar American names.

Taken together, these results suggest that social biases such as in-group bias, stereotyping, and discrimination affect the mutual fund investments of U.S. investors. Our findings contribute to the literature on discrimination, which shows that despite growing public awareness, discrimination influences decision-making in many areas, including legal courts, sports refereeing, consumer choice, and labor markets. Our paper adds a new dimension to this literature by demonstrating that social biases affect capital allocations even in one of the largest and most liquid segments of U.S. capital markets.

In future work, it may be interesting to examine whether social biases influence market participants when they evaluate CEOs, equity analysts, and hedge fund managers. It also would be interesting to examine whether other aspects of names beyond foreignness (e.g., social or economic class indicated by the name) exacerbate the impact of social biases.

Appendix: Brief Definitions and Sources of Main Variables

This table briefly defines the main variables used in the empirical analysis. The data sources are

(1) CRSP: CRSP Survivorship Bias Free Mutual Fund Database,
(2) ARDA: Association of Religion Data Archives,
(3) GSS: General Social Survey,

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(4) Brokerage: Large U.S. discount brokerage,

(5) Estimated: Estimated by the authors,

(6) AMT: Amazon Mechanical Turk,

(7) Census: U.S. Census County Files,

(8) ANES: American National Election Studies,

(9) MS: Morningstar Database, and

(10) SEC: SEC Web page, NSAR-B filings.

Panel A: Main dependent variables

Variable name	Description	Source
Fund flow	Computed as $(TNA_{i,t} - TNA_{i,t-1})/TNA_{i,t-1} - r_{i,t}$ where $TNA_{i,t}$ denotes fund i's total net assets in year t and r_t denotes fund i's return in year t as reported in CRSP, winsorized at the top 99% and bottom 1%.	CRSP, Estimated
LN(inflows)	Logarithm of absolute USD inflows into fund i by household h in year t.	Brokerage, Estimated
LN(outflows)	Logarithm of absolute USD money withdrawals from fund i by household h in year t.	Brokerage, Estimated
Net flows	Inflows$_{i,h,t}$ minus Outflows$_{i,h,t}$.	Brokerage, Estimated

Panel B: Main independent variable

Variable name	Description	Source
ForeignX	Dummy variable equal to one if at least X% of respondents indicated that the fund manager's name sounds foreign, and zero otherwise. X = 50, 75. Respondents indicating "Unsure" are left out.	MS, AMT

Panel C: Other measures

Variable name	Description	Source
Fund characteristics		
Return	Annual fund return.	CRSP
Fund size	Logarithm of a fund's total net assets.	CRSP
Turnover	A fund's annual turnover ratio in % winsorized at top 99% and bottom 1%.	CRSP
Expense ratio	A fund's annual expense ratio in %.	CRSP
Fund risk	A fund's annualized standard deviation based on twelve monthly fund returns.	CRSP, Estimated
Fund age	Logarithm of a fund's age computed from the date the fund was first offered (variable $first\_offer\_dt$).	CRSP, Estimated
Segment flow	Computed as $(TNA_{j,t} - TNA_{j,t-1})/TNA_{j,t-1} - r_{j,t}$ where $TNA_{j,t}$ denotes segment j's total net assets less fund i in year t and $r_{j,t}$ denotes segment j's equal weighted return in year t.	CRSP, Estimated
Family flow	Computed as $(TNA_{f,t} - TNA_{f,t-1})/TNA_{f,t-1} - r_{f,t}$ where $TNA_{f,t}$ denotes fund company f's total net assets less fund i in year t and $r_{f,t}$ denotes fund company f's equal weighted return in year t.	CRSP, Estimated
Marketing expenses	The fraction of a fund's marketing expenses divided by its total expenses.	SEC

(continued)

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Panel C: Other measures—Continued		
Variable name	Description	Source
Location data		
HHAge	Age of the household.	Brokerage
HHGender	Dummy variable equal to one for male investors, and zero otherwise.	Brokerage
HHEducation	Proportion of residents in investor's zip code with a Bachelor's or higher educational degree.	Brokerage, Census
HHForeign	Proportion of foreign-born residents in the investor's zip code.	Brokerage, Census
Rural area	Proportion of MSA residents living in rural areas.	Census
Old population	Dummy equal to one if fund is located in a county with above-median average age in the sample.	Census
Church attendance	Dummy variable equal to one if share of people in a state who go to church at least once a week is larger than median share of people across all states, and zero otherwise. The survey is conducted every second year. We use interpolated values for years in which no survey was conducted.	GSS
PCRATIO	Ratio of Protestant population to Catholic population in a county.	ARDA
Stereotypes	Dummy based on average rating assigned to African-Americans and Asian-Americans divided by the average rating assigned to whites in the question "*Where would you rate WHITES (BLACKS/ASIAN-AMERICANS) in general on this scale?*" on a scale from one (intelligent) to seven (unintelligent). Ratings are aggregated at the state level. We then define a dummy variable that is equal to one if the state level stereotype rating is larger than the sample median, and zero otherwise.	ANES

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