

Market Integration, Ambiguity, and Expected Returns: Evidence from the Stock Connect in China

Jianhua Mei¹ and Nikolai Sheung-Chi Chow²

¹Australian National University

²City University of Macau

September 8, 2026

Abstract

We study whether equity-market integration reduces ambiguity and whether integration changes how ambiguity is priced. Using China’s Shanghai–Shenzhen–Hong Kong STOCK CONNECT program, we exploit staggered, rule-based expansions of STOCK CONNECT as a quasi-experiment. We measure stock-level ambiguity using high-frequency intraday returns based on the Brenner–Izhakian ambiguity measure. Staggered difference-in-differences and Sun–Abraham interaction-weighted event studies show that eligibility is associated with lower ambiguity, although the decline is not immediate and emerges mainly after a short post-inclusion lag. The reduction is larger for stocks that were ex ante more illiquid, although liquidity proxies in the first stage move in mixed directions. Cross-sectionally, ambiguity negatively predicts future returns, and STOCK CONNECT inclusion amplifies this negative ambiguity premium for eligible stocks. We also find that integration increases the sensitivity of stock-level ambiguity to global policy uncertainty, consistent with a shift in the composition of uncertainty that investors price. Overall, the evidence suggests that STOCK CONNECT compresses local probability instability while redirecting attention toward global sources of model uncertainty that matter for expected returns.

1 Introduction

Does opening a stock market to foreign investors make that market less uncertain? A natural first answer is yes. A broader investor base can improve price discovery, increase analyst attention, strengthen information production, and reduce trading frictions. Much of the financial liberalization literature follows this logic and studies how market opening affects prices, liquidity, and growth (Henry, 2000; Bekaert and Harvey, 2000; Bekaert et al., 2005; Hoang and Mateus, 2024). But integration can also have a less obvious effect. Once a local market becomes connected to global capital, local stocks may become more exposed to foreign sentiment, global policy shocks, and international funding conditions. Market opening may therefore reduce some kinds of uncertainty while increasing the relevance of others. This paper studies that tension.

Our focus is *ambiguity*: uncertainty about probabilities rather than uncertainty about outcomes alone. Under standard risk, investors face uncertain outcomes but know the probability

distribution that governs them. Under ambiguity, investors are unsure about the probability model itself (Knight, 1921). The Ellsberg paradox makes the distinction intuitive: people often prefer a gamble with known odds to one with unknown odds (Ellsberg, 1961). In asset pricing, this distinction matters because a market can become more informative without becoming less volatile, and it can become less locally opaque while becoming more exposed to global sources of uncertainty. Modern ambiguity theory and finance show why this distinction is economically meaningful and why information quality is central to pricing under ambiguity (Gilboa and Schmeidler, 1989; Chen and Epstein, 2002; Klibanoff et al., 2005; Epstein and Schneider, 2008).

This paper studies these questions in China, where the Shanghai–Shenzhen–Hong Kong STOCK CONNECT program opened a large subset of A-shares to offshore investors through Hong Kong. STOCK CONNECT is economically important and empirically useful. It represents a major step in China’s gradual capital-market liberalization, but unlike earlier channels that required licences and quotas, it expanded access through a transparent trading link and a sequence of publicly announced eligibility updates. Those updates were staggered and largely rule-based, relying on observable criteria such as index membership, size, and liquidity. This creates sharp variation in the timing at which individual stocks become accessible to foreign investors and makes STOCK CONNECT a useful setting for studying how integration changes the level and pricing of ambiguity.

A central empirical challenge is measurement. The uncertainty literature uses many proxies, including volatility-based measures, macro uncertainty indices, and policy-uncertainty indicators (Bloom, 2009; Jurado et al., 2015; Baker et al., 2016). These measures are useful, but they do not directly isolate ambiguity. Our paper instead studies a probability-based measure grounded in the framework of Izhakian (2020) and implemented empirically by Brenner and Izhakian (2018). The idea is simple. If risk is about the dispersion of outcomes, ambiguity is about the instability of the probabilities attached to those outcomes. We therefore construct stock-level ambiguity from high-frequency intraday returns by tracking how the estimated daily return distribution changes within a month.

This distinction also clarifies our contribution relative to the broader uncertainty literature. We do not ask whether market opening changes volatility or whether it changes a news-based uncertainty index. We ask whether market opening changes ambiguity, and whether it also changes how ambiguity is priced once stocks become accessible to foreign investors. That question connects three literatures that are often studied separately: financial liberalization, ambiguity in asset pricing, and global uncertainty transmission. In that sense, the paper contributes not only to the literature on Stock Connect, but also to a broader question in international finance: whether integration changes the *composition* of uncertainty by reducing local opacity while increasing exposure to global shocks.

Our empirical design targets two closely related objects. The first is a *level effect*: whether STOCK CONNECT eligibility changes stock-level ambiguity. The second is a *pricing effect*: whether the return premium associated with probability instability changes after eligibility. Because eligibility is not random, larger and more liquid firms tend to enter earlier, the paper follows the logic of the modern staggered difference-in-differences literature (de Chaisemartin and D’Haultfoeuille, 2020; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Roth et al.,

2023). We report conventional TWFE estimates as benchmarks for average post-eligibility effects and use the Sun–Abraham interaction-weighted estimator to estimate both the overall post-eligibility effect and its dynamics (Sun and Abraham, 2021). This design lets us study both average post-eligibility effects and the dynamics around first inclusion.

We report four main results. First, STOCK CONNECT eligibility is associated with a meaningful decline in stock-level ambiguity. The decline is not immediate. It emerges after a short lag and remains economically important over subsequent months. Second, ambiguity is negatively priced in the Chinese A-share market, and STOCK CONNECT eligibility significantly amplifies this negative premium. In other words, integration changes not only the level of ambiguity but also the cross-sectional price attached to the remaining ambiguity. We interpret this pricing result as evidence that eligibility changes the demand and compensation environment for probability instability. Third, the decline in ambiguity is stronger for stocks that were more illiquid before integration, which is consistent with a market-quality channel, although the individual liquidity proxies do not all move in the same direction. Fourth, integration changes the *linkage* between local ambiguity and global uncertainty. After eligibility, stock-level ambiguity becomes more sensitive to global policy uncertainty, and in some specifications its relation to VIX also changes. Taken together, these results imply that market opening compresses ambiguity level while redirecting attention toward global, policy-driven sources of uncertainty.

This “levels and linkage” result is the central message of the paper. Market integration is not a one-dimensional reduction in uncertainty. Instead, it can lower local ambiguity by improving the information environment while simultaneously making local assets more exposed to global uncertainty. That interpretation is consistent with theories in which information quality matters for ambiguity and with empirical work showing that foreign institutional participation can improve price efficiency (Epstein and Schneider, 2008; Kacperczyk et al., 2021). In our setting, market opening appears to reduce local belief instability but increase the importance of global shocks in shaping those beliefs.

The paper makes four contributions. First, it brings a theory-consistent ambiguity object to a large stock-level panel by measuring ambiguity as the instability of return probabilities rather than as return volatility. Second, it provides causal evidence on how integration affects ambiguity using a staggered, rule-based liberalization design. Third, it shows that market opening alters the cross-sectional pricing of ambiguity in unexpected ways: rather than simply attenuating the negative ambiguity premium, eligibility is associated with a stronger negative premium, highlighting how institutional opening can interact with local investor demand, market frictions, and the compensation attached to probability instability. Fourth, it documents that integration changes the economic content of ambiguity by shifting it toward stronger comovement with global uncertainty proxies. Existing work on STOCK CONNECT has studied market quality, price synchronicity, and real outcomes such as innovation (Cheng et al., 2019; Xu et al., 2020; Wang, 2021; Ma et al., 2021; Li et al., 2022; Sha et al., 2022). Our contribution is to show that integration also changes the structure of uncertainty faced by investors.

The remainder of the paper is organised as follows. Section 2 describes STOCK CONNECT and the institutional basis for treatment timing. Section 3 details the construction of ambiguity index from intraday returns. Section 4 describes data sources and sample construction. Section 5

presents the staggered DID and event-study framework, as well as the pricing test and the main results. Section 6 studies mechanisms via liquidity heterogeneity and global-linkage interactions. Section 7 concludes.

2 Stock Connect, Market Integration, and Ambiguity

China’s equity-market opening has proceeded in discrete institutional phases. Early channels such as QFII and RQFII expanded foreign participation through *quota-based* access for approved institutions, with licensing and operational constraints that limited breadth and frequency of entry. STOCK CONNECT marked a shift toward *mutual market access*: offshore investors can trade selected mainland A-shares through the Hong Kong market infrastructure, without obtaining onshore quotas, while mainland investors can trade selected Hong Kong shares through the same cross-border link.¹

The program began with *Shanghai–Hong Kong Stock Connect* on 17 November 2014 and expanded to *Shenzhen–Hong Kong Stock Connect* on 5 December 2016. Operationally, orders are submitted through investors’ local brokers and routed through the local exchange and clearing house; cross-border linkages between the exchanges and central counterparties coordinate clearing, settlement, and quota management.²

A central feature for identification is that STOCK CONNECT eligibility is *rule-based* and updated through publicly disclosed list-maintenance procedures. For Northbound trading, eligibility is primarily tied to index membership and observable screens such as size, liquidity, and trading-status constraints, evaluated at scheduled reviews; securities can be removed or designated *sell-only* when they no longer meet the criteria.³ Certain segments, notably STAR Market and ChiNext, are subject to additional investor-eligibility restrictions for Northbound trading.⁴

This institutional evolution places STOCK CONNECT within a broader literature on financial liberalization. Classic studies ask whether market opening lowers the cost of capital, raises valuations, or supports growth (Henry, 2000; Bekaert and Harvey, 2000; Bekaert et al., 2005; Hoang and Mateus, 2024). Our question is different. We study whether integration changes a distinct object: ambiguity. More precisely, we ask whether market opening changes not only the *level* of uncertainty faced by investors, but also the *source composition* of that uncertainty.

STOCK CONNECT is not merely an expansion of the investor base; it also changes the *micro-institutional environment* in which prices are formed. Northbound trading operates through a cross-jurisdiction infrastructure with distinct trading calendars, order-routing conventions, quota and trading-limit regimes, settlement arrangements, and nominee holding structures. These features can affect the speed and reliability with which information is incorporated into prices, the identity of the marginal trader, and the kinds of shocks to which a stock becomes exposed once it is integrated into a broader investor network. This perspective is consistent

¹Institutional descriptions, eligibility criteria, review schedule, and list maintenance are summarized in HKEX program materials. See HKEX (2025) and HKEX (2024).

²For quota levels and subsequent adjustments, see CSRC and SFC (2018) and HKEX (2024).

³See HKEX (2025) for the eligibility logic, review cadence, and the treatment of removals and sell-only status; HKEX also publishes the evolving eligible lists at HKEX (2026).

⁴See HKEX (2021) and HKEX (2025).

with work showing that information quality matters for ambiguity and asset prices, and that foreign institutional investors can improve price efficiency (Chen and Epstein, 2002; Epstein and Schneider, 2008; Kacperczyk et al., 2021).

These institutional features motivate a distinction between two sources of ambiguity that are conceptually separable but need not move in the same direction. The first is *fundamental ambiguity*: uncertainty about firm fundamentals, governance, disclosure quality, and the processing of firm-specific information. By enlarging the investor base and potentially improving price discovery and market quality, integration can reduce this type of ambiguity and make investors’ probability assessments more stable. The second is *structural or systemic ambiguity*: uncertainty associated with trading frictions and exposure to broader shocks that accompany cross-market integration, including global policy uncertainty, cross-border funding conditions, and international financial cycles. In that sense, integration can reduce local opacity while making connected stocks more sensitive to global sources of uncertainty (Jurado et al., 2015; Rey, 2015; Baker et al., 2016).

This distinction sharpens the empirical agenda. We test whether eligibility reduces ambiguity *levels* and whether it changes ambiguity *composition*. In particular, beyond estimating average treatment effects on our stock-level ambiguity measure, we examine whether the ambiguity of connected stocks becomes more sensitive to global uncertainty after integration and whether the largest ambiguity declines occur among stocks that were more illiquid before integration (Section 6). This “levels and linkage” structure follows the logic of recent work that studies both the first-order effects of market opening and the channels through which those effects operate.

Our paper also differs from the existing STOCK CONNECT literature in outcome space. Most studies focus on liquidity, market quality, stock-price synchronicity, or real outcomes such as innovation (Cheng et al., 2019; Xu et al., 2020; Li et al., 2022; Wang, 2021; Sha et al., 2022). We instead study ambiguity. That shift in focus matters because a market can become more integrated and more informative without becoming less exposed to global uncertainty. Figure 1 places STOCK CONNECT in the broader chronology of China’s equity-market opening and highlights our sample window.

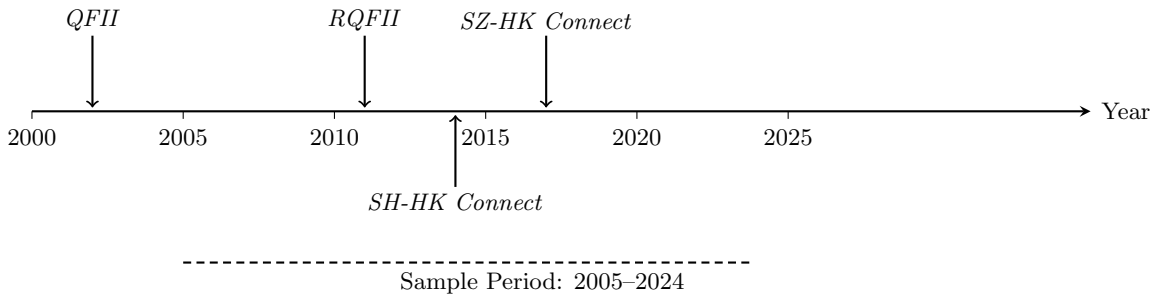


Figure 1: Timeline of capital market liberalization and sample period

3 Ambiguity Measurement

Ambiguity refers to uncertainty about the probability distribution of outcomes, whereas under risk that distribution is taken as known and only realized outcomes are uncertain. This distinction

is central to modern ambiguity theory and is one reason ambiguity need not move one-for-one with standard volatility-based measures (Knight, 1921; Ellsberg, 1961; Gilboa and Schmeidler, 1989; Klibanoff et al., 2005). In the broader uncertainty literature, researchers often rely on proxies such as policy-uncertainty indices or macro uncertainty factors (Baker et al., 2016; Jurado et al., 2015). Those measures are useful for tracking aggregate uncertainty, but they do not directly isolate instability in the probability law itself. Our focus is therefore narrower and more structural: we study ambiguity as uncertainty about the probabilities assigned to return outcomes.

We measure ambiguity using the Brenner–Izhakian index $\mathcal{U}^2$ (Brenner and Izhakian, 2018). The underlying ambiguity metric is theoretically grounded in Izhakian (2020), while Brenner and Izhakian (2018) provides an empirical implementation using high-frequency return data. This implementation is well suited to our setting because it allows us to construct stock-level ambiguity directly from intraday returns. It also has an important conceptual advantage for our purposes. Unlike realized volatility or news-based uncertainty proxies, the Brenner–Izhakian measure is designed to capture variation in the probabilities of returns rather than variation in returns themselves. That makes it especially appropriate in a paper that studies whether market integration changes the ambiguity, not merely the dispersion of outcomes.

Following Brenner and Izhakian (2018), we assume that daily returns are normally distributed but not identically distributed, with each trading day characterized by a distinct mean μ and variance σ^2 . Ambiguity is then defined as

$$\mathcal{U}^2[r] = \int \mathbb{E}[\varphi(r; \mu, \sigma)] \text{Var}[\varphi(r; \mu, \sigma)] dr, \quad (1)$$

where $\varphi(\cdot)$ denotes the normal density function, $\mathbb{E}[\varphi(r; \mu, \sigma)]$ is the expected probability density of return r across daily return distributions, and $\text{Var}[\varphi(r; \mu, \sigma)]$ is its variance. Intuitively, just as risk is measured by the volatility of returns, ambiguity is measured by the volatility of return probabilities.

To implement this measure at the monthly frequency, we discretize each daily return distribution into a histogram. Specifically, daily returns are divided into 60 bins of width $w = 0.2\%$ over the range $[-6\%, +6\%]$, together with two tail bins, yielding 62 bins in total. Monthly ambiguity is then computed from the cross-day mean and variance of daily bin probabilities within each month:

$$\begin{aligned} \mathcal{U}^2[r] = \frac{1}{w(1-w)} & \left[\mathbb{E}[\Phi(r_0; \mu, \sigma)] \text{Var}[\Phi(r_0; \mu, \sigma)] \right. \\ & + \sum_{j=1}^{60} \mathbb{E}[\Phi(r_j; \mu, \sigma) - \Phi(r_{j-1}; \mu, \sigma)] \text{Var}[\Phi(r_j; \mu, \sigma) - \Phi(r_{j-1}; \mu, \sigma)] \\ & \left. + \mathbb{E}[1 - \Phi(r_{60}; \mu, \sigma)] \text{Var}[1 - \Phi(r_{60}; \mu, \sigma)] \right], \end{aligned} \quad (2)$$

where $r_0 = -0.06$, $r_{60} = 0.06$, $w = r_j - r_{j-1} = 0.002$, $\Phi(\cdot)$ denotes the normal cumulative distribution function. The scaling factor $1/[w(1-w)]$ follows the discrete implementation of

Brenner and Izhakian (2018, p. 511). This normalization scales the weighted-average volatility of probabilities to the bin size and reduces the sensitivity of the estimated ambiguity measure to the chosen bin width. We therefore adopt the same scaling factor and the same fixed return grid in our empirical implementation.

A fixed return grid is important for the monthly implementation of the measure. The object of interest is the variation across trading days in the probabilities assigned to the same return events within a month. For this reason, the return intervals must remain common across days when computing the expectation and variance terms in (2). Following the high-frequency implementation of Brenner and Izhakian (2018), we use 5-minute intraday returns for each stock i and trading day d within month t to estimate the day-specific mean $\mu_{i,d}$ and volatility $\sigma_{i,d}$. This sampling frequency is also standard in the intraday volatility literature, where it is commonly adopted as a practical compromise between preserving intraday information and limiting market microstructure noise (Andersen et al., 2001; Hansen and Lunde, 2006). These day-specific parameters are then used to compute the bin probabilities, such as $\Phi(r_j; \mu_{i,d}, \sigma_{i,d}) - \Phi(r_{j-1}; \mu_{i,d}, \sigma_{i,d})$, so the time variation in daily return distributions is fully retained. The two tail bins further ensure that probability mass outside the interior interval is incorporated rather than discarded.⁵

4 Data Sources and Sample Construction

We construct a stock-month panel of Chinese A-share common stocks spanning the period 2005 to 2024, combining high-frequency intraday data for ambiguity measurement, adjusted daily series for returns and controls, and official Northbound eligible lists for treatment timing.

4.1 Data sources

We draw from three primary inputs. First, we use 5-minute intraday returns from RESSET to construct the Brenner–Izhakian stock-level ambiguity measure, $\mathcal{U}_{i,t}^2$. Second, we obtain adjusted daily files from CSMAR to compute monthly stock returns, market capitalization, and liquidity proxies. Third, we map the official HKEX eligible-security lists to define the stock-specific month of first Northbound inclusion, which determines our treatment timing. Table 1 summarizes the sources used for each component.

⁵Using day-specific standardized intervals such as $[\mu_{i,d} - 3\sigma_{i,d}, \mu_{i,d} + 3\sigma_{i,d}]$ would change the underlying return events from day to day, thereby weakening the comparability of probability masses across trading days within a month.

Table 1: Data sources

Data item	Source and construction
5-minute prices/returns	RESSET high-frequency data (5-minute).
Daily returns and trading variables	CSMAR.
Stock Connect eligibility timing	Official HKEX eligible-security lists .
Risk-free rate	Monthly risk-free proxy from CSMAR.
Global uncertainty (VIX)	CBOE VIX .
Global uncertainty (GEPU)	Global EPU from PolicyUncertainty.com.

4.2 Sample construction and treatment definition

The baseline universe consists of A-share common stocks listed on the SSE and SZSE. We exclude STAR Market and ChiNext listings in the baseline because they operate under distinct listing and eligibility rules.

The empirical treatment is based on the stock-specific first effective month of Northbound Stock Connect eligibility. Let T_i denote the first month in which stock i is effectively eligible for Northbound trading according to the official Northbound eligible list. We define the absorbing treatment indicator

$$SC_{i,t} \equiv \mathbb{1}\{t \geq T_i\}.$$

Thus, the treatment timing is aligned with the effective eligibility month rather than the announcement month of the relevant list-maintenance notice. Because eligibility changes may be announced before they become effective, some price adjustment may occur during the announcement-to-effective window.

This definition treats Stock Connect eligibility as an absorbing first-inclusion exposure, consistent with a staggered-adoption design in which stocks enter the treated group at different effective eligibility months. Once a stock first becomes eligible, it remains classified as treated in subsequent months. Later list-maintenance changes, including temporary removal or sell-only status, are therefore treated as part of the post-first-exposure period. Accordingly, the estimates should be interpreted as event-time effects of first effective eligibility. Pre-inclusion coefficients may partly reflect anticipation during the announcement-to-effective window, while post-inclusion coefficients do not isolate whether a stock remains actively eligible in each subsequent month.

Because eligibility is rule-based and depends on observables (size, liquidity, index membership, and trading-status constraints), treated stocks differ systematically from never-treated stocks even prior to integration. This motivates the identification strategy in Section 5, which pairs staggered DID/event-study estimators with predetermined (pre-STOCK CONNECT) covariates and explicit pre-trend diagnostics.

We implement the analysis panel at the stock-month level. Months in which a stock is suspended for the full month are excluded; partial-month issues are handled through the construction of within-month valid trading-day coverage for ambiguity and the daily-to-month aggregation rules for controls. Table 2 summarizes the treatment-status composition of the estimation panel by subperiod. The treated share rises sharply after 2014 as eligibility expands,

while a sizeable pool of not-yet-treated and never-treated observations remains throughout, supporting clean comparisons in staggered designs.

Table 2: Observations by treatment status and period

Period	Treated	Not-yet-treated	Never-treated	Total
2005–2013	0	134,303	30,527	164,830
2014–2016	21,071	39,130	13,847	74,048
2017–2019	55,859	16,219	21,010	93,088
2020–2024	118,636	15,568	43,428	177,632
Full sample	195,566	205,220	108,812	509,598

Notes: Entries report stock-month observations in each treatment state (each stock counted once per month). “Treated” indicates $SC_{i,t} = 1$; “not-yet-treated” are stocks with $T_i > t$; “never-treated” are stocks with $T_i = \infty$ within the sample window.

4.3 Variable construction

Our primary variable of interest is stock-level ambiguity, $\mathcal{U}_{i,t}^2$, estimated at the monthly frequency using 5-minute intraday returns, as described in Section 3. For the ambiguity-premium pricing tests, explanatory variables measured in month t are aligned with next-month excess returns, $rx_{i,t+1}$, defined as the stock return in excess of the monthly risk-free rate.

To isolate the pricing effect of ambiguity and account for observable characteristics related to STOCK CONNECT eligibility, we construct a set of firm-level controls. These include standard firm characteristics and risk proxies, such as firm size (ME) and realized volatility (RV). Given the role of trading frictions in our setting, we also include several market-quality proxies. Trading activity is measured by dollar volume and turnover, while price impact is captured by the Amihud (2002) illiquidity ratio. We compute the Amihud measure as the within-month median of daily ratios to reduce the influence of extreme daily observations. We also include a high-low spread proxy to control for transaction costs.

To reduce concerns that post-treatment changes in firm characteristics may affect the estimates, we construct baseline controls as stock-specific medians over the pre-STOCK CONNECT window ending in October 2014 and hold them fixed thereafter. As a robustness diagnostic, we also report specifications using one-period lagged time-varying controls. Table 3 summarizes the formal definitions and construction of the key variables.

Table 3: Key variables and construction

Variable	Symbol	Construction
Treatment	$SC_{i,t}$	Absorbing indicator equal to one after stock i first becomes effectively eligible for Northbound STOCK CONNECT trading. T_i denotes the first effective eligibility month, and $SC_{i,t} = \mathbb{1}\{t \geq T_i\}$.

Table 3 (continued)

Variable	Symbol	Construction
Daily return	$r_{i,d}^{1d}$	Adjusted daily stock return.
Monthly return	$r_{i,t}$	Compounded from adjusted daily returns: $r_{i,t} = \prod_{d \in t} (1 + r_{i,d}^{1d}) - 1$.
Next-month excess return	$rx_{i,t+1}$	One-month-ahead return in excess of the risk-free rate: $rx_{i,t+1} = 100 \times (r_{i,t+1} - r_{f,t+1})$.
Ambiguity	$\mathcal{U}_{i,t}^2$	Stock-level Brenner–Izhakian ambiguity measure estimated from 5-minute intraday returns within month t ; see Section 3.
Firm size	$\log ME_{i,t}$	Log month-end market capitalization.
Realized volatility	$\log(1 + RV_{i,t})$	Log of one plus monthly realized volatility, where $RV_{i,t}$ is the square root of the sum of squared daily returns.
Dollar volume	$\log(1 + \text{DoIVol}_{i,t})$	Log of one plus aggregate daily trading value within the month.
Turnover	$\text{Turnover}_{i,t}$	Value-based monthly turnover, aggregated from daily trading value scaled by market capitalization.
Illiquidity	$ILLIQ_{i,t}$	Amihud-type price-impact measure, defined as the within-month median of daily ratios: $ILLIQ_{i,t} = \text{median}_{d \in t} (r_{i,d}^{1d} / \text{DoIVol}_{i,d})$.
Bid–ask spread proxy	$\text{Spread}_{i,t}$	High–low spread proxy estimated from daily high and low prices and averaged within the month.
Global uncertainty	VIX, GEPV	Monthly CBOE Volatility Index and Global Economic Policy Uncertainty index, standardized when used in interaction specifications.

4.4 Descriptive statistics

Table 4 reports summary statistics for the stock–month analysis panel. Monthly ambiguity is sizable and right-skewed: the mean of $\mathcal{U}_{i,t}^2$ is 1.41, while the median is 0.37. Next-month excess returns (in percent) are dispersed (s.d. 13.05), with a mean of 0.96 and a slightly negative median (−0.19). The panel is large (about 0.51 million stock–months), though the effective sample varies across variables because intraday coverage is incomplete for a small subset of stock–months and next-month returns are missing at the sample endpoints. The average treated share, $\mathbb{E}[\text{SC}_{i,t}]$, is 0.384. Market-quality proxies exhibit substantial cross-sectional heterogeneity. Realised volatility averages 0.12 (s.d. 0.056), and turnover is highly skewed (mean 25.39 versus

median 16.37). The spread proxy averages 0.016.

Table 4: Descriptive statistics

Variable	Mean	Median	Std. dev.	P25	P75	Obs.
Excess Return	0.958	−0.186	13.046	−6.911	7.525	506,212
Monthly Return	1.100	0.000	13.062	−6.791	7.672	509,598
Ambiguity	1.411	0.371	2.316	0.153	1.528	509,284
Realized Volatility	0.121	0.111	0.056	0.079	0.152	509,598
Log Volatility	0.113	0.104	0.049	0.076	0.141	509,598
Illiquidity	0.656	0.283	1.240	0.127	0.618	509,598
Bid-Ask Spread	0.016	0.014	0.007	0.010	0.019	509,598
Turnover	25.386	16.372	25.955	8.443	32.425	509,598
Dollar Volume	21.075	21.058	1.287	20.221	21.928	509,598
Firm Size	15.650	15.495	1.112	14.887	16.278	509,598
Connect Eligibility	0.384	—	—	—	—	509,598

Notes: The unit of observation is a stock-month. Detailed variable definitions are provided in Table 3. Both excess return (rx_{t+1} , measured at month $t + 1$) and monthly return (r) are reported in percent. To facilitate reporting, the Amihud illiquidity measure ($ILLIQ$) is scaled by 10^9 . The mean of the STOCK CONNECT eligibility indicator (SC) represents the share of treated observations in the sample.

Figure 2 plots portfolio-level ambiguity over time for currently eligible stocks and a portfolio of not-yet-treated stocks. The two series comove strongly around major market-wide episodes (notably 2015), while post-2016 ambiguity for eligible stocks is typically below the not-yet-treated portfolio. The figure is descriptive; identification and inference rely on the panel designs in Section 5. ⁶

⁶The *eligible* portfolio averages monthly ambiguity ($\bar{U}^2$) across all stocks currently included in Stock Connect. The *not-yet-treated* portfolio averages the same measure across stocks that eventually enter Stock Connect but have not yet done so, providing a pre-treatment benchmark drawn from the same eligibility pool.

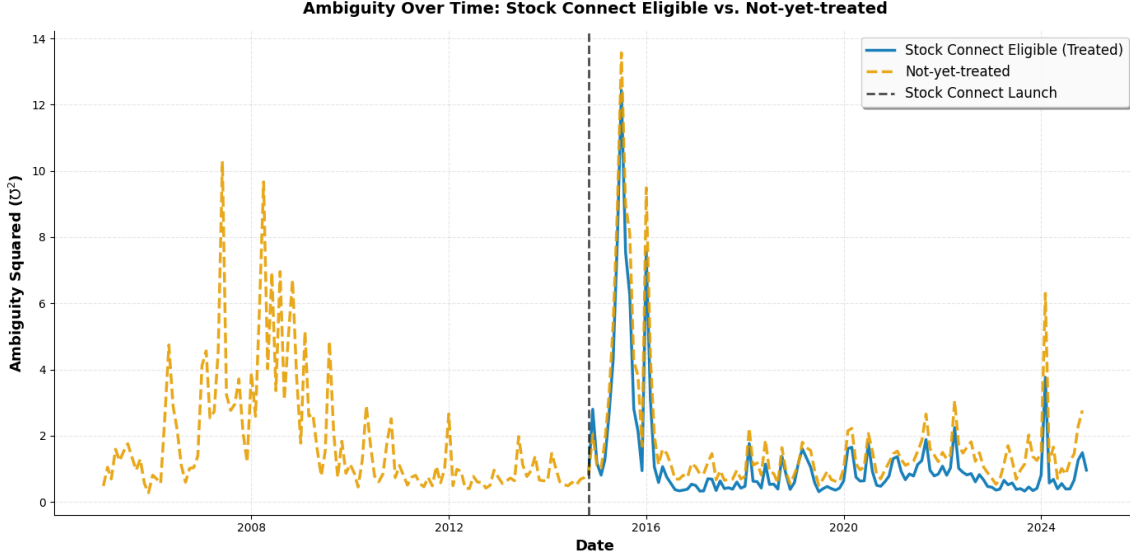


Figure 2: Ambiguity $\mathcal{U}^2$ over time: eligible versus not-yet-treated portfolios

5 Empirical Analysis

5.1 Staggered difference-in-differences

Eligibility for Northbound STOCK CONNECT trading is determined by observable screens such as size, liquidity, and index membership, all of which may also be related to information environments and ambiguity. That feature makes a simple before-after comparison inappropriate. Because eligibility is not random, we follow the logic of the modern staggered difference-in-differences literature (de Chaisemartin and D’Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021; Roth et al., 2023). Our conventional benchmark estimates use pooled stock-month regressions with a *single* post-eligibility indicator and report one coefficient per specification, combining (i) calendar *year-month* fixed effects to absorb common shocks and macro trends, and (ii) high-dimensional cross-sectional fixed effects (stock or industry/market-type) to absorb persistent differences across units.⁷

Conventional TWFE benchmark. For comparison with conventional specifications, we first estimate the average post-eligibility effect using the two-way fixed-effects (TWFE) regression. Our main estimand is a *static* post-eligibility average treatment effect on the treated (ATT) on ambiguity in levels,

$$\text{ATT} \equiv \mathbb{E}[\mathcal{U}_{i,t}^2(1) - \mathcal{U}_{i,t}^2(0) \mid i \text{ ever treated}, t \geq T_i].$$

We estimate

$$\mathcal{U}_{i,t}^2 = \beta \text{SC}_{i,t} + \alpha_i + \lambda_t + \mathbf{Z}_{i,t}'\boldsymbol{\theta} + \varepsilon_{i,t}, \quad (3)$$

⁷This presentation mirrors the logic of a main “static” DiD table with one post indicator coefficient, as in Braghieri et al. (2022).

where α_i denotes stock fixed effects, λ_t denotes year-month fixed effects, and $\mathbf{Z}_{i,t}$ is a vector of controls described below. In specifications (4)–(5), α_i is replaced by industry fixed effects $\phi_{j(i)}$, where $j(i)$ denotes the industry of stock i . We estimate equation (3) using absorbed fixed effects and cluster standard errors by stock.

Under homogeneous treatment effects across cohorts and event time, the conventional TWFE coefficient has the usual ATT interpretation. With heterogeneous treatment effects, however, conventional TWFE estimates may not have a straightforward ATT interpretation (de Chaisemartin and D’Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021; Sun and Abraham, 2021). We therefore treat the TWFE estimates as benchmarks and complement them with the interaction-weighted estimator of Sun and Abraham (2021), which accommodates treatment-effect heterogeneity across cohorts and event time.

Sun–Abraham interaction-weighted estimation. Our Sun–Abraham analysis is used for both the overall post-eligibility effect and the dynamic event-study estimates. Following Sun and Abraham (2021), we first estimate cohort-by-event-time treatment effects and then aggregate them according to the estimand of interest. For the overall post-eligibility estimate, we aggregate the cohort-by-event-time effects over post-treatment months 0 through 36 using treated stock-month shares. To study treatment-effect dynamics, we additionally report interaction-weighted event-study estimates by relative time. Let relative time be $\ell \equiv t - T_i$ (in months from first eligibility month T_i). We then define one *pre-treatment lead bin* plus a set of *post-treatment lag bins*:

$$G^- \equiv \{g_- = \{-24, \dots, -2\}\},$$

$$G^+ \equiv \{g_0 = \{0\}, g_1 = \{1, 2, 3\}, g_2 = \{4, 5, 6\}, g_3 = \{7, \dots, 12\}, g_4 = \{13, \dots, 24\}, g_5 = \{25, \dots, 36\}\}.$$

For each bin $g \in G^- \cup G^+$, the corresponding dynamic estimand is the event-time average treatment effect on the treated,

$$\text{ATT}_g \equiv \frac{1}{|g|} \sum_{\ell \in g} \text{ATT}_\ell, \quad \text{ATT}_\ell \equiv \mathbb{E}[\mathcal{U}_{i,t}^2(1) - \mathcal{U}_{i,t}^2(0) \mid i \text{ ever treated}, t - T_i = \ell],$$

reported relative to the omitted reference month $\ell = -1$.

Binning relative time serves two purposes. First, it reduces sampling noise and keeps the event study path interpretable in a setting with many cohorts and a long post period. Second, by summarizing effects at coarse horizons, it makes the exposure-response pattern easier to read, in particular whether ambiguity responds immediately at $\ell = 0$ or accumulates over subsequent months after STOCK CONNECT eligibility.

The lead bin g_- provides a direct diagnostic for differential pre-trends: under the identifying assumptions, the corresponding IW estimand should be close to zero. In the empirical implementation, however, estimation proceeds at the monthly event-time level and only then aggregates to the reported bins. Index cohorts by the first-eligibility month e (i.e., $T_i = e$). We estimate

$$\mathcal{U}_{i,t}^2 = \alpha_i + \lambda_t + \sum_e \sum_{\ell \neq -1} \delta_{e,\ell} \left(\mathbb{1}\{T_i = e\} \cdot \mathbb{1}\{t - T_i = \ell\} \right) + \mathbf{Z}'_{i,t} \boldsymbol{\theta} + \varepsilon_{i,t}, \quad (4)$$

where α_i denotes stock fixed effects, λ_t denotes year-month fixed effects, and $\mathbf{Z}_{i,t}$ collects the specification-specific controls. In specifications (4)–(5), α_i is replaced by industry fixed effects $\phi_{j(i)}$, where $j(i)$ denotes the industry of stock i . Thus, $\delta_{e,\ell}$ is the cohort-specific effect at relative month ℓ relative to the omitted month $\ell = -1$.

Following [Sun and Abraham \(2021\)](#), the monthly IW estimand aggregates cohort-specific effects using *risk-set cohort shares*:

$$\nu_\ell \equiv \sum_{e \in h_\ell} \omega_{e,\ell} \delta_{e,\ell}, \quad \omega_{e,\ell} \equiv \Pr(T_i = e \mid T_i \in h_\ell), \quad (5)$$

where h_ℓ denotes the set of cohorts observed at relative month ℓ . The sample analog is

$$\hat{\nu}_\ell = \sum_{e \in h_\ell} \hat{\omega}_{e,\ell} \hat{\delta}_{e,\ell}, \quad \hat{\omega}_{e,\ell} = \frac{N_e}{\sum_{e' \in h_\ell} N_{e'}},$$

with N_e the number of stocks in cohort e in the estimation sample. The bin-level IW estimand is then obtained by averaging the monthly IW estimates within each bin:

$$\hat{\nu}_g = \frac{1}{|g|} \sum_{\ell \in g} \hat{\nu}_\ell. \quad (6)$$

We also compute an equal-weight post-period IW average over $\ell \in [0, 36]$ as a summary of the dynamic estimates, giving equal weight to each event month. By contrast, the reported overall post-eligibility estimate aggregates the same monthly IW estimates over $\ell \in [0, 36]$, weighting each month by its share of treated stock-month observations. Thus, the two summaries are based on the same monthly IW estimates but differ in their weighting across event time.

Control sets. Baseline controls are predetermined stock characteristics computed as medians over the pre-STOCK CONNECT window through October 2014 and held fixed thereafter: $\log(1 + ME_i)$ and $\log(1 + ME_i)^2$, baseline return volatility, baseline returns, and baseline turnover ratio. In specifications with stock fixed effects, these baseline controls are perfectly collinear with α_i and therefore absorbed; they matter only insofar as they define a common sample and facilitate comparison to specifications without stock fixed effects. Lagged controls $\mathbf{Z}_{i,t}$ are measured at $t - 1$ and include the lagged counterparts $\log(1 + ME_{i,t-1})$, $\log(1 + ME_{i,t-1})^2$, realised volatility, returns, and turnover ratio. Because these variables may still respond to earlier STOCK CONNECT exposure, specifications including lagged controls are interpreted as sensitivity/mediation diagnostics rather than preferred causal estimates.

5.2 Pricing Test on Ambiguity Premium

We next examine whether Stock Connect eligibility alters the cross-sectional pricing of ambiguity. The analysis is implemented as a predictive panel regression of one-month-ahead excess returns on month- t ambiguity, the Stock Connect eligibility indicator, and their interaction:

$$rx_{i,t+1} = \alpha + \beta_1 \mathcal{U}_{i,t}^2 + \beta_2 (\mathcal{U}_{i,t}^2 \times \text{SC}_{i,t}) + \beta_3 \text{SC}_{i,t} + \gamma' \mathbf{Z}_{i,t} + \phi_{j(i)} + \lambda_t + \varepsilon_{i,t+1}, \quad (7)$$

where $rx_{i,t+1}$ is the excess return of stock i in month $t + 1$, and $\bar{U}_{i,t}^2$ is the stock-level ambiguity measure observed in month t . The variable $SC_{i,t}$ is an absorbing indicator that equals one from stock i 's first effective month of Northbound Stock Connect eligibility onward, and zero otherwise. Thus, the specification exploits the staggered entry of stocks into the eligible list to compare the ambiguity-return relation between non-eligible stock-months and post-eligibility stock-months. Following standard asset pricing literature, we include industry fixed effects $\phi_{j(i)}$ rather than stock fixed effects to preserve the cross-sectional identification of the risk premium, while controlling for a comprehensive vector of time-varying firm characteristics $\mathbf{Z}_{i,t}$. Year-month fixed effects λ_t are included to absorb aggregate market-wide shocks. Standard errors are clustered at the firm level.

The coefficients of primary interest are β_1 and β_2 . The coefficient β_1 measures the baseline conditional ambiguity-return slope for observations with $SC_{i,t} = 0$, which include both never-eligible stocks and stocks that are not yet eligible at month t . The coefficient β_2 captures the incremental change in the ambiguity premium induced by Stock Connect inclusion. Accordingly, the total implied ambiguity premium for post-eligibility observations is given by $\beta_1 + \beta_2$. This interaction framework effectively evaluates the average shift in the market's pricing mechanism of probability instability following capital market liberalization, while the year-month fixed effects ensure that identification originates strictly from within-month cross-sectional variation.

Control sets. To ensure that our measure of ambiguity captures a distinct premium associated with Knightian uncertainty rather than proxying for other known risk factors, we include a comprehensive set of control variables in the vector $\mathbf{Z}_{i,t}$. Most critically, we control for return volatility to disentangle ambiguity from risk. Since ambiguity is empirically correlated with volatility, omitting it could lead the ambiguity coefficient to partially capture a standard risk effect. This follows [Brenner and Izhakian \(2018\)](#), who also control for volatility when estimating the ambiguity premium.

We further include standard firm characteristics established in the asset pricing literature. We control for *Size* (log market capitalization) and the *book-to-market ratio* to capture the size and value premia ([Fama and French, 1992](#)); controlling for size is particularly important given that smaller firms tend to suffer from greater information asymmetry. Regarding price trends, we include *Momentum* (cumulative returns from $t-12$ to $t-2$) and *Short-term Reversal* (return in month $t-1$) to account for intermediate-horizon return continuation ([Jegadeesh and Titman, 1993](#)) and short-term mean reversion ([Jegadeesh, 1990](#)), respectively. Finally, the *Amihud* type illiquidity measure ([Amihud, 2002](#)) is included to ensure that our findings reflect compensation for probability uncertainty rather than the price impact of trading in thinly traded stocks.

5.3 Main Results

5.3.1 Integration and ambiguity

Table 5 reports estimates of the effect of STOCK CONNECT eligibility on monthly stock-level ambiguity, $\bar{U}_{i,t}^2$. Panel A reports conventional fixed-effects benchmark estimates. Columns (1)–(3) use stock and year-month fixed effects and therefore correspond to the conventional TWFE specifications. Columns (4)–(5) replace stock fixed effects with industry fixed effects and are

reported as additional sensitivity specifications. Panel B reports the corresponding interaction-weighted estimates obtained from cohort-by-event-time interactions and aggregated over post-treatment months 0 through 36. Columns (1)–(3) implement the Sun–Abraham interaction-weighted estimator with stock and year-month fixed effects, while columns (4)–(5) apply the same cohort-by-event-time aggregation to the industry-fixed-effect sensitivity specifications.

Table 5: Static treatment-effect estimates across alternative estimators

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: TWFE estimates</i>					
Post-eligibility effect	-0.4945*** (0.0256)	-0.4239*** (0.0304)	-0.3741*** (0.0238)	-0.3057*** (0.0248)	-0.2701*** (0.0192)
<i>Panel B: Sun–Abraham estimates</i>					
Post-eligibility effect	-0.2684*** (0.0434)	-0.2030*** (0.0534)	-0.2169*** (0.0411)	-0.2195*** (0.0279)	-0.1977*** (0.0217)
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	No	No
Industry FE	No	No	No	Yes	Yes
Baseline-complete sample	No	Yes	No	Yes	No
Baseline controls	No	No	No	Yes	No
Lagged controls	No	No	Yes	No	Yes
Observations	229,828	167,279	225,235	167,279	225,235
Stocks	3,236	2,084	3,208	2,084	3,208

Notes: Panel A reports conventional static fixed-effects benchmark estimates. Panel B reports the interaction-weighted overall ATT estimates of [Sun and Abraham \(2021\)](#), aggregating cohort-by-event-time effects over $[0, 36]$ using treated-stock-month shares. Column (1) uses the full sample without controls. Column (2) restricts the estimation to the baseline-complete sample without including baseline controls. Columns (3) and (5) include one-month-lagged stock characteristics and are reported as sensitivity specifications. Column (4) includes predetermined baseline characteristics with industry and year-month fixed effects. Treated observations are restricted to event months $[-24, 36]$. Standard errors are clustered by stock. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results are consistently negative across estimators and specifications. In Panel A, the conventional fixed-effects estimates range from -0.4945 to -0.2701 and are statistically significant at the 1 percent level in all five specifications. The corresponding interaction-weighted estimates in Panel B are smaller in magnitude, ranging from -0.2684 to -0.1977 , but remain negative and statistically significant throughout. In particular, the Sun–Abraham estimates in the stock-fixed-effect specifications, columns (1)–(3), range from -0.2684 to -0.2030 . Thus, the estimated decline in ambiguity remains economically and statistically meaningful after allowing treatment effects to vary across eligibility cohorts and event time.

The comparison across specifications also indicates that the main result is not driven by a particular sample or control choice. Column (2) differs from column (1) only by restricting the sample to stocks with complete predetermined baseline characteristics. We treat column (2) as our focal specification because it retains stock and year-month fixed effects, uses a sample

for which the predetermined baseline characteristics are fully observed, and avoids potentially post-treatment time-varying controls. Column (3) adds one-month-lagged stock characteristics. Columns (4)–(5) replace stock fixed effects with industry fixed effects; column (4) includes the predetermined baseline characteristics directly, whereas column (5) includes lagged stock-level controls. Because the lagged characteristics may themselves respond to earlier STOCK CONNECT exposure, columns (3) and (5) are interpreted as sensitivity specifications rather than preferred causal specifications.

As an additional robustness check, Appendix Table B1 reports group-time ATT estimates following Callaway and Sant’Anna (2021), using not-yet-treated stocks as the comparison group. The overall estimates range from -0.3062 to -0.2019 and are statistically significant in all three specifications. The negative post-eligibility effect therefore also appears under an alternative staggered-DID estimator that constructs treatment effects from comparisons between treated and not-yet-treated stocks.

Table 6 examines how the post-eligibility effect evolves around the first effective eligibility month. The event-study estimates show little evidence of an immediate decline in ambiguity. At event month zero, the estimated effects are small and statistically insignificant in all five specifications. During months 1–3, the estimates remain close to zero and statistically insignificant in columns (1) and (3), while columns (2), (4), and (5) produce positive estimates.

A clear decline emerges from months 4–6 onward. All five estimates over $[4, 6]$ are negative and statistically significant, ranging from -0.3276 to -0.2051 . The decline becomes substantially larger during months 7–12, with estimates between -0.7635 and -0.5527 , and remains negative and statistically significant during months 13–24. By months 25–36, all five point estimates remain negative but are considerably smaller in magnitude; only column (5) remains statistically significant. The equal-weight post-period IW averages are also negative and statistically significant in all five specifications, ranging from -0.2570 to -0.1928 . Overall, the dynamic estimates indicate that the reduction in ambiguity develops gradually following STOCK CONNECT eligibility rather than occurring contemporaneously with first inclusion.

This gradual response is economically intuitive. STOCK CONNECT broadens foreign investor access and exposes eligible stocks to a wider investor base. If integration improves information production, price discovery, and the incorporation of firm-specific information into prices, these adjustments need not occur immediately. The relatively small effects around the first eligibility month, followed by a substantially larger decline during the subsequent four to twelve months, are therefore consistent with a gradual adjustment in the information environment following market integration.

In the focal specification (2), the pre-treatment estimate is statistically insignificant; the doubly robust Callaway–Sant’Anna specification with baseline covariates in Appendix Table B1 likewise shows no statistically significant pre-treatment estimates.⁸ The Callaway–Sant’Anna dynamic estimates also reproduce the main post-treatment pattern: effects are statistically insignificant at event month zero and during months 1–3, become negative and statistically significant from months 4–6 onward, and remain negative at subsequent horizons. The gradual

⁸Some alternative sensitivity specifications produce positive and statistically significant pre-treatment estimates. We therefore interpret the event-study estimates primarily as describing the timing of the post-eligibility decline rather than as establishing uniformly flat pre-treatment dynamics across every specification.

post-eligibility decline therefore survives when treatment effects are estimated using not-yet-treated stocks as the comparison group.

Figure 3 visualizes the binned IW path for specification (2), with 95% confidence intervals and the dashed line showing the average of the monthly IW estimates. Parallel figures for the remaining specifications are reported in the Appendix.

Table 6: Dynamic effects on ambiguity: Sun–Abraham interaction-weighted estimates

	(1)	(2)	(3)	(4)	(5)
<i>Pre-treatment</i>					
$\ell \in [-24, -2]$	0.1363*** (0.0465)	0.0811 (0.0543)	0.0648 (0.0442)	0.0741** (0.0340)	0.0654** (0.0267)
<i>Post-treatment</i>					
$\ell = 0$	-0.0366 (0.0468)	0.0556 (0.0592)	-0.0326 (0.0475)	0.0647 (0.0553)	0.0186 (0.0408)
$\ell \in [1, 3]$	0.0262 (0.0474)	0.1902*** (0.0596)	0.0241 (0.0446)	0.1823*** (0.0449)	0.0698** (0.0323)
$\ell \in [4, 6]$	-0.3276*** (0.0508)	-0.2714*** (0.0653)	-0.2433*** (0.0473)	-0.2724*** (0.0486)	-0.2051*** (0.0344)
$\ell \in [7, 12]$	-0.7330*** (0.0532)	-0.7587*** (0.0686)	-0.6049*** (0.0497)	-0.7635*** (0.0533)	-0.5527*** (0.0368)
$\ell \in [13, 24]$	-0.2832*** (0.0451)	-0.2090*** (0.0548)	-0.2209*** (0.0426)	-0.2222*** (0.0309)	-0.2010*** (0.0241)
$\ell \in [25, 36]$	-0.0642 (0.0525)	-0.0034 (0.0638)	-0.0616 (0.0484)	-0.0428 (0.0363)	-0.0848*** (0.0250)
Post-period IW average	-0.2570*** (0.0438)	-0.1970*** (0.0537)	-0.2084*** (0.0414)	-0.2153*** (0.0279)	-0.1928*** (0.0215)
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	No	No
Industry FE	No	No	No	Yes	Yes
Baseline-complete sample	No	Yes	No	Yes	No
Baseline controls	No	No	No	Yes	No
Lagged controls	No	No	Yes	No	Yes
Observations	229,828	167,279	225,235	167,279	225,235
Stocks	3,236	2,084	3,208	2,084	3,208

Notes: The dependent variable is monthly ambiguity. Relative time is measured in months from first Stock Connect eligibility, with $\ell = -1$ omitted. The pre-treatment bin is $[-24, -2]$. Post-treatment effects are reported for 0, $[1, 3]$, $[4, 6]$, $[7, 12]$, $[13, 24]$, and $[25, 36]$. The post-period IW average is the equal-weight average of the monthly IW estimates over event months 0 through 36. Columns (1)–(2) use stock and year-month fixed effects; column (2) differs from column (1) only through the baseline-complete sample restriction. Columns (3) and (5) include one-month- lagged stock characteristics and are reported as sensitivity specifications. Column (4) includes predetermined baseline characteristics with industry and year-month fixed effects. Standard errors are clustered by stock. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

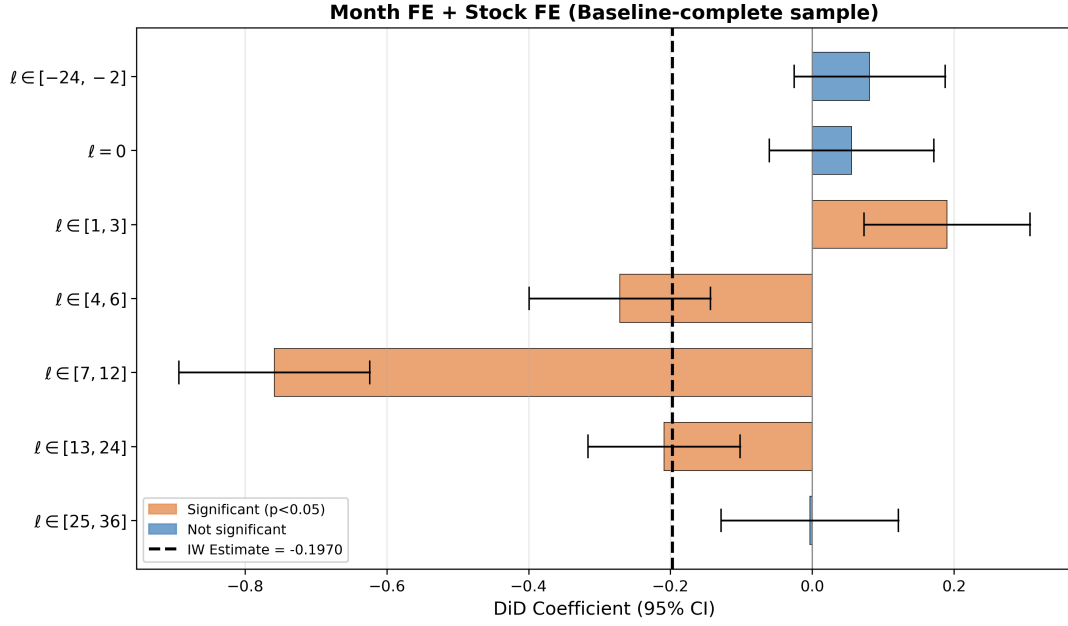


Figure 3: Binned interaction-weighted event-study estimates (Specification (2))

5.3.2 Pricing tests: the ambiguity premium and eligibility

Table 7 reports the pricing tests based on Equation (7). Column (1) includes the core ambiguity variables and the Stock Connect eligibility indicator. Columns (2)–(4) progressively add standard return predictors: size and book-to-market in Column (2), momentum and reversal in Column (3), and volatility and illiquidity in Column (4). All specifications include year-month and industry fixed effects, and standard errors are clustered at the stock level.

Table 7: Pricing tests: ambiguity and Stock Connect eligibility

	(1)	(2)	(3)	(4)
$\mathcal{U}^2$	-0.3549*** (0.0466)	-0.2865*** (0.0478)	-0.2627*** (0.0488)	-0.1795*** (0.0552)
$\mathcal{U}^2 \times \text{SC}$	-0.5498*** (0.0565)	-0.5682*** (0.0569)	-0.5600*** (0.0570)	-0.5478*** (0.0573)
SC	-0.4637*** (0.0381)	0.0460 (0.0487)	0.0493 (0.0488)	0.0419 (0.0485)
<i>Size</i>		-0.5878*** (0.0245)	-0.5918*** (0.0249)	-0.5237*** (0.0289)
<i>Book-to-Market</i>		0.4080*** (0.0319)	0.4199*** (0.0322)	0.4056*** (0.0323)
<i>Momentum</i>			0.1247*** (0.0385)	0.1457*** (0.0391)
<i>Reversal</i>			-1.0158*** (0.2261)	-0.9095*** (0.2193)
<i>Volatility</i>				-0.0801*** (0.0257)
<i>ILLIQ</i>				0.6943*** (0.1510)
R^2	0.3437	0.3453	0.3454	0.3457
Observations	468,438	468,438	468,438	468,438

Notes: The dependent variable is next-month excess return in percentage points. The ambiguity measure $\mathcal{U}^2$ and ILLIQ are standardized. All regressions include industry and year-month fixed effects. Standard errors, clustered by stock, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

The coefficients on ambiguity should be interpreted as reduced-form net prices of measured ambiguity. They need not reflect a single pricing force. In particular, a negative ambiguity-return relation is consistent with ambiguity-seeking or lottery-type demand: investors may bid up stocks with more ambiguous payoff distributions, causing these stocks to earn lower subsequent returns. At the same time, ambiguity-averse investors may require compensation for holding stocks with residual ambiguity, which works in the opposite direction. The estimated ambiguity coefficient therefore captures the net effect of these forces, rather than separately identifying them.

The baseline ambiguity coefficient, β_1 , measures the conditional ambiguity premium among stocks that are not eligible for Stock Connect in a given month, including both never-eligible stocks and stocks before their first eligibility. Across all specifications, β_1 is negative and statistically significant. In the fully specified model in Column (4), a one-standard-deviation increase in ambiguity is associated with a 0.1795 percentage-point lower excess return in the

following month. This negative estimate is consistent with China-specific evidence on ambiguity pricing. [Yuan et al. \(2024\)](#) document a negative individual-stock ambiguity premium in the Chinese A-share market and interpret it through heterogeneous ambiguity aversion and limited market participation. As stock-level ambiguity rises, more ambiguity-averse investors may withdraw from trading, leaving a marginal investor base that is less ambiguity-averse, or even ambiguity-seeking, and therefore willing to accept lower ambiguity premia. In China’s segmented and retail-dominated A-share market, this investor-base channel may be reinforced by speculative or lottery-type demand: high-ambiguity stocks may attract investors who are willing to pay for probability uncertainty, pushing up contemporaneous prices and lowering subsequent returns. This interpretation is consistent with China’s lottery-preference literature, which shows that retail demand for lottery-like payoff exposure can lead to lower subsequent returns ([Zhou et al., 2025](#)), and with broader evidence that speculative retail trading is concentrated in lottery-like stocks and is associated with overpricing ([Han and Kumar, 2013](#)).

The magnitude of β_1 attenuates as controls are added, from -0.3549 in Column (1) to -0.1795 in Column (4). The first sizable attenuation occurs when the size and book-to-market controls are introduced in Column (2), suggesting that part of the raw ambiguity premium overlaps with standard firm characteristics related to scale and valuation. Further attenuation after adding volatility and illiquidity indicates that ambiguity is also correlated with risk and trading-friction proxies. Importantly, however, β_1 remains negative and statistically significant in the fully controlled specification, indicating that ambiguity retains independent predictive content for future returns.

Our coefficient of primary interest is β_2 , the interaction between ambiguity and Stock Connect eligibility. Across all four specifications, β_2 is consistently negative and highly significant, ranging from -0.5478 to -0.5682 . Since the conditional ambiguity premium after a stock becomes eligible for Stock Connect is $\beta_1 + \beta_2$, the negative interaction implies that eligibility makes the ambiguity premium more negative. In Column (4), the post-eligibility ambiguity premium is approximately -0.7273 , compared with -0.1795 before eligibility or among stocks that never become eligible.

This pattern suggests that Stock Connect changes not only the level of ambiguity but also the pricing environment for the remaining ambiguity. One possible explanation is that eligibility weakens the positive compensation component required by ambiguity-averse investors. Stock Connect improves information transmission, trading access, and market integration, thereby reducing the compensation required for bearing a given unit of stock-level ambiguity measured by $\mathcal{U}^2$. A source-dependent view of ambiguity attitudes provides one way to rationalize this effect ([Baillon et al., 2025](#)). The ambiguity index captures instability in return probabilities, but the compensation attached to a given unit of measured ambiguity may depend on the market environment in which that ambiguity is embedded. For non-eligible stocks, a high value of $\mathcal{U}^2$ is more likely to be associated with local information opacity, segmented trading, limited investor access, and higher exit risk. For Stock Connect eligible stocks, the same value of $\mathcal{U}^2$ is observed in a more integrated, visible, and liquid trading environment. Ambiguity-averse investors may therefore attach a less pessimistic valuation to this source of uncertainty and require less positive compensation for bearing it. If ambiguity-seeking demand remains present, a decline in this

offsetting compensation component can make the net ambiguity-return relation more negative among eligible stocks.

A complementary demand-side channel may also contribute to the stronger negative interaction. Eligible stocks are typically more visible, more liquid, and easier to access. These features may attract greater investor attention and may concentrate trading by investors who value ambiguous upside or lottery-like payoffs. Under this interpretation, eligibility can amplify the negative ambiguity-seeking component of the premium. We therefore view the interaction result as consistent with a change in the balance between ambiguity-aversion-based compensation and ambiguity-seeking demand.

The Stock Connect inclusion dummy, β_3 , is negative and significant in Column (1), but becomes insignificant once firm characteristics are controlled for in Columns (2)–(4). This suggests that firm characteristics, such as size and liquidity, help account for the conditional return difference between eligible and non-eligible stocks at the sample mean of ambiguity, where standardized ambiguity equals zero. At this ambiguity level, we find no statistically significant evidence that eligible stocks earn systematically different returns from non-eligible stocks after controlling for these characteristics. The significant negative interaction, β_2 , points to an ambiguity-pricing channel, with eligibility associated with a stronger negative ambiguity premium.

The estimated coefficients on the control variables are broadly consistent with standard cross-sectional return patterns documented in the asset-pricing literature, including the size, value, momentum, reversal, volatility, and liquidity effects (e.g., [Fama and French, 1992, 1993](#); [Jegadeesh and Titman, 1993](#); [Jegadeesh, 1990](#); [Ang et al., 2006](#); [Amihud, 2002](#)). More importantly, the interaction coefficient β_2 remains negative and highly significant after these controls are included, indicating that the differential ambiguity premium associated with Stock Connect eligibility is not absorbed by standard firm characteristics, return dynamics, or risk and liquidity controls.

Overall, Table 7 shows that ambiguity negatively predicts future excess returns in China’s A-share market and that this negative ambiguity premium becomes stronger after Stock Connect eligibility. The evidence is consistent with the view that market integration changes the demand and pricing environment for high-ambiguity stocks, rather than simply reducing the level of ambiguity.

6 Mechanisms: Liquidity and Global Linkage

This section provides mechanism-oriented diagnostics for the ambiguity decline documented in Section 5.3.1. We study (i) a market-quality channel, whether ambiguity falls more for stocks that were ex ante less liquid, and (ii) a global-linkage channel, which we interpret as a metamorphosis in the nature of ambiguity: eligible stocks’ ambiguity becomes more sensitive to global uncertainty after integration.

6.1 Liquidity / price-efficiency channel

This subsection evaluates whether the post-STOCK CONNECT ambiguity decline documented in Section 5 is consistent with a market-quality mechanism. The liquidity-channel prediction is *heterogeneous*: ambiguity should fall more for stocks that were initially less liquid, because these stocks have more scope for reductions in trading frictions and for improvements in price efficiency. Consistent with this market-quality perspective, Xu et al. (2020) document that the Shanghai–Hong Kong Stock Connect narrows quoted and effective spreads, while being associated with lower market depth and higher short-term volatility.

We first test whether STOCK CONNECT eligibility is associated with changes in implementable liquidity proxies constructed from daily prices and volume. We focus on turnover, bid–ask spread, and the Amihud price-impact measure.⁹ For each liquidity outcome $L_{i,t}$, we estimate:

$$L_{i,t} = \alpha_i + \lambda_t + \beta_L SC_{i,t} + \mathbf{X}'_{i,t-1} \boldsymbol{\theta}_L + \varepsilon_{i,t}, \quad (8)$$

where α_i are stock fixed effects, λ_t are year-month fixed effects, and $\mathbf{X}_{i,t-1}$ is a lagged control set $\mathbf{X}_{i,t-1} = [\log(ME_{i,t-1}), \log(RV_{i,t-1}), IVOL_{i,t-1}]$. All regressions are estimated with absorbed fixed effects and standard errors clustered by stock.

Table 8 reports the average first-stage association in the full sample. The estimates do not support a uniform improvement across all liquidity dimensions. The spread effect is negative and statistically significant, consistent with narrower transaction costs after eligibility. By contrast, turnover falls and the Amihud price-impact measure increases. Thus, the first-stage evidence is best interpreted as a spread-based reduction in trading frictions rather than as a broad improvement in all liquidity proxies.

Table 8: STOCK CONNECT eligibility and liquidity proxies

Outcome $L_{i,t}$	$\hat{\beta}_L$ on $SC_{i,t}$	t -stat
Turnover	−5.0590***	−12.44
Amihud ILLIQ	1.9663e-10***	7.45
Bid–ask spread	−0.001188***	−10.12

Notes: Each row reports a separate regression of $L_{i,t}$ on $SC_{i,t}$ using (8). All regressions include absorbed stock and year-month fixed effects and the lagged control set $\mathbf{X}_{i,t-1} = [\log(ME_{i,t-1}), \log(RV_{i,t-1}), IVOL_{i,t-1}]$. Standard errors are clustered by stock. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

To connect the mechanism to baseline liquidity, we re-estimate (8) separately within terciles of $ILLIQ_i^{pre}$. To avoid post-treatment endogeneity, we construct a pre-treatment baseline illiquidity measure: $ILLIQ_i^{pre} \equiv \text{median}(ILLIQ_{i,t})$ over months $t < \text{Nov 2014}$. Table 9 shows

⁹The Amihud measure is extremely small in levels in our data (order of magnitude 10^{-9}). We therefore treat its sign and statistical significance as diagnostics and rely primarily on spread and turnover as more interpretable proxies.

that the spread-narrowing effect is largest for the high- $ILLIQ^{pre}$ tercile (initially least liquid stocks), consistent with a market-quality channel for this proxy. Turnover declines are also largest for the high- $ILLIQ^{pre}$ tercile.

Table 9: STOCK CONNECT eligibility and liquidity proxies by baseline illiquidity terciles

Outcome $L_{i,t}$	Low $ILLIQ^{pre}$	Medium $ILLIQ^{pre}$	High $ILLIQ^{pre}$
Turnover	-1.1458 ($t = -1.27$)	-0.4829 ($t = -0.69$)	-2.7044*** ($t = -4.18$)
Amihud ILLIQ	2.9794e-11 ($t = 0.45$)	-9.2710e-12 ($t = -0.21$)	5.6716e-11 ($t = 1.10$)
Bid-ask spread	-0.000425* ($t = -1.69$)	-0.000720*** ($t = -3.54$)	-0.001499*** ($t = -6.06$)

Notes: Each cell reports $\hat{\beta}_L$ from (8) estimated separately within terciles of $ILLIQ_i^{pre}$. Regressions include absorbed stock and year-month fixed effects and the lagged control set $\mathbf{X}_{i,t-1} = [\log(ME_{i,t-1}), \log(RV_{i,t-1}), IVOL_{i,t-1}]$. Standard errors are clustered by stock. Stars reflect the corresponding p -values.

Table 9 shows that the first-stage evidence is concentrated in the spread proxy. The bid-ask spread declines in all three baseline-illiquidity terciles, with the largest estimate in the high- $ILLIQ^{pre}$ tercile. By contrast, the Amihud measure is statistically insignificant in all terciles, and turnover declines significantly only among stocks in the high- $ILLIQ^{pre}$ tercile. This pattern cautions against interpreting the results as evidence of a broad liquidity channel. Instead, the observable first-stage evidence points primarily to a reduction in spread-based transaction costs. To summarize heterogeneity parsimoniously, we also estimate an interaction specification using a winsorized-and-standardized baseline illiquidity score $ILLIQ_i^{pre,z}$ computed at the stock level.¹⁰

$$\mathcal{U}_{i,t}^2 = \alpha_i + \lambda_t + \beta SC_{i,t} + \eta(SC_{i,t} \times ILLIQ_i^{pre,z}) + \mathbf{X}_{i,t-1}'\boldsymbol{\theta}_M + \varepsilon_{i,t}. \quad (9)$$

Here $ILLIQ_i^{pre,z}$ is absorbed by stock fixed effects, while η is identified from within-stock changes in $SC_{i,t}$ interacted with cross-sectional baseline illiquidity. Table 10 reports the interaction estimates for the main ambiguity outcome $\mathcal{U}^2$. The negative and statistically significant $\hat{\eta}$ indicates larger ambiguity declines for stocks that were more illiquid prior to STOCK CONNECT.

Table 10: Interaction specification: baseline illiquidity amplifies ambiguity declines

Outcome	$\hat{\beta}$ on $SC_{i,t}$	$\hat{\eta}$ on $SC_{i,t} \times ILLIQ_i^{pre,z}$	N
$\mathcal{U}^2$	-0.1914*** (0.01879)	-0.07628*** (0.01224)	420,579

Notes: Estimates are from (9) with absorbed stock and year-month fixed effects and lagged controls $\mathbf{X}_{i,t-1} = [\log(ME_{i,t-1}), \log(RV_{i,t-1}), IVOL_{i,t-1}]$. $ILLIQ_i^{pre,z}$ is baseline illiquidity winsorized at the 1st and 99th percentiles and standardized at the stock level. Standard errors (in parentheses) are clustered by stock. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

¹⁰winsorized at the 1st and 99th percentiles and standardized to mean zero and unit standard deviation across stocks

6.2 Global/systemic exposure channel

Market integration can increase exposure to *global* sources of uncertainty, because connected stocks attract cross-border investors and become more sensitive to global news and policy shocks. This makes the global-comovement channel especially relevant in an environment of elevated policy uncertainty and renewed attention to financial fragmentation (IMF, 2023; AFA, 2025). Figure 4 plots standardized eligible-portfolio (defined by current eligibility) ambiguity alongside standardized global uncertainty proxies.

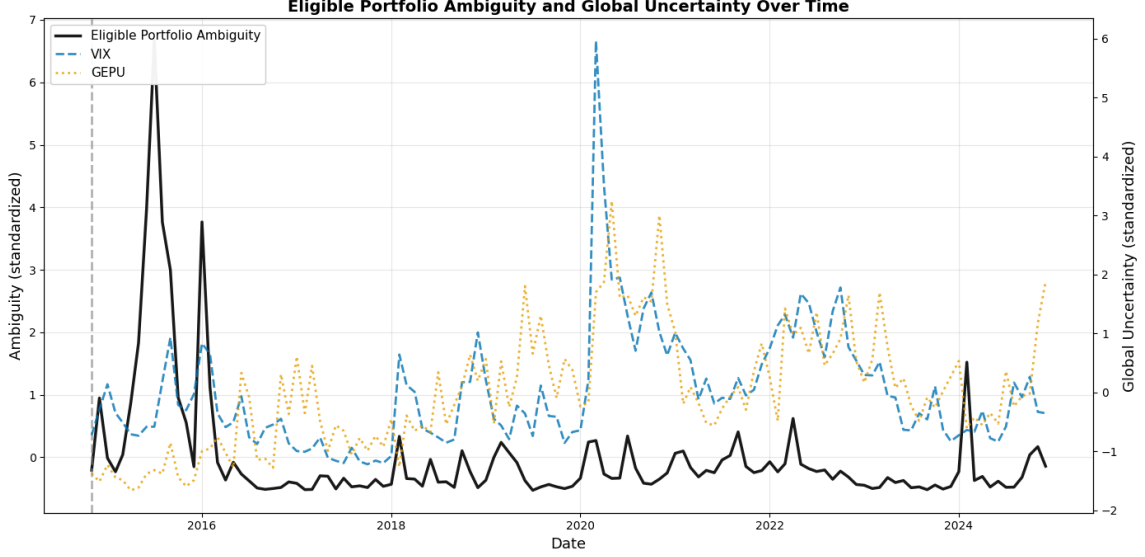


Figure 4: Eligible-portfolio ambiguity and global uncertainty

Motivated by this integration logic, we next test whether eligible stocks become more globally linked in terms of ambiguity comovement. Let VIX_t^{std} denote the standardized monthly VIX series and $GEPU_t^{std}$ the standardized monthly Global EPU series. With year-month fixed effects λ_t , the *levels* of global uncertainty are absorbed; thus identification comes from interactions with stock-level eligibility:

$$\mathcal{U}_{i,t}^2 = \alpha_i + \lambda_t + \beta SC_{i,t} + \psi_V(SC_{i,t} \times VIX_t^{std}) + \psi_G(SC_{i,t} \times GEPU_t^{std}) + \mathbf{X}_{i,t-1}'\boldsymbol{\theta}_G + \varepsilon_{i,t}. \quad (10)$$

The interaction coefficients (ψ_V, ψ_G) measure whether eligible stocks' ambiguity is differentially sensitive to global shocks relative to non-eligible stocks in the same month.

Table 11 reports estimates of (10). When included alone, the VIX interaction is small and statistically insignificant (column (1)), while the GEPU interaction is positive and highly significant (column (2)). When both interactions are included jointly (column (3)), the GEPU interaction remains positive and significant, while the VIX interaction becomes significantly negative. As a robustness check, column (4) uses the non-squared ambiguity measure $\mathcal{U}_{i,t}$ instead of the baseline squared ambiguity outcome $\mathcal{U}_{i,t}^2$. Results are similar using the alternative ambiguity outcome (column (4)).

Table 11: Metamorphosis test: global uncertainty linkage

	(1) VIX	(2) GEPU	(3) Both	(4) Alt. outcome
$SC_{i,t}$	-0.1511*** (0.0163)	-0.1860*** (0.0172)	-0.2167*** (0.0187)	-0.0583*** (0.0050)
$SC_{i,t} \times VIX_t^{std}$	-0.0086 (0.0080)		-0.0919*** (0.0116)	-0.0102*** (0.0033)
$SC_{i,t} \times GEPU_t^{std}$		0.0983*** (0.0101)	0.1511*** (0.0138)	0.0211*** (0.0036)
Year-Month FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes
Dep. var.	$\mathcal{U}^2$	$\mathcal{U}^2$	$\mathcal{U}^2$	$\mathcal{U}$
Obs.	505,899	505,899	505,899	505,899
R^2 (within)	0.0913	0.0916	0.0918	0.1293

Notes: Global uncertainty proxies are standardized to mean zero and unit variance. With year-month fixed effects, the main effects of VIX_t^{std} and $GEPU_t^{std}$ are absorbed; reported coefficients are interaction (differential-slope) effects. Controls are lagged by one month and correspond to $\mathbf{X}_{i,t-1} = [\log(ME_{i,t-1}), \log(RV_{i,t-1}), IVOL_{i,t-1}]$. Standard errors (in parentheses) are clustered by stock. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

To clarify why the VIX interaction changes between the separate and joint specifications, we examine the correlation between the two global uncertainty measures. The monthly correlation between VIX and GEPU over the Table 11 estimation window is 0.276. More importantly for the interaction specification, the correlation between $SC_{i,t} \times VIX_t^{std}$ and $SC_{i,t} \times GEPU_t^{std}$, after partialling out the stock and year-month fixed effects and the regression controls, is 0.563. This indicates moderate overlap but not severe multicollinearity. The difference between columns (1) and (3) instead reflects the distinction between the separate and partial effects of the two uncertainty measures. In particular, the joint VIX coefficient of -0.0919 receives an omitted-GEPU component of $+0.0833$ when GEPU is excluded, reproducing the standalone estimate of -0.0086 . Thus, column (1) captures both the VIX-specific component and the component shared with GEPU, whereas column (3) isolates their partial differential associations with ambiguity.

We also examine whether the interaction results reflect persistent differences between stocks that eventually become eligible for STOCK CONNECT and other stocks. Such differences may arise because eligible stocks tend to be larger, more internationalized, more closely followed by institutions, or more likely to be index constituents. We augment (10) with interactions between an $EverTreated_i$ indicator and both global uncertainty measures. This specification allows stocks that eventually become eligible for STOCK CONNECT to have different global uncertainty sensitivities regardless of the timing of their actual eligibility. The $EverTreated_i \times GEPU_t^{std}$ coefficient is positive and significant (0.0650), indicating that eventually eligible stocks exhibit greater GEPU sensitivity even apart from the timing of eligibility. However, after allowing for

this persistent stock-type difference, the post-eligibility $SC_{i,t} \times GEPU_t^{std}$ coefficient remains positive and significant (0.1102), while $SC_{i,t} \times VIX_t^{std}$ remains negative and significant (-0.0817). We therefore interpret the global-linkage evidence cautiously: part of the GEPU sensitivity reflects persistent differences in the types of stocks that become eligible, but these differences do not fully account for the additional change in uncertainty sensitivity following STOCK CONNECT eligibility.

7 Conclusion

This paper studies whether equity-market integration reduces ambiguity and whether integration changes how ambiguity is priced. We use China’s Stock Connect program, which expanded Northbound access to A-shares through staggered, largely rule-based eligibility updates. Our ambiguity proxy is the Brenner–Izhakian ambiguity measure $\mathcal{U}_{i,t}^2$ constructed from intraday returns (Brenner and Izhakian, 2018; Izhakian, 2020). The institutional timing of eligibility, together with panel designs that absorb fixed effects and heterogeneous-treatment-effect-robust staggered estimators, allows us to estimate the causal effect of market opening on probability instability (Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; de Chaisemartin and D’Haultfoeuille, 2020; Sun and Abraham, 2021; Roth et al., 2023).

We find that Stock Connect eligibility is associated with a statistically and economically meaningful decline in $\mathcal{U}_{i,t}^2$. This result is robust in conventional TWFE regressions and in Sun–Abraham interaction-weighted event studies, which are valid under staggered adoption with heterogeneous effects (Sun and Abraham, 2021). The dynamic estimates indicate that the decline is not immediate: effects around the eligibility month are small and in some specifications mildly positive, while the reduction emerges after a short lag, becomes strongest at intermediate horizons, and attenuates at longer horizons. Overall, the pattern points to a persistent post-integration adjustment rather than a purely on-impact response.

We also provide evidence on mechanisms, but the results are mixed. On the one hand, the ambiguity decline is stronger for stocks that were ex ante more illiquid, and the standardized interaction estimates imply a significantly larger post-eligibility ambiguity drop for those stocks. On the other hand, the first-stage liquidity proxies do not move uniformly in the expected “improved liquidity” direction: spreads narrow, but Amihud-based measures increase and turnover falls. We therefore interpret the evidence as suggestive of market-quality heterogeneity, with the observable first-stage channel operating mainly through spread-based trading frictions rather than through a broad improvement in all liquidity dimensions. At the same time, we find evidence that integration shifts the composition of ambiguity toward greater sensitivity to global uncertainty shocks, consistent with the broader idea that market opening can reallocate uncertainty rather than simply reduce it (Jurado et al., 2015; Rey, 2015; Baker et al., 2016).

Finally, integration is associated with a substantial change in the pricing of ambiguity. The ambiguity–return relation is negative in the segmented market and becomes significantly more negative for eligible stocks after inclusion. We interpret this result as a change in the net price of measured ambiguity. One possible explanation is that eligibility weakens the positive compensation component required by ambiguity-averse investors, because the same value of $\mathcal{U}^2$

is observed in a more integrated, visible, and liquid trading environment. This interpretation is consistent with a source-dependent view of ambiguity attitudes, under which attitudes toward uncertainty can depend on its source (Baillon et al., 2025); consequently, a change in the market environment may also alter the compensation investors require for a given degree of ambiguity. A complementary channel is that eligibility may increase visibility and accessibility, thereby attracting greater lottery-type or ambiguity-seeking demand for high-ambiguity stocks (Yuan et al., 2024; Zhou et al., 2025). We do not separately identify these channels. The evidence instead shows that market opening changes the pricing environment for probability instability: it can reduce the level of ambiguity while making the remaining ambiguity more negatively priced.

Several limitations are worth noting. First, while eligibility updates provide quasi-experimental timing variation, treatment is not random: eligibility depends on observables such as size and liquidity. We address this through fixed effects, predetermined controls, modern staggered estimators, and pre-trend diagnostics, but residual concerns about differential trends motivate complementary designs. Second, $\mathcal{U}_{i,t}^2$ is a market-based measure of probability instability; it does not directly identify the beliefs of specific investor groups. Linking eligibility to ownership composition, Northbound flows, or broker-level trading would help map changes in the investor base into belief instability and its compensation. Third, our baseline treatment is absorbing and timed by the effective eligibility month rather than the announcement month; future work can incorporate removals, sell-only designations, and announcement dates to better capture anticipation and richer institutional dynamics.

More broadly, the results show that market integration is not a one-dimensional reduction in informational frictions. The classic liberalization literature emphasizes lower segmentation, lower costs of capital, and improved market functioning after opening (Henry, 2000; Bekaert and Harvey, 2000; Bekaert et al., 2005; Hoang and Mateus, 2024). Our evidence adds a complementary insight. In this setting, market opening compresses a theoretically grounded measure of local probability instability while reallocating ambiguity toward global, policy-driven sources and altering how ambiguity is priced. This interpretation fits naturally with work that highlights the role of information quality in ambiguity (Epstein and Schneider, 2008) and with the broader uncertainty literature, which distinguishes policy and macro uncertainty from simple volatility (Baker et al., 2016; Jurado et al., 2015). Understanding this tradeoff is important for evaluating the welfare and policy consequences of capital-market opening, particularly in emerging markets where institutional features of trading can materially shape how investors form and update probabilistic beliefs.

Appendix A Additional Figures

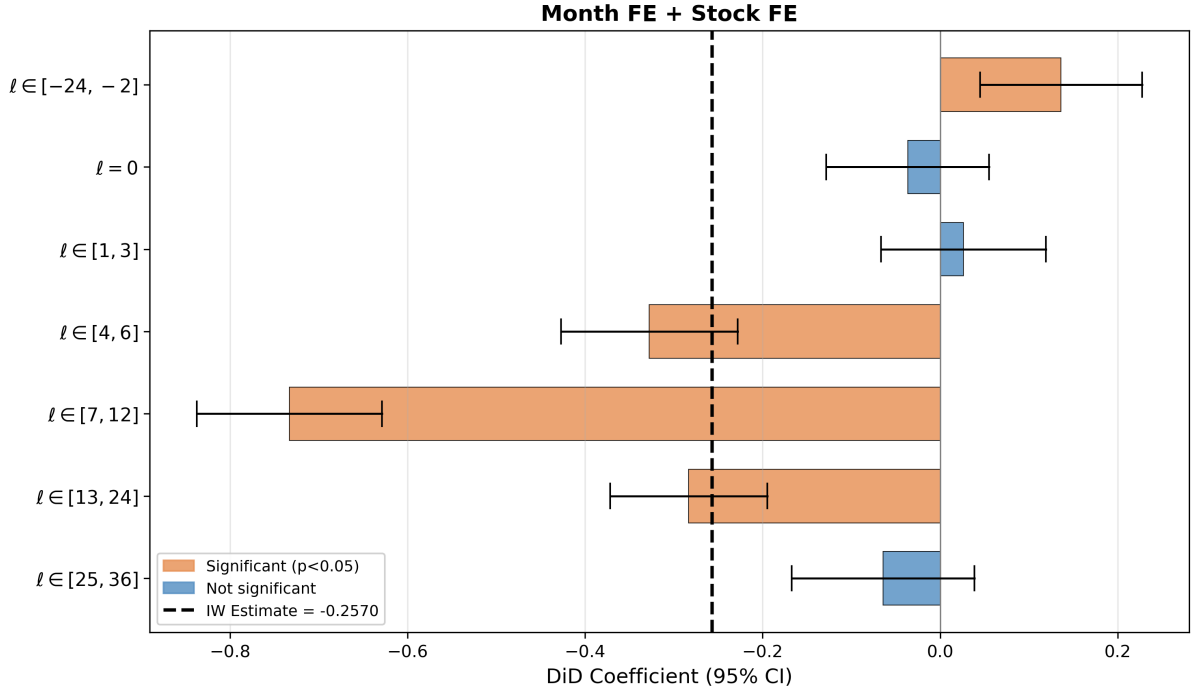


Figure A1: STOCK CONNECT eligibility and ambiguity: calendar-year-specific effects under Table 6 column (1) (Year-Month FE + Stock FE; no controls). The figure plots year-specific eligibility coefficients from a regression interacting $SC_{i,t}$ with calendar-year indicators, using the same sample and fixed-effect structure as column (1). Whiskers are 95% confidence intervals. The red dashed line reports the corresponding average of the monthly IW estimates.

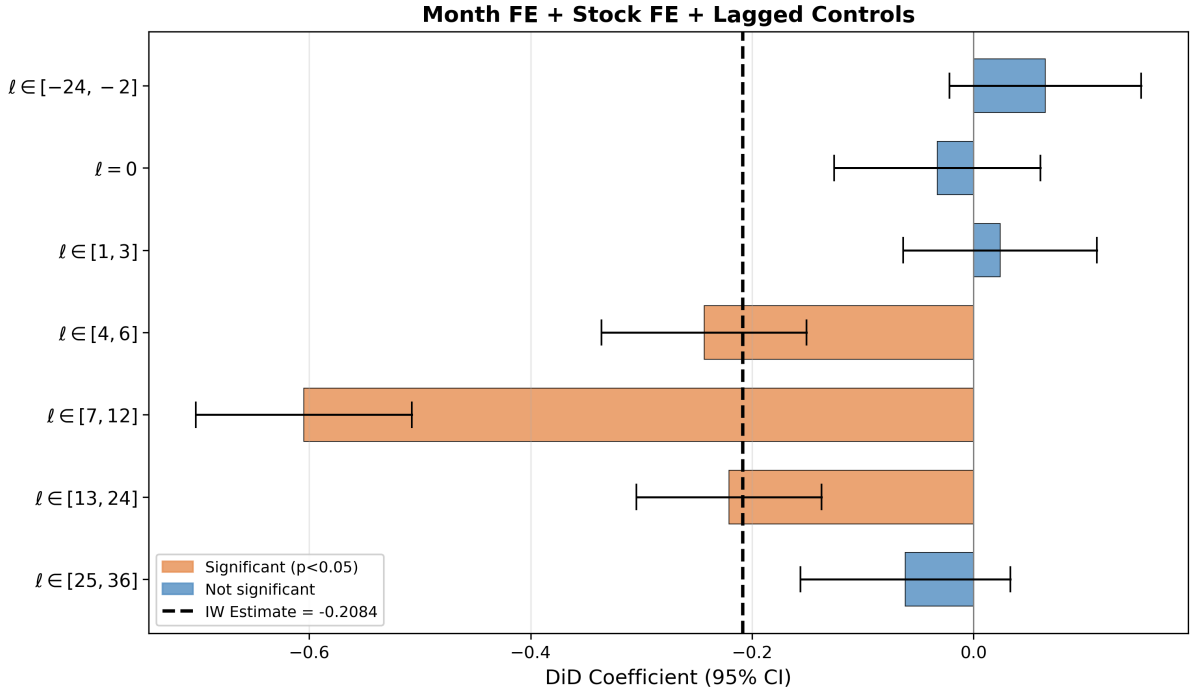


Figure A2: STOCK CONNECT eligibility and ambiguity: calendar-year-specific effects under Table 6 column (3) (Year-Month FE + Stock FE + lagged controls). The figure plots year-specific eligibility coefficients from a regression interacting $SC_{i,t}$ with calendar-year indicators, using the same sample, fixed effects, and current control set as column (3). Whiskers are 95% confidence intervals. The red dashed line reports the corresponding average of the monthly IW estimates.

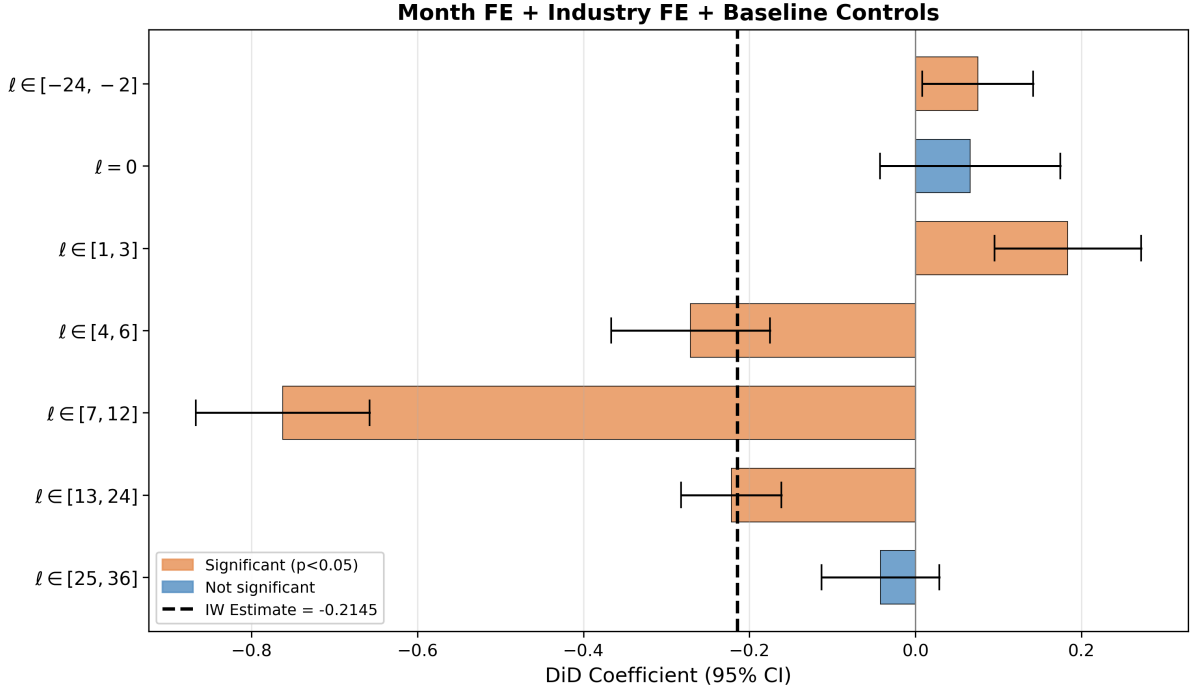


Figure A3: STOCK CONNECT eligibility and ambiguity: calendar-year-specific effects under Table 6 column (4) (Year-Month FE + Industry FE + baseline controls). The figure plots year-specific eligibility coefficients from a regression interacting $SC_{i,t}$ with calendar-year indicators, using the same sample, fixed effects, and baseline control set as column (4). Whiskers are 95% confidence intervals. The red dashed line reports the corresponding average of the monthly IW estimates.

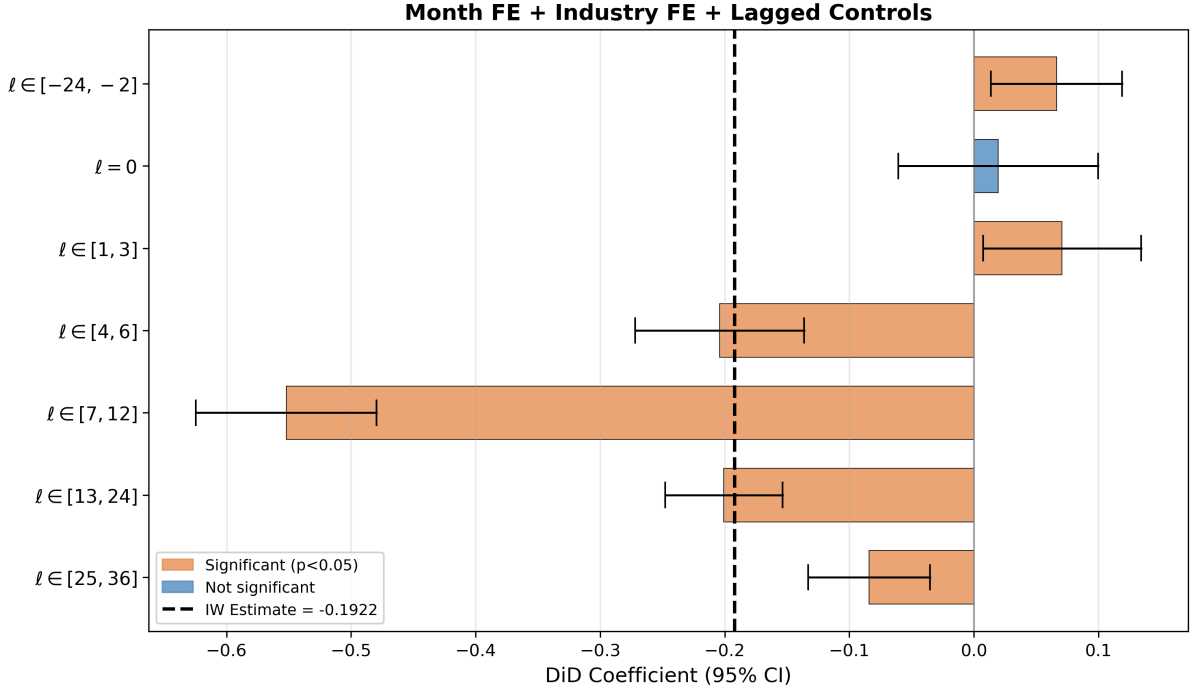


Figure A4: STOCK CONNECT eligibility and ambiguity: calendar-year-specific effects under Table 6 column (5) (Year-Month FE + Industry FE + lagged controls). The figure plots year-specific eligibility coefficients from a regression interacting $SC_{i,t}$ with calendar-year indicators, using the same sample, fixed effects, and current control set as column (5). Whiskers are 95% confidence intervals. The red dashed line reports the corresponding average of the monthly IW estimates.

Appendix B Callaway–Sant’Anna Robustness Analysis

As an additional robustness check, we re-estimate the treatment effects using the group-time ATT estimator of Callaway and Sant’Anna (2021), with not-yet-treated stocks serving as the comparison group. We consider the full sample, the baseline-complete sample, and a covariate-adjusted doubly robust specification using predetermined baseline characteristics.

Table B1: Callaway–Sant’Anna estimates: static and dynamic effects

	(1)	(2)	(3)
<i>Panel A: Overall treatment effect</i>			
Overall ATT [0, 36]	-0.3062*** (0.0383)	-0.2019*** (0.0443)	-0.2401*** (0.0559)
<i>Panel B: Pre-treatment dynamics</i>			
$\ell \in [-24, -13]$	0.1590*** (0.0439)	0.1889*** (0.0485)	0.0008 (0.0926)
$\ell \in [-12, -7]$	0.0044 (0.0539)	-0.1585** (0.0632)	0.0345 (0.1494)
$\ell \in [-6, -2]$	0.0668 (0.0448)	-0.0064 (0.0532)	0.0494 (0.1164)
<i>Panel C: Post-treatment dynamics</i>			
$\ell = 0$	-0.0231 (0.0431)	0.1070** (0.0521)	0.0729 (0.0699)
$\ell \in [1, 3]$	-0.0620 (0.0427)	0.0925* (0.0496)	-0.0597 (0.0628)
$\ell \in [4, 6]$	-0.3394*** (0.0446)	-0.2811*** (0.0532)	-0.1979*** (0.0725)
$\ell \in [7, 12]$	-0.6945*** (0.0494)	-0.6548*** (0.0611)	-0.3143*** (0.0802)
$\ell \in [13, 24]$	-0.3218*** (0.0410)	-0.2049*** (0.0477)	-0.3430*** (0.0729)
$\ell \in [25, 36]$	-0.1449*** (0.0457)	-0.0329 (0.0522)	-0.2530** (0.1094)
Sample	Full	Baseline-complete	Baseline-complete
Baseline covariates	No	No	Yes
Comparison group	Not-yet	Not-yet	Not-yet
Observations	507,198	422,769	422,769
Stocks	3,345	2,191	2,191

Notes: Group-time DID estimates Group-time DID estimates follow [Callaway and Sant’Anna \(2021\)](#), using not-yet-treated stocks as the comparison group. Column (3) additionally uses doubly robust adjustment with predetermined baseline characteristics. Event month -1 is the reference period. Binned standard errors account for covariance across the underlying monthly estimates. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure B1 provides a more disaggregated view of the pre-treatment period for the baseline-adjusted doubly robust specification. The monthly estimates vary in sign and show no persistent directional pattern.

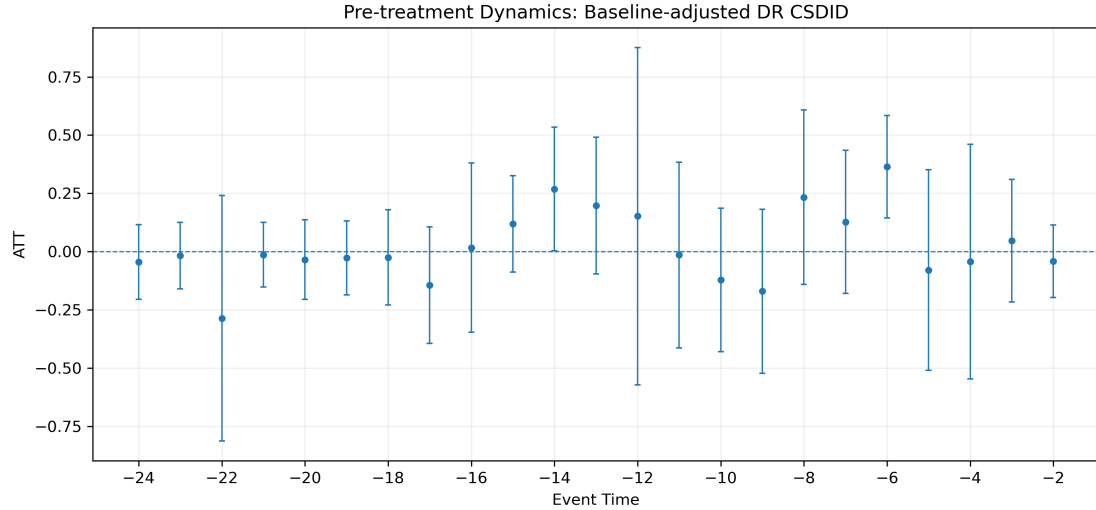


Figure B1: Pre-treatment dynamics: baseline-adjusted doubly robust CSDID estimates
Notes: Group-time DID estimates follow [Callaway and Sant’Anna \(2021\)](#), with not-yet-treated stocks as the comparison group. Column (3) additionally uses doubly robust adjustment with predetermined baseline characteristics. Event month -1 is the reference period. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

References

- American Finance Association (AFA) (2025). AFA Panel: Geopolitical Fragmentation. ASSA Annual Meeting program session, San Francisco, 3 January 2025.
- Amihud, Y. (2002). Illiquidity and stock returns: Cross-section and time-series effects. *Journal of Financial Markets*, 5(1):31–56.
- Andersen, T. G., Bollerslev, T., Diebold, F. X., and Ebens, H. (2001). The distribution of realized stock return volatility. *Journal of Financial Economics*, 61(1):43–76.
- Ang, A., Hodrick, R. J., Xing, Y., and Zhang, X. (2006). The cross-section of volatility and expected returns. *Journal of Finance*, 61(1):259–299.
- Baillon, A., Bleichrodt, H., Li, C., and Wakker, P. P. (2025). Source theory: A tractable and positive ambiguity theory. *Management Science*, 71(10):8767–8782.
- Baker, S. R., Bloom, N., and Davis, S. J. (2016). Measuring economic policy uncertainty. *Quarterly Journal of Economics*, 131(4):1593–1636.
- Bekaert, G. and Harvey, C. R. (2000). Foreign speculators and emerging equity markets. *Journal of Finance*, 55(2):565–613.
- Bekaert, G., Harvey, C. R., and Lundblad, C. T. (2005). Does financial liberalization spur growth? *Journal of Financial Economics*, 77(1):3–55.
- Bloom, N. (2009). The impact of uncertainty shocks. *Econometrica*, 77(3):623–685.
- Braghieri, L., Levy, R., and Makarin, A. (2022). Social media and mental health. *American Economic Review*, 112(11):3660–3693.

- Brenner, M. and Izhakian, Y. (2018). Asset pricing and ambiguity: Empirical evidence. *Journal of Financial Economics*, 130(3):503–531.
- Callaway, B. and Sant’Anna, P. H. C. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.
- Chen, Z. and Epstein, L. G. (2002). Ambiguity, risk, and asset returns in continuous time. *Econometrica*, 70(4):1403–1443.
- Cheng, A. W.-W., Chow, N. S.-C., Chui, D. K.-H., and Wong, W.-K. (2019). The three musketeers relationships between Hong Kong, Shanghai and Shenzhen before and after Shanghai–Hong Kong stock connect. *Sustainability*, 11(14):3845.
- China Securities Regulatory Commission (CSRC) and Hong Kong Securities and Futures Commission (SFC) (2018). Joint Announcement of the China Securities Regulatory Commission and the Hong Kong Securities and Futures Commission. Announcement dated 11 April 2018; quota increase effective 1 May 2018. Available at: https://www.csrc.gov.cn/csrc_en/c102030/c1370517/content.shtml.
- de Chaisemartin, C. and D’Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–2996.
- Ellsberg, D. (1961). Risk, ambiguity, and the Savage axioms. *Quarterly Journal of Economics*, 75(4):643–669.
- Epstein, L. G. and Schneider, M. (2008). Ambiguity, information quality, and asset pricing. *Journal of Finance*, 63(1):197–228.
- Fama, E. F. and French, K. R. (1992). The cross-section of expected stock returns. *Journal of Finance*, 47(2):427–465.
- Fama, E. F. and French, K. R. (1993). Common risk factors in the returns on stocks and bonds. *Journal of Financial Economics*, 33(1):3–56.
- Gilboa, I. and Schmeidler, D. (1989). Maxmin expected utility with non-unique prior. *Journal of Mathematical Economics*, 18(2):141–153.
- Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.
- Han, B. and Kumar, A. (2013). Speculative retail trading and asset prices. *Journal of Financial and Quantitative Analysis*, 48(2):377–404.
- Hansen, P. R. and Lunde, A. (2006). Realized variance and market microstructure noise. *Journal of Business & Economic Statistics*, 24(2):127–161.
- Henry, P. B. (2000). Stock market liberalization, economic reform, and emerging market equity prices. *Journal of Finance*, 55(2):529–564.

- Hong Kong Exchanges and Clearing Limited (HKEX) (2025). Frequently Asked Questions: Shanghai and Shenzhen Stock Connect. PDF. See especially Part 1 (Trading), Question 1.4 and follow-ons (eligibility criteria, review schedule, and sell-only designations). Available at: https://www.hkex.com.hk/-/media/HKEX-Market/Mutual-Market/Stock-Connect/Getting-Started/Information-Booklet-and-FAQ/FAQ/FAQ_En.pdf.
- Hong Kong Exchanges and Clearing Limited (HKEX) (2024). Stock Connect (February 2024) Factsheet / Booklet. PDF; includes quota levels and high-level eligible A-share definitions. Available at: <https://www.hkex.com.hk/-/media/HKEX-Market/Mutual-Market/Stock-Connect/Getting-Started/Information-Booklet-and-FAQ/StockConnectFeb2024.pdf>.
- Hong Kong Exchanges and Clearing Limited (HKEX) (2026). View All Eligible Securities (Stock Connect Eligible Stocks). Webpage; publishes current eligible lists and list changes (including sell-only lists). Available at: https://www.hkex.com.hk/Mutual-Market/Stock-Connect/Eligible-Stocks/View-All-Eligible-Securities?sc_lang=en.
- Hong Kong Exchanges and Clearing Limited (HKEX) (2021). HKEX Welcomes Inclusion of STAR Market Stocks into Stock Connect and Further Expansion of Southbound Stock Connect Trading. Corporate news release, 22 Jan 2021; STAR inclusion effective 1 Feb 2021. Available at: https://www.hkex.com.hk/News/News-Release/2021/210122news?sc_lang=en.
- Hoang, B. T. and Mateus, C. (2024). How does liberalization affect emerging stock markets? Theories and empirical evidence. *Journal of Economic Surveys*, 38(3):877–898.
- International Monetary Fund (IMF) (2023). Geopolitics and Financial Fragmentation: Implications for Macro-Financial Stability. In *Global Financial Stability Report: Safeguarding Financial Stability amid High Inflation and Geopolitical Risks* (April 2023), Chapter 3.
- Izhakian, Y. (2020). A theoretical foundation of ambiguity measurement. *Journal of Economic Theory*, 187:105001.
- Jegadeesh, N. (1990). Evidence of predictable behavior of security returns. *Journal of Finance*, 45(3):881–898.
- Jegadeesh, N. and Titman, S. (1993). Returns to buying winners and selling losers: Implications for stock market efficiency. *Journal of Finance*, 48(1):65–91.
- Jurado, K., Ludvigson, S. C., and Ng, S. (2015). Measuring uncertainty. *American Economic Review*, 105(3):1177–1216.
- Kacperczyk, M., Sundaresan, S., and Wang, T. (2021). Do foreign institutional investors improve price efficiency? *Review of Financial Studies*, 34(3):1317–1367.
- Klibanoff, P., Marinacci, M., and Mukerji, S. (2005). A smooth model of decision making under ambiguity. *Econometrica*, 73(6):1849–1892.
- Knight, F. H. (1921). *Risk, Uncertainty and Profit*. Houghton Mifflin, Boston.

- Li, Q., Liu, X., Chen, J., and Wang, H. (2022). Does stock market liberalization reduce stock price synchronicity?—Evidence from the Shanghai–Hong Kong stock connect. *International Review of Economics & Finance*, 77:25–38.
- Ma, C., Rogers, J. H., and Zhou, S. (2021). The effect of the China Connect. *Working paper*.
- Rey, H. (2015). Dilemma not trilemma: The global financial cycle and monetary policy independence. *NBER Working Paper* No. 21162.
- Roth, J., Sant’Anna, P. H. C., Bilinski, A., and Poe, J. (2023). What’s trending in difference-in-differences? A synthesis of the recent econometrics literature. *Journal of Econometrics*, 235(2):2218–2244.
- Sha, Y., Zhang, P., Wang, Y., and Xu, Y. (2022). Capital market opening and green innovation—Evidence from Shanghai–Hong Kong stock connect and the Shenzhen–Hong Kong stock connect. *Energy Economics*, 111:106048.
- Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.
- Wang, S. (2021). How does stock market liberalization influence corporate innovation? Evidence from Stock Connect scheme in China. *Emerging Markets Review*, 47:100762.
- Xu, K., Zheng, X., Pan, D., Xing, L., and Zhang, X. (2020). Stock market openness and market quality: Evidence from the Shanghai–Hong Kong Stock Connect program. *Journal of Financial Research*, 43(2):373–406.
- Yuan, Y., Ling, A., and Hu, Z. (2024). Ambiguity, limited market participation, and the cross-sectional stock return. *Journal of Management Science and Engineering*, 9(4):522–534.
- Zhou, X., Roh, T.-Y., and Xu, Y. (2025). Lottery preference and skewness risk premium: Evidence from the Chinese market. *Journal of Futures Markets*, 45:1818–1851.