Regulation and the funding of new ventures*

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Abstract

We explore the role of regulation in promoting venture capital investment into young and innovative firms. Our testing ground is the largely unregulated cryptocurrency ecosystem. To analyse this issue, we construct a comprehensive measure of regulatory stringency at the state-month level for the United States. Regulation is positively correlated with VC funding in states with a more developed financial sector. Looking at granular deal-level data, we show that the increase in funding is consistent with a reduction in information asymmetries: younger firms with less tangible assets benefit more, and foreign investors, investors that are not specialised in the crypto sector, and those with fewer investment professionals invest more capital. Our findings underscore how regulation can encourage the development of new firms.

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1 Introduction

Venture capital (VC) plays a crucial role in financing young innovative firms, and helping them commercialise their products (Da Rin et al., 2013; Comin and Nanda, 2019). They bridge the gap between the earliest stages of start-ups, when there is more uncertainty around an idea and its potential returns, and more mature firms that can rely on bank loans or capital markets. Firms funded by VC achieve greater scale, are responsible for a greater share of employment and are less likely to fail, especially in the first years of their life (Puri and Zarutskie, 2012).[1] Many of the most high-growth firms in the U.S. were initially backed by VC (Lerner and Nanda, 2023).

For venture capitalists, however, the selection of and investment into new ventures or start-ups presents significant challenges. They face uncertainty about the feasibility of the investment, opacity about the processes that the firm follows, and a potential lack of clarity on how public authorities will react to the innovative firm's product (Hall and Lerner, 2010). Given VC funding's substantial positive economic effects, it is important to determine the consequences of regulation on these types of investment.[2]

The effects of regulation on VC investments are ambiguous ex ante. On the one hand, regulation could stifle innovation if it adds to the costs of doing business (Aghion et al., 2023), making new firms less likely to be financially viable and thus complicating younger firms access to capital. On the other hand, the development of a regulatory framework may further contribute to better outcomes at the earlier stages of the capital raising process, either because they reduce information asymmetries or simply because they

[1]For example, Gornall and Strebulaev (2021) focus on public firms funded before 1968, and find that venture capital-backed companies accounted for 40% of U.S. market capitalisation and more than 60% of R&D spending, and highlight evidence that venture capital was causally responsible for the rise of 50 of the largest public companies in 2020.

[2]See Llewellyn (1999) for a discussion of the economic rationale for financial regulation focused on solving market failures and Aquilina et al. (2024) for an application to crypto and decentralized finance.

decrease uncertainty for both investors and entrepreneurs.[3] Regulation can encourage investors looking to finance firms' innovation and risky ventures.[4] This study explores a crucial way in which public authorities can potentially facilitate the financing of new ventures: the introduction of regulation.

To assess the impact of regulatory frameworks on the financing of emerging firms by VC, we focus on the cryptocurrency industry as a testing ground.[5] The crypto industry represents an ideal setting to assess the effect of regulatory attitudes towards the industry on VC investments: first, crypto developed from scratch around 2009, and there were no obvious reasons why VC would fund crypto deals more in one U.S. state rather than the others (see Figure 2); second, there were –by definition–no pre-existing rules that may confound the effects of any new regulatory framework subsequently introduced. We hence avoid common pitfalls associated with the regulation of established sectors, where path dependence can significantly influence outcomes. Since public authorities had to build a regulatory framework for the industry as a whole, rather than focusing on specific aspects or processes, we are able to assess the effects of regulation holistically. Finally, there was little reason for crypto firms to be located in specific states, as the industry does not need to be close to producers or consumers of a particular type. Regulation may have been one of the most important factors for start-ups and firms to consider as different regulatory approaches developed.

We focus on the United States, a global hub for venture capital investment and an early adopter of crypto. We compile detailed information on state laws and regulations

[3]Fully-fledged regulation is not the only tool available for public intervention: public grants have proven to be an effective tool (Howell, 2017), as well as business accelerators and incubators (Gonzalez-Uribe and Leatherbee, 2018; Yu, 2020), regulatory sandboxes (Cornelli et al., 2024) and antitrust enforcement (Zhang, 2023).

[4]Regulation can also have positive effects for well established businesses. An example would be laws mandating the disclosure of information and facilitating private enforcement of disputes (La Porta et al., 2006).

[5]We use the crypto sector as a testing ground for the unique setting that it offers, but our results can be generalised to other sectors that rely on VC funding.

impacting the crypto sector across all U.S. states, sorting them into 15 different categories. Our scope is intentionally broad, as we aim to capture the overall level of stringency that each state has with respect to crypto activities. We looked for regulation on whether crypto platforms must comply with money transmission law and if they require a general/ specific license; if crypto-related earnings are taxable or tax exempt; whether state banks can act as crypto custodians; or whether contracts signed using the blockchain are enforceable by law. Using this information, we construct an index of regulatory stringency at the state-month level from January 2010 to December 2022. We call this index the Crypto Stringency Index or CryStIn for short.

To study the effect of regulatory stringency on VC funding we use data from PitchBook Data Inc, one of the leading sources for private markets deal-level information (Cornelli et al., 2024). It provides granular deal-level information, covering characteristics of the firm seeking funding, such as the sector where it operates, the age of the firm, or the level of education of the CEO; information on the deal itself, such as the date, the type and the amount raised in the deal; and information on the investors (VC and other private market actors), like the number of investment professionals or their location.

We first study the relationship between regulatory stringency, as measured by our index, and private market deal-making at the state level. We find that regulation is positively and significantly correlated with VC funding only in states with a more developed financial sector (which we call financial hubs): both capital invested and the number of deals are associated with tighter regulatory attitudes. In particular, a one standard deviation increase in the index is associated with an increase in capital raised in a financial hub by around 30%. States with a less developed financial sector do not display such a correlation.

To understand the causal mechanism behind these results, we zoom in on a change in one of the elements of the index in a state that is a financial hub. In particular, we focus on the introduction in 2015 of regulation 23 NYCRR Part 200, commonly known as the BitLicense, in the state of New York. The BitLicense is the most recent example of a sizeable change in regulatory stringency towards the crypto industry. This regulation requires that firms conducting crypto activities in New York obtain a specific license, thereby implying additional disclosure of information for the benefit of both start-up customers and VC investors. For this analysis, we use detailed firm-level data. To build a control group for the treated crypto firms, we match deals by firms in New York to deals by firms headquartered in other states using coarsened exact matching. Methodologically, we run difference-in-differences specifications at the firm-quarter-year level on a window of two years around the introduction of the Bitlicense.

We look for evidence that the BitLicense decreased information asymmetries, making it easier for VC firms to filter and select projects. Extensive literature indicates that information asymmetries are more pronounced for young and start-up firms (Morellec and Schürhoff, 2011; Conti et al., 2013), as entrepreneurs have better knowledge of the quality of their risky project compared to potential investors. Established companies, on the other hand, have a proven track record that can help potential investors assess their value. Firms with less tangible assets that could be pledged as collateral as, for example, software firms (Goyal and Wang, 2013; Aboody and Lev, 2000) are also harder to asses.

Our analysis of granular deal-making activity before and after the introduction of the BitLicense reveals a positive role of regulation in mitigating information asymmetries. From the perspective of firms, we observe a substantial increase in funding for young firms, start-ups, and firms in sectors characterized by limited pledgeable collateral. Looking at the same issue from the perspective of investors, we find that foreign investors, those with less experience in crypto firms, and the ones with fewer investment professionals invest more capital in crypto start-ups following the introduction of the BitLicense. These results are consistent with the channel of lower information asymmetries driving our results.

Our results are robust to placebo tests and alternative definitions of the control group. We run a falsification test using California rather than New York as treatment group, and we find no effect of the introduction of the BitLicense for firms headquartered in California. To confirm that the results are not driven by unobservable New York specific characteristics, we run the analysis within the state of New York using deals in New York for fintech firms not active in crypto (which are not impacted by the introduction of the BitLicense) as control group. Our findings strongly support the hypothesis that regulation has positive effects on the funding of new ventures, especially in states with a significant financial sector. This underscores the potential for collaboration between private and public actors to mitigate the negative effects of market inefficiencies.

Contributions and related literature. The first contribution of this article is the production of the index on regulatory stringency itself (CryStIn), which we are making publicly available to other researchers.

Our article also contributes to the literature showing that regulation can have a positive effect on VC investment. Some papers study the effect on individual firms of entering regulator-designed programs, such as regulatory sandboxes (Cornelli et al., 2024), or receiving a government grant (Howell, 2017). These studies find that firms that benefited directly from grants or access to a sandbox raised significantly more venture capital than comparable firms that did not benefit from these programs. Similar evidence holds for business accelerators or incubators (Gonzalez-Uribe and Leatherbee, 2018; Yu, 2020), which operate either as public-private initiatives or as industry-led programs.

Regulatory sandboxes, grants, and business accelerators all act as quality certifications that allow potential venture capitalists to better asses the quality and potential of a project.

Other strands of the literature show that regulation as a whole can encourage VC investment and consequently innovation. Kim et al. (2018) show that the passage of the European Orphan Drug Act, aimed at encouraging investment for the discovery of new treatments for rare diseases, was positively associated with VC investment. Useche (2014) and Hoenig and Henkel (2015) find that patent regulation encourages both innovation and VC investment. We contribute to this literature by showing that the effects hold at the state level, underscoring that regulatory attitudes as summarized by our index, can have a positive impact in VC investment into an industry. Our results underscore that policy makers have a wide range of options, which we summarize through CryStIn.

This paper has implications for the policy debate, especially as several jurisdictions are introducing new regulation for crypto assets, like the Markets in Crypto-Assets Regulation (MiCA) in the European Union, or for regulation targeting artificial intelligence. Our findings underscore how regulation can encourage the development of new firms. Regulators concerned about increasing red tape costs for existing firms should also consider the positive effects for younger firms that regulation can have in promoting VC investment.

The rest of the paper is structured as follows: Section 2 presents the data and the index construction; Section 3 develops our state-level analysis and the relative robustness test; Section 4 discusses the analysis of the BitLicense; Section 5 concludes.

2 Variable construction and descriptive statistics

2.1 Building a state index of crypto-regulation stringency

We create a comprehensive and detailed database of U.S. crypto-related state regulation, from January 2010 to December 2022. Our focus is intentionally broad as we aim to capture overall regulatory attitudes toward crypto in each state. Therefore, we look for laws passed in 15 wide-ranging topics, covering whether: the state's money transmission regulation applies to crypto-assets; there is a license required to trade and exchange money and it applies to transactions conducted with crypto-assets; the state additionally requires a specific license for conducting transactions with crypto-assets; such license requires a third-party audit of the systems; there is regulation covering crypto-ATMs; there is a sandbox program in place; income from crypto-related activities is explicitly taxable or is tax-exempt; sales of crypto-related assets are taxable or tax-exempt; anti-money laundering and know-your-customer legislation applies to crypto-related activities; state banks can act as custodians of crypto-assets; banks acting as custodians have specific liquidity provisions for those crypto-assets; the public sector accepts payments in crypto-currencies; and whether transactions in a blockchain are legally recognized in the state.

Our methodology follows closely the one developed by Babina et al. (2022) in the context of open banking. For each of our 15 items, we conduct Google searches for mentions of laws that relate to crypto applications and then refer to the original texts. We prioritize official government or policy documents, and when those are not available, we use documents by law firms, industry participants, and academia.[6] We retrieve from the passed bill the date when the law was approved and the date when it came

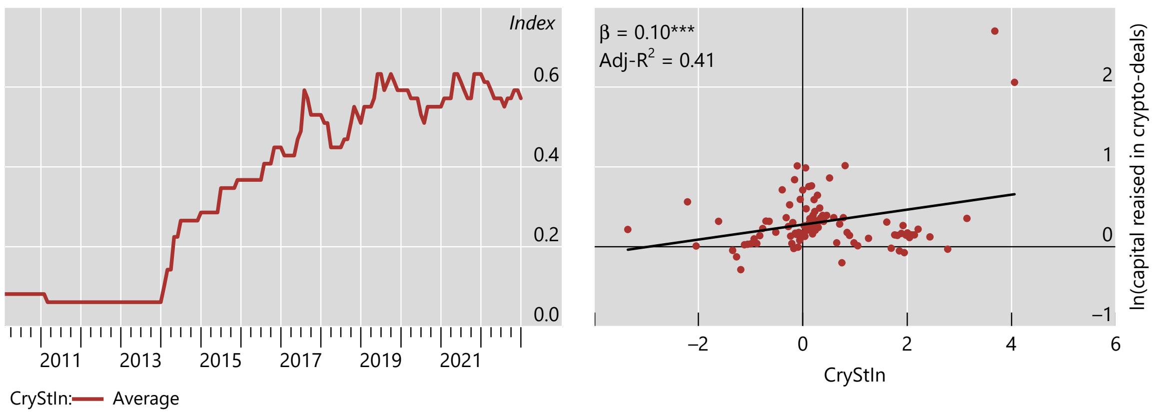
[6] Like, for example, the Stevens Center at The Wharton in the University of Pennsylvania.

into effect.[7] Each author conducted these searches independently and we then jointly reconciled any discrepancies.

The result is a monthly panel from January 2010 to December 2022 for each state, where each of our 15 items is a categorical variable that takes the value of one in the months where a law in such item was in force, thereby resulting in a more restrictive in the regulatory framework. Conversely, the variable takes the value -1 for those laws that are permissive rather than restrictive, like tax exemptions. We then sum across all 15 items to obtain the index for each month in each state. We denote this index the Crypto Stringency Index, or CryStIn in short.

The left-hand panel of Figure 1 shows CryStIn's evolution. On average, regulatory stringency across states increased over time. The right-hand panel of Figure 1 shows that regulatory stringency is positively associated with capital raised in crypto deals. The average state passes 1.6 laws, with some states passing as many as four. Each year, around 6.3 laws are passed related to crypto regulation.

Figure 1: **The Crypto Stringency Index (CryStIn)**



Note: The left-hand panel shows the cross-state simple average of the CryStIn. The right-hand panel shows a binned scatter plot of the variables reported along the axes. Based on monthly data from to 2010 to 2022. Includes state- and time fixed effects.
Source: PitchBook Data Inc; authors' calculations.

[7] We use the websites law.justia.com, legiscan.com, and casetext.com.

There could be two concerns regarding what our index captures: first, it could be that states that had higher levels of VC investment before 2010 (that is, before we observe the first states passing crypto specific regulation) were more likely to regulate the crypto sector to encourage VC investment. The correlation between the logarithm of overall VC investment between 2000 and 2009 and various measures of the index, such as the average index over 2010–2022, or the index by December 2022, is very low (around 0.16) and not statistically significant. This low correlation suggests that our index captures something other than pre-regulation VC funding. Another concern could be that our index is too broad and that, instead of capturing the stringency of crypto regulation, it captures general state attitudes or state-specific policies affecting economic activities. We therefore contrast CryStIn with the Fraser Index of economic freedom, which measures individuals' ability to act in the economic sphere free of restrictions (see Fraser Institute Economic Freedom). The pairwise correlation of our CryStIn with the overall state-level Fraser Index of economic freedom is low (ie less than 0.2 in absolute value). We find an even smaller correlation when comparing CryStIn to the Fraser Index sub-components. This suggests that our CryStIn is unlikely to be confounded by state-specific policies affecting freedom of conducting generic economic activity.

2.2 Data on private market deals

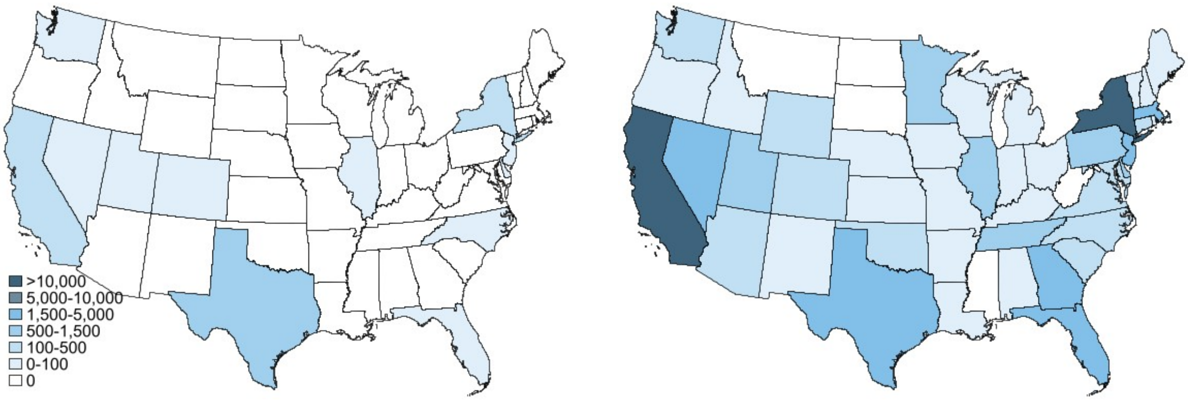
Data on private market deals come from PitchBook Data Inc. PitchBook is one of the leading sources for private markets deal-level data and it has been extensively used in research on VC (Cornelli et al., 2024; Ewens et al., 2022; Gompers et al., 2021; Gornall and Strebulaev, 2020; Nanda, 2020).[8] The sample for our analysis covers more than 3,600 transactions over the period January 2010 to December 2022 (see Figure 2). Of these,

[8] The deals and investment we focus on are made by professionals, denominated in U.S. dollars and with the objective to finance a project to develop a product. We completely abstract from initial coin offerings, which have often been promoted to unsophisticated retail investors and have been a fertile ground for scams (Morris, 2017; Phua et al., 2022).

more than 90% are VC transactions, with the remaining being private equity, private debt and mergers and acquisition deals. Over this period more than 2,000 crypto firms domiciled in the United States raised capital. For each deal, PitchBook collects granular information on the amount raised, the exact date of the deal, the type and purpose of the deal, information on education and gender of the CEO, the age of the firm, the business sector in which it operates, the business status, the firm's geographical location, and information on the investors.

Figure 2: **Investment in crypto firms increased remarkably from 2012 to 2022**

(a) Investment up to 2012 (b) Investment up to 2022



NOTE: The graph shows the cumulative capital invested in crypto deals since 2010, in millions of U.S. dollars, excluding Alaska, Hawaii, and Mississippi, by 2012 (left) and 2022 (right).
SOURCE: PitchBook Data Inc; U.S. Census Bureau; authors' calculations.

Leveraging this information we derive three indicator variables that we use to identify firms that are more affected by information asymmetries: $\mathbb{1}\left[\text{Young}_{i,t}\right]$ is an indicator variable that takes value 1 when firm i is less than 2 years old; $\mathbb{1}\left[\text{Start-up}_{i,t}\right]$, is an indicator variable that takes value 1 in the year a firm is founded; $\mathbb{1}\left[\text{Low-collateral}_{i}\right]$, is an indicator variable that takes value 1 if a firm is active in an industry whose asset tangibility, and consequently pledgeability as collateral, is traditionally limited. Following Aboody and Lev (2000) and Trester (1998) we consider firms for which the primary industry group is software as low-collateral. Furthermore, we derive

$\mathbb{1}\,[\text{Survival}_i]$ which is an indicator variable that takes value 0 if by October 2023 the firm had gone bankrupt, and value 1 if it was still in business. Finally, we derive three indicator variables that we use to identify investors that are more affected by information asymmetries: $\mathbb{1}\left[\text{Foreign investor}_j\right]$ is and indicator variable that takes value one if the investor is not headquartered in the United States; $\mathbb{1}\left[\text{Non-specialist investor}_j\right]$ is an indicator variable that takes value one if cryptocurrency is a sector that the firm had not traditionally targeted; $\mathbb{1}\,[\text{Small investment firm}_j]$ is and indicator variable that takes value one when the investor has less than five investment professionals.

2.3 Data on the finance and insurance sectoral GDP

The data on sectoral GDP for the finance and insurance sector comes from the Bureau of Economic Analysis. We use these data to identify financial hub states. Specifically, for each state we compute the total GDP for the finance and insurance sector for the period 2000–09 (right before crypto trading picked up), and we use this measure to determine whether a state belongs to the top-half/bottom-half, or the top-tercile/bottom-tercile of the finance GDP distribution.

Table 1 shows the descriptive statistics. Panel A provides summary statistics at the state-month-year level. There is an average of one deal per month in a state, and the average monthly capital raised is USD 4.17 millions, although there is a considerable range (some states do not raise capital in some months, while others raise more than USD 100 millions in a singe month). Panel B provides the summary statistics at the firm level at the time of the deal. The average firm in our sample is less than one year old when making a deal, its CEO is most-often male, and 73% of the firms in our sample remain operational by October 2023. Finally, Panel C shows summary statistics at the investor-firm level. The average investor is headquartered in the United States, does not specialise in the crypto industry and has more than five employees.

Table 1: Descriptive statistics

Panel A: state-level analysis

	No obs	Mean	St dev	Min	Max
Deals					
Capital raised, in USD mn	7,644	4.17	23.31	0	196.76
Number	7,644	0.73	3.31	0	74
CryStIn	7,644	0.35	1.21	−4	5

NOTE: The sample includes 49 states for the period 2010–22. *Capital raised* is winsorised at the 1st and 99th percentiles. *CryStIn* refers to the Cryptocurrecy Stringency Index.

Panel B: firm-level analysis

	No obs	Mean	St dev	Min	Max
Cumulative capital raised, in USD mn	2,584	4.18	18.15	0	262
Firm age	2,584	0.85	1.91	0	7
CEO male, (0/1)	2,584	0.97	0.16	...	...
CryStIn	2,584	0.59	1.34	0	4
Deal number	2,584	1.21	1.26	0	5
Young, (0/1)	2,584	0.62	0.48	...	...
Startup, (0/1)	2,584	0.18	0.39	...	...
Low-collateral, (0/1)	2,584	0.80	0.40	...	...
Survival, (0/1)	2,584	0.73	0.44	...	...

NOTE: The sample includes quarterly data for 152 firms around the approval of the New York DFS BitLicense ie Sep 2013 to Jun 2017. *Cumulative capital raised* is winsorised at the 2nd and 98th percentiles.

Panel C: investor-firm-level analysis

	No obs	Mean	St dev	Min	Max
Cumulative capital invested, in USD mn	21,968	0.56	1.98	0	48.37
Foreign investor, (0/1)	21,935	0.21	0.45	...	...
Non-specialist investor, (0/1)	21,935	0.68	0.47	...	...
Small investment firm, (0/1)	21,968	0.41	0.49	...	...

NOTE: The sample includes quarterly data for 942 investors and 142 firms around the approval of the New York DFS BitLicense ie Sep 2013 to Jun 2017. Foreign investor refers to investors headquartered outside of the U.S. Non-specialist investor refers to investors whose main sector is not the crypto sector, and Small investment firm refers to VC firms with less than five investment professionals.

3 Assessing the impact of regulation on private market deals

We begin our analysis with an assessment of whether, at the state level, a more stringent regulation of crypto applications is associated with more private market deals conducted in the state.

Regulation and the development of a state's financial system may complement one another. Financial system development could significantly impact how VC funding responds to regulatory changes.[9] The ex-ante development of financial markets facilitates the ex-post growth of firms that rely on external finance by reducing its costs (Fisman and Love, 2003). We would expect that, following a regulatory shift, VC firms will respond more in those states where the financial sector is more developed. Therefore, we divide our sample in two groups depending on their aggregate finance and insurance GDP before 2010.

We fit a state-month-year panel OLS specification with the following functional form:

$$\ln\left(y_{s,t}\right) = \beta \text{CryStIn}_{s,t} + \gamma \text{CryStIn}_{s,t} \times \mathbb{1}\left[\text{Fin Hub}_s\right] + \alpha_s + \theta_t + \varepsilon_{s,t}, \tag{1}$$

where the dependent variable $\ln(y_{s,t})$ is either the logarithm of capital raised or the logarithm of the number of deals in state s at month-year t, $\text{CryStIn}_{s,t}$ refers to the crypto regulatory stringency index we introduced in Section 2.1, $\mathbb{1}\left[\text{Fin Hub}_s\right]$ is an indicator variable that takes a value of one for states that have an aggregate sectoral GDP for the finance and insurance sector above the median, and α_s and θ_t correspond to state- and time fixed effects, respectively.

[9] For example, similarly to the way the overall level of financial development and real outcomes interact (Rajan and Zingales, 1998; Guiso et al., 2004; Kerr and Nanda, 2011).

The results, which are reported in Table 2 show that more stringent regulation of crypto is positively associated with both a larger amount of funds raised and a higher number of deals (Columns I and V). The pooled association is not statistically significant, which could be due to different states responding in different directions. We therefore compare financial hubs with other states using an interaction term (Columns II and VI) and running regressions separately for the two sub-samples (Columns III, IV, VII and VIII). We find that regulation is positively and significantly correlated with VC funding only in financial hubs, as reported by the non-statistically significant results for non financial hubs in columns IV and VIII. The magnitude of the coefficients is economically significant. For example, a one-standard deviation increase in CryStIn is associated to an increase in the capital raised of about 30% (column III) to 35% (column II) in financial hubs.

As a robustness test, we rule out that our findings are contingent on a specific definition of financial hubs. In Table B2 we consider a state a (non)financial hub if its aggregate sectoral GDP for the finance and insurance sector for the period 2000–2009 falls in the (bottom)top tercile of the distribution. Our findings are similar to the ones from Table 2 and robust to this stricter definition of financial hubs. We also explore in Appendix A a number of instruments for the index that account for possible confounding factors, and find that our results hold.

Table 2: Regulatory stringency and deal-making activity

	Dependent Variable							
	ln(capital raised)$_{s,t}$				ln(number of deals)$_{s,t}$			
Explanatory Variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
$\text{CryStIn}_{s,t}$	0.102	-0.100^{**}	0.198^{**}	-0.058	0.052	-0.072^{**}	0.101^{*}	-0.041
	(0.07)	(0.05)	(0.09)	(0.04)	(0.04)	(0.03)	(0.06)	(0.03)
$\mathbb{1}\,[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$		0.358^{***}				0.219^{***}		
		(0.10)				(0.06)		
Observations	7,644	7,644	3,900	3,744	7,644	7,644	3,900	3,744
Sample	Pooled	Pooled	Fin hub	Non fin hub	Pooled	Pooled	Fin hub	Non fin hub
Adjusted R^2	0.409	0.430	0.474	0.167	0.559	0.581	0.622	0.258

NOTE: Monthly data from 2010 to 2022. The sample in columns I–II and V–VI includes all states except for Alaska and Mississippi, for which there is no information on VC crypto activity. The sample in columns III and VII includes financial hub states only. The sample in columns IV and VIII includes non financial hub states only. Fin hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 above the median of the distribution. The entries denote the coefficients of a panel-OLS regression. Regressions include state- and time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

4 Economic channel: more regulatory stringency alleviates asymmetric information problems

In this section we investigate the channel through which more stringent regulation is associated to to more funding for firms, as found in Section 3. Specifically, we posit that a more stringent regulatory framework alleviates the asymmetric information problems that plague young and innovative firms, thus simplifying their access to private capital markets. Guided by the results in Section 3, we focus on a particular regulatory tightening event in a financial hub state: the introduction of the BitLicense in New York. To explore the economic channel, we rely on granular deal-level data.[10]

4.1 Institutional details on the BitLicense

On June 24, 2015 the New York Department of Financial Service (NYDFS) issued Virtual Currency Regulation 23 NYCRR Part 200 under the New York Financial Services Law to provide regulatory clarity to business active in the cryptocurrency space.[11] The regulation is also known under the name of *BitLicense*, as it introduces the requirement to obtain a specific business license to conduct cryptocurrency related activities in the state of New York.

The obligation to have a BitLicense, which imposes disclosure and capital requirements on firms operating in the crypto sphere, applies to those engaging in virtual currency business activities either involving New York residents or taking place in the state of New York (see 23 NYCRR 200.2(q)). The regulated activities include

[10] Another state that has passed substantial crypto regulation is Wyoming. This state, however, does not qualify as a financial hub. Furthermore, it is not clear that the regulatory push coincides with individuals and investors' sentiment (See The Economist Wyoming wants to become America's crypto capital. The data do not show any sustained increase in VC funding in Wyoming, consistent with our finding in Section 3.

[11] See Virtual Currency Businesses: Main Page - DFS.NY.gov for further details.

receiving virtual currency for transmission or transmitting it; storing, holding, or maintaining custody or control of virtual currency on behalf of others; buying and selling virtual currency as a customer business (not as an individual); performing exchange services as a customer business; or controlling, administering, or issuing a virtual currency. The requirements are comprehensive and require firms to disclose a substantial amount of information, such as detailed business plans, financial statements, and a description of each type of transaction or service to be conducted.[12] The NYDFS granted its first BitLicense on September 22, 2015.[13]

Our focus with this exercise is whether a more stringent regulatory environment in the state of New York, through the introduction of the BitLicense, facilitated VC funding of firms most affected by information asymmetries operating in the sector. We do not establish the direct effect of being awarded a BitLicense for a particular firm. It is unambiguous that the introduction of the BitLicense corresponds to a regulatory tightening. At the time, some firms already active in the sector opposed the introduction of the BitLicense, arguing that the regulatory burden that it introduced would limit their activities in the state of New York.[14] But as the BitLicense required greater transparency to engage in crypto-related activities, investment into traditionally more opaque firms–like start-ups and younger firms–could have increased.[15] In particular, our data set covers several start-ups that develop software for the virtual currency space, an application that does not require a BitLicense to operate per se, but that can be impacted by the introduction of the license, as the software is used for crypto-related activities.

[12] For details see NY Virtual Currency Business Activity License New Application Checklist.
[13] See NYDFS announces approval of the first BitLicense application form a virtual currency firm.
[14] See The Real Cost of Applying for a New York BitLicense.
[15] See for example this WSJ interview with the Coinbase CEO.

4.2 Empirical strategy

To disentangle the effect of a more stringent regulatory environment on information asymmetries, we run difference-in-differences specifications at the firm-quarter-year level on a window of two years around the introduction of the Bitlicense ie Q3 2013–Q3 2017.

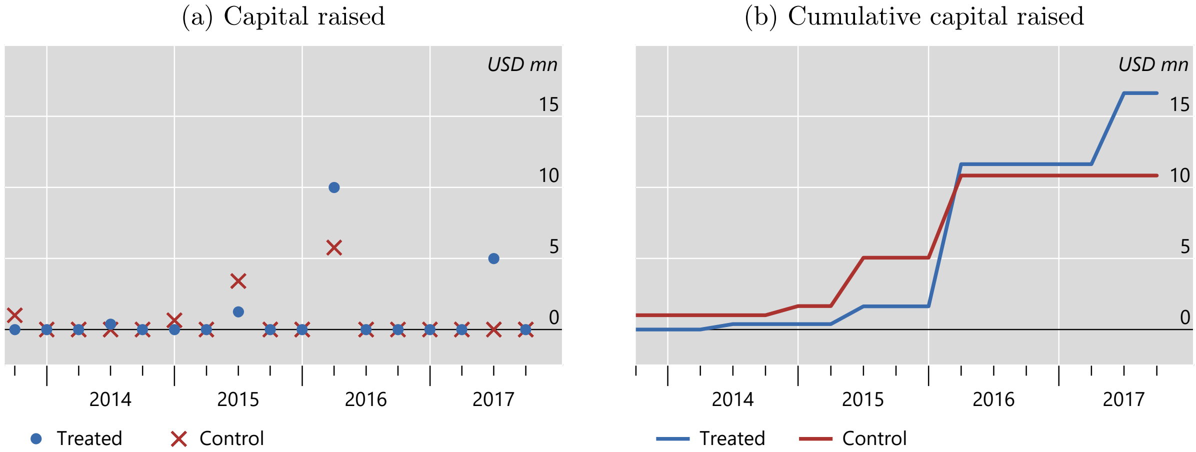
4.2.1 Building a control group

The treated firms are those headquartered in New York after the introduction of the BitLicense. To construct a control group as close as possible to the treatment group, we employ a coarsened exact matching (CEM) approach (Blackwell et al., 2009). This procedure selects into the control group firms in other states by matching firms that are statistically similar in terms of observable characteristics.[16] Specifically, we match based on firm characteristics: age, sector of operations, type of deal, CEO gender and level of education. To control for state differences in regulatory attitudes, we also include CryStIn in the covariates to match.

Figure 3 provides a visual representation of the fundraising activity of two matched firms, one in New York (treated, blue dots) and another one in a different state (control, red crosses). The capital raised by a firm through private market deals follows a jump process (panel (a)): between 2010 and 2018, firms raise money on average 2.3 times (with a standard deviation of 1.6), and the amounts vary in each of the capital rounds. Capital raised is zero in those periods when no deal is closed. Taking capital raised in a given deal, ie the deal size, as the dependent variable corresponds to estimating the effect on the average deal size. Since we are interested in estimating the effect of a more

[16] Coarsening of controls is done to maximize the balancedness in covariates and to guarantee that most treated observations have a match (Iacus et al., 2012).

Figure 3: **Fundraising activity by a representative firm**



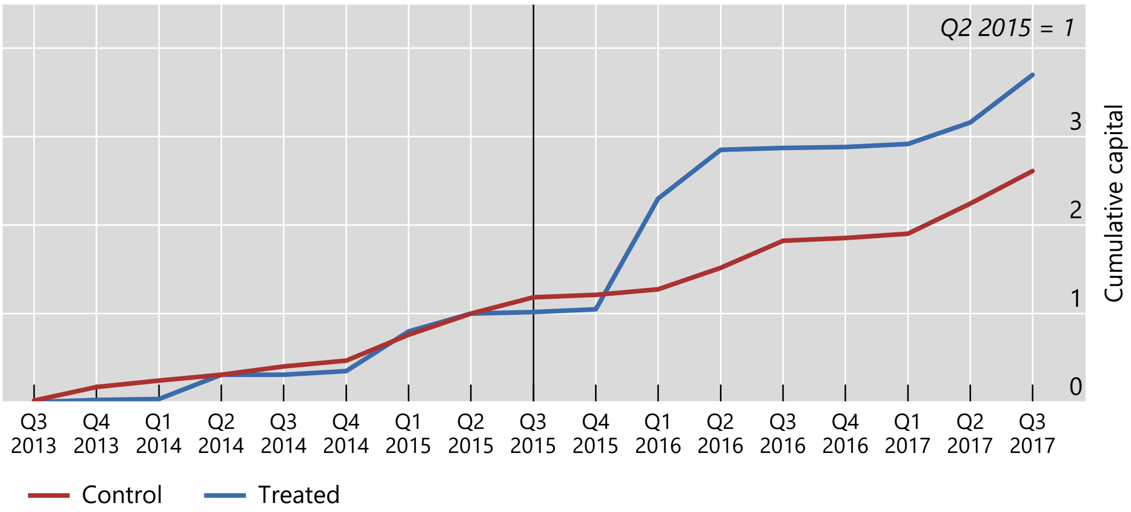
NOTE: The left-hand (right-hand) panel shows the (cumulative) capital raised by a representative treated and control firm.
SOURCE: PitchBook Data Inc; authors' calculations.

stringent regulatory environment on total capital raised, and not on the average deal size, we take cumulative capital as the dependent variable for our analysis (panel (b)).[17]

Figure 4 shows the average cumulative capital raised by treated and untreated firms, normalized to their value in the quarter right before treatment (ie the second quarter of 2015). Before the introduction of the BitLicense, firms in the treatment and the control group showed a similar evolution in their cumulative capital raised. However, two years after the tightening of the regulatory environment, firms based in New York raised, on average, 1.4 times the amount raised by the firms in the control group.

[17] See Beraja et al. (2023).

Figure 4: **Cumulative capital around the shock to regulatory stringency**



NOTE: The figure shows the simple average of the cumulative capital raised by treated- and control firms. The black vertical line indicates $t0$ –ie 2015 Q3 –the quarter when the NY DFS BitLicense was introduced.
SOURCE: PitchBook Data Inc; authors' calculations.

4.2.2 Regression specification

To account for the count like nature of the variable, we estimate Poisson Pseudo Maximum Likelihood regressions (Chen and Roth, 2024; Mullahy and Norton, 2022). Specifically, we fit the following specification at the firm i quarter-year t level:

$$y_{i,t} = \exp\left(\beta \mathbb{1}\left[\text{Post}_t\right] \times \mathbb{1}\left[\text{NY}_i\right] \times \mathbb{1}\left[\text{IA}\right] + \ X'_{i,t}\gamma + \alpha_i + \theta_t + \varepsilon_{i,t}\right) \tag{2}$$

The dependent variable $y_{i,t}$ is the cumulative capital raised by firm i from the beginning of our observation window up to quarter-year t. The indicator variable $\mathbb{1}\left[\text{Post}_t\right]$ equals one after the introduction of the BitLicense. The dummy variable $\mathbb{1}\left[\text{NY}_s\right]$ varies at the state level and takes the value of one for firms headquartered in New York and zero otherwise.

The indicator variable $\mathbb{1}[\text{IA}]$ varies at the firm- or firm-time level and identifies firms for which information asymmetries are stronger. Depending on the specification, it signals whether the firm is less than two years old ($\mathbb{1}\left[\text{Young}_{i,t}\right]$), newly created in the year ($\mathbb{1}\left[\text{Start-up}_{i,t}\right]$), or has limited asset pledgeability ($\mathbb{1}\left[\text{Low collateral}_{i}\right]$).[18] The coefficient of interest β corresponds to the estimated change in cumulative capital raised after the introduction of the BitLicense for firms that are more affected by information asymmetries versus others. $X_{i,t}$ is a vector of the controls included in the matching. α_i and θ_t correspond to firm- and time fixed effects. Firm fixed effects control for firm-specific unobserved characteristics that our dataset might not include, like CEO productivity or market strategies. Time (quarter-year) fixed effects control for time-specific trends that are common to all firms, like overall trends in crypto-VC funding or the price of crypto currencies. We cluster standard errors at the state level.

4.3 Results

Table 3 reports the regressions results.[19] The positive and statistically significant coefficient from column I is consistent with the effect found for financial hubs in Section 3; a tighter regulatory environment has a positive effect on capital raised by treated firms. In dollar terms, the effect corresponds to an increase in total capital raised of more than USD 1.1 millions, on average. The effect is somewhat larger, but comparable to the USD 700,000 found by Cornelli et al. (2024) in the context of the UK FCA regulatory sandbox.

[18] Extensive literature indicates that information asymmetries are more pronounced for young firms (Morellec and Schürhoff, 2011), start-ups (Conti et al., 2013), and firms with less tangible assets that could be pledged as collateral as, for example, software firms (Chung et al., 2010; Goyal and Wang, 2013).

[19] Throughout the paper, we winsorize capital raised at the 2% level. We do so to avoid that a single, outlier deal drives our results. One concern, however, could be that the bigger VC firms only engage in such big deals and hence our results would not be representative of their behaviour. In unreported regressions, we run Equation 2 without winsorizing the dependent variable and find that the results remain consistent.

Table 3: Regulatory stringency and information asymmetries

	Dependent Variable: Cumulative capital raised$_{i,t}$							
Explanatory Variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i]$	0.228***	0.582	0.180**	0.544	0.210***	0.901	−0.250	0.203
	(0.07)	(0.64)	(0.07)	(0.58)	(0.05)	(0.58)	(0.29)	(0.73)
$\mathbb{1}[\text{Young}_{i,t}]$			−0.537	−0.276				
			(0.35)	(0.35)				
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Young}_{i,t}]$			0.296	0.231				
			(0.25)	(0.26)				
$\mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Young}_{i,t}]$			−1.229***	−1.383***				
			(0.32)	(0.35)				
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Young}_{i,t}]$			0.542**	0.881***				
			(0.27)	(0.30)				
$\mathbb{1}[\text{Start-up}_{i,t}]$					−0.669**	−0.436		
					(0.30)	(0.32)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Start-up}_{i,t}]$					−0.063	−0.174		
					(0.40)	(0.38)		
$\mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Start-up}_{i,t}]$					−0.989***	−1.299***		
					(0.35)	(0.33)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Start-up}_{i,t}]$					1.740***	2.109***		
					(0.41)	(0.38)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Low-collateral}_i]$							−0.315	−0.375
							(0.34)	(0.44)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Low-collateral}_i]$							0.996***	1.249**
							(0.34)	(0.49)
Controls		✓		✓		✓		✓
Observations	2,584	2,584	2,584	2,584	2,584	2,584	2,584	2,584
Pseudo R^2	0.881	0.897	0.885	0.899	0.883	0.898	0.882	0.898

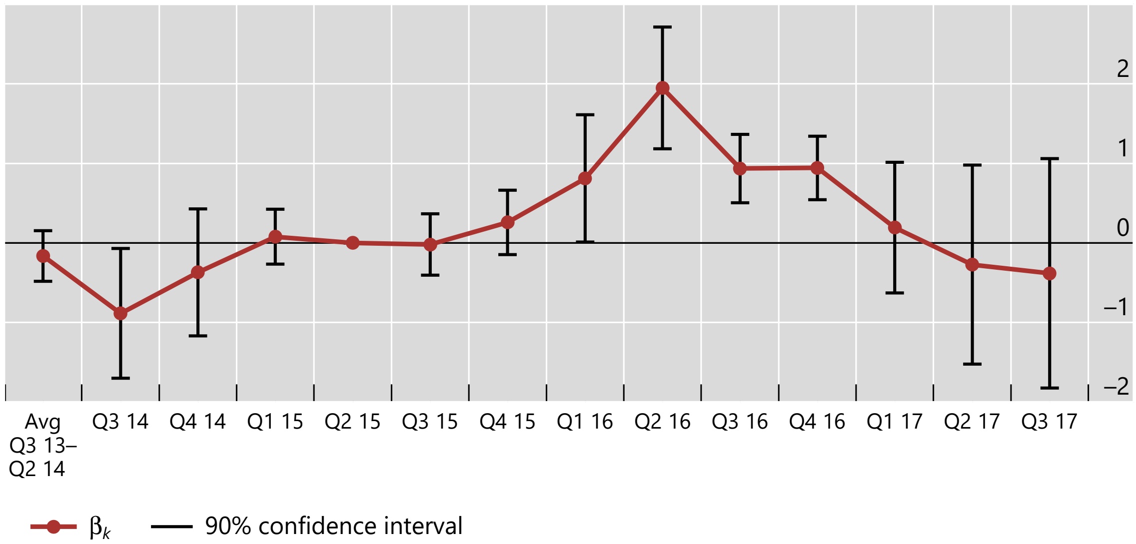
NOTE: Firm-level data for the 8 quarters before to the 8 quarters after the introduction of the New York DFS BitLicense ie Sep 2013 to Jun 2017. The table reports the coefficients of Poisson pseudo-maximum-likelihood regressions. The dependent variable *Cumulative capital raised*$_{i,t}$ is the cumulative capital raised by firm i up to period t. $Young_{i,t}$ is and indicator variable that takes value one when firm age is less than 2 years old. $Start-up_{i,t}$ is and indicator variable that takes value one in the year the firms is founded. $Low-collateral_i$ is and indicator variable that takes value one when primary business group is Software (Aboody and Lev, 2000; Trester, 1998). Regressions include firm- and time fixed effects. Controls are firm age, CEO- gender and education level, deal type, firm- status and number of deals, and $\text{CryStIn}_{s,t-1}$. Regressions are weighted by CEM weights. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Column III introduces the triple interaction term for young firms. Capital raised by young firms in the state of New York is significantly higher (almost three-quarters more) compared to older, more established firms. Our results remain robust after controlling for firm- and deal level characteristics in column IV.

Our results rely on the assumption that the treated and untreated firms followed a similar trend before the introduction of the BitLicense. We therefore estimate how cumulative capital raised by young firms in the state of New York changes with respect to older firms in each quarter. Figure 5 reports the results of this analysis, where each dot corresponds to the triple interaction term of an expanded variant of equation Equation 2, in which we replace the indicator variable $\mathbb{1}\left[\text{Post}_t\right]$ with an indicator variable for each quarter-year. The quarter before the BitLicense was introduced (ie Q2 2015) is the omitted category. Figure 5 shows that there is no discernible difference in the cumulative capital raised by young and old firms in the periods before the introduction of the BitLicense in New York. However, capital raised by younger firms increases significantly more compared to older firms after the BitLicense came into effect. The effect persistently lasts for five quarters and levels out from the sixth quarter onward.[20]

[20]Pre-trends for start-up firms also hold, albeit with more noise given that the sample size is smaller.

Figure 5: **Coefficient plot: pre-trends**



NOTE: The figure shows coefficient estimates for the regression $y_{i,t} = \exp\left(\sum_{k=-8}^{K=8} \beta_k \mathbb{1}\left[\text{quarter}_t\right] \times \mathbb{1}\left[\text{NY}_i\right] \times \mathbb{1}\left[\text{IA}\right] + X'_{i,t}\gamma + \alpha_i + \theta_t + \varepsilon_{i,t}\right)$, where the coefficient β_k corresponds to the estimated change in cumulative capital raised k quarters before or after the introduction of the BitLicense for firms that are more affected by information asymmetries versus the others. Regressions are weighted with the respective CEM weights. We average coefficients for -8 to -5 quarters before the treatment date as not all of these are identified due to data limitations.
SOURCE: PitchBook Data Inc; authors' calculations.

For further evidence that our proposed mechanism is driving the results, we zoom in on the youngest possible firms, new start-ups. Columns V and VI report the results for start-up firms and show that the effects are stronger than for the young firms. Start-up firms in the state of New York raise significantly more capital than established firms. The effect is economically sizeable and corresponds to 5.7x–8.2x more in total capital raised by treated start-up firms relative to the older firms based in the same state. In absolute terms, the overall effect for start-ups corresponds to an increase in total capital raised of about 22% (column VI), a magnitude that corresponds to around USD 925,000 based on the average cumulative capital raised.[21] Furthermore, these results on start-ups are consistent with the positive effect of a stricter regulatory framework on total

[21] Calculated as the sum of the coefficients $\mathbb{1}\left[\text{Start-up}_i\right] + \mathbb{1}\left[\text{Post}_i\right] \times \mathbb{1}\left[\text{Start-up}_i\right] + \mathbb{1}\left[\text{NY}_i\right] \times \mathbb{1}\left[\text{Start-up}_i\right] + \mathbb{1}\left[\text{Post}_i\right] \times \mathbb{1}\left[\text{NY}_i\right] \times \mathbb{1}\left[\text{Start-up}_i\right]$. The p-value of the corresponding F-test is 0.003 suggesting the statistical significance of the effect at the 1% level.

capital raised operating not only at the intensive margin, but at the extensive margin too. Our result is consistent with and somewhat larger than what Gonzalez-Uribe and Leatherbee (2018) find for the accelerator *Start-up Chile* (see their Table 6) and Howell (2017) finds in the context of the SBIR program (see Table 3).

Finally, columns VII and VIII focus on firms operating in the software industry, which is characterized by low collateral. The results show that, after the introduction of the BitLicense in New York, these firms raise significantly more capital. Overall, evidence from Table 3 provides empirical support for our proposed channel: a more stringent regulatory environment alleviates information asymmetries and facilitates access to capital for those firms that are more constrained.

One reasonable concern is that the majority of VC funding happens in the states of California and New York, as these are two VC hubs (Howell, 2020). Moreover, New York in particular has attracted several innovative fintech startups. Thus, VC funding of start-ups could increase in New York over time for factors different from regulation. We address these concerns with two robustness tests in Subsection 4.3.3. First, we run a falsification test considering deals in California, rather than in New York, as the treatment group (see Table 7). Second, we compare crypto firms to other fintech (non-crypto) firms within the state of New York (see Table 8). If the confounding factors were driving our results, we would expect a positive and statistically significant coefficient for the triple interaction term in the test using California as the treatment group and non statistically significant results in the test comparing firms within the state of New York. This is not what we find: overall, the results from these tests confirm our proposed explanation.

4.3.1 Ex-post survival

As an additional test, we check if by October 2023 the firms that raised money after the introduction of the BitLicense continued to operate or had gone bankrupt.[22] Column I in Table 4 shows that among young firms, firms that are operating by October 2023 raised 3 times more capital that the ones that end up going bankrupt, an economically sizeable effect. Column II estimates the regression on the subsample of firms that eventually went bankrupt. The results show that young firms belonging to this group raised less capital than old firms. Column III estimates the same regression on the subsample of firms that did not go bankrupt. Among those firms, following the introduction of the BitLicense, young firms raise more capital than old firms. Column IV shows that results are consistent if we consider all firms together. Overall, evidence from Table 4 provides empirical support to the role of regulatory stringency in alleviating asymmetric information and enabling more capital to flow to firms that (ex-post) survive.

4.3.2 Investors' characteristics and information asymmetries

Not all investors are equally affected by information asymmetries. A closer relationship or shorter geographical distance between investors and target firms helps alleviate such frictions (Grinblatt and Keloharju, 2001; Degryse and Ongena, 2005).[23] Thus, in our setting, information asymmetries should be stronger for investors that are based outside of the United States (ie foreign investors), since they have an information disadvantage when investing into U.S. firms; for investors that are not specialised in crypto; and small investment firms (ie investors with few investment professionals), since the cost of acquiring information for these firms is larger given the smaller headcount. To investigate

[22]The effect of bankruptcies for investors are more negative the more capital the bankrupted firm had raised (Altman, 1984).

[23]For further evidence see Coval and Moskowitz (1999), who document that investors tend to invest a larger share of their portfolio in stocks of firms that are geographically close and Ivković and Weisbenner (2005) who find that investors earn abnormal returns on stocks of firms that are physically close.

Table 4: Regulatory stringency and ex-post survival

	Dependent Variable: Cumulative capital raised$_{i,t}$			
Explanatory Variables	(I)	(II)	(III)	(IV)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i]$	-0.152	4.404	0.685	1.324**
	(0.67)	(3.43)	(0.57)	(0.60)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Survival}_i]$	-0.542			0.421**
	(0.46)			(0.20)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Survival}_i]$	1.162***			-0.692***
	(0.44)			(0.15)
$\mathbb{1}\left[\text{Young}_{i,t}\right]$		-0.546**	0.044	-1.551**
		(0.28)	(0.43)	(0.72)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}\left[\text{Young}_{i,t}\right]$		0.686***	0.066	1.189**
		(0.26)	(0.27)	(0.57)
$\mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Young}_{i,t}\right]$		0.566*	-2.178***	1.535**
		(0.34)	(0.41)	(0.72)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Young}_{i,t}\right]$		-0.333*	1.241***	-0.973*
		(0.19)	(0.31)	(0.56)
$\mathbb{1}\left[\text{Young}_{i,t}\right] \times \mathbb{1}[\text{Survival}_i]$				1.550
				(0.95)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}\left[\text{Young}_{i,t}\right] \times \mathbb{1}[\text{Survival}_i]$				-1.048
				(0.70)
$\mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Young}_{i,t}\right] \times \mathbb{1}[\text{Survival}_i]$				-3.662***
				(0.93)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Young}_{i,t}\right] \times \mathbb{1}[\text{Survival}_i]$				2.200***
				(0.76)
Sample of firms	Young	Eventually bankrupt	No bankruptcy	All
Observations	1,370	697	1,887	2,584
Pseudo R^2	0.792	0.792	0.914	0.901

NOTE: Firm-level data for the 8 quarters before to the 8 quarters around the introduction of the New York DFS BitLicense ie Sep 2013 to Sep 2017. The table reports the coefficients of Poisson pseudo-maximum-likelihood regressions. The dependent variable *Cumulative capital raised*$_{i,t}$ is the cumulative capital raised by firm i up to period t. $Young_{i,t}$ is and indicator variable that takes value one when firm age is less than 2 years. $Survival_i$ is and indicator variable that takes value 0 if by October 2023 firm i went bankrupt, 1 if it is still in business. Regressions include firm- and time fixed effects. Controls are firm age, CEO- gender and education level, deal type, firm- status and number of deals, and $CryStIn_{s,t-1}$. Regressions are weighted by CEM weights. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

if the investor angle confirms our finding that a tighter regulatory framework leads to a reduction in information asymmetries, in what follows, we perform analyses at the investor-firm level. Specifically, we estimate the following equation:

$$y_{j,i,t} = \exp\left(\beta \mathbb{1}\left[\text{Post}_t\right] \times \mathbb{1}\left[\text{NY}_i\right] \times \mathbb{1}\left[\text{Investor}_j\right] + \alpha_{j,i} + \theta_{k,t} + \varepsilon_{j,i,t}\right) \tag{3}$$

The dependent variable $y_{j,i,t}$ is the cumulative capital invested by investor j, in firm i from the beginning of our observation window up to quarter-year t. The indicator variable $\mathbb{1}\left[\text{Investor}_j\right]$ corresponds to investor-level characteristics proxying for the degree of information asymmetry to which they are exposed (ie foreign, non-specialised or small). We include investor × firm fixed effects (ie $\alpha_{j,i}$) to account for unobservable heterogeneity within each firm-investor combination (Jiménez et al., 2014), and industry × time fixed effects (ie $\theta_{k,t}$) to account for unobservable time-varying characteristics at the industry level, like aggregate demand factors.

Results from Table 5 confirm our findings from the firm-level analysis. The positive and statistically significant coefficient in column I suggests that a tighter regulatory framework leads to more capital invested. The triple interaction term (ie $\mathbb{1}\left[\text{Post}_t\right] \times \mathbb{1}\left[\text{NY}_i\right] \times \mathbb{1}\left[\text{Foreign investor}_j\right]$) in column II shows that after the introduction of the BitLicense foreign investors invested nearly twice more capital in New York based firms. Consistently, results from column III suggest that investors that don't have the crypto sector as a typical investment target, increase their investment of about 40%. Similarly, the positive and statistically significant coefficient $\mathbb{1}\left[\text{Post}_t\right] \times \mathbb{1}\left[\text{NY}_i\right] \times \mathbb{1}\left[\text{Small investment firm}_j\right]$ in column IV confirms our finding for small investment firms. Overall, our findings show that investors that are typically more affected by information asymmetry like foreign, non-specialist investors, and small investment firms comparatively invest more capital under a tighter regulatory framework.

Table 5: Investors' characteristics and informational asymmetries

	Dependent Variable: Cumulative capital invested$_{j,i,t}$			
Explanatory variables	(I)	(II)	(III)	(IV)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i]$	0.515***	0.383***	0.288**	0.331*
	(0.14)	(0.14)	(0.13)	(0.17)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Foreign investor}_j]$		0.462*		
		(0.27)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Foreign investor}_j]$		0.550**		
		(0.27)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Non-specialist investor}_j]$			−0.078	
			(0.13)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Non-specialist investor}_j]$			0.327**	
			(0.13)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Small investment firm}_j]$				−0.328***
				(0.09)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Small investment firm}_j]$				0.543***
				(0.13)
Observations	21,968	21,935	21,935	21,968
Pseudo R^2	0.646	0.648	0.646	0.647

NOTE: Investor-firm level data for the 8 quarters before to the 8 quarters after the introduction of the New York DFS BitLicense ie Sep 2013 to Jun 2017. The table reports the coefficients of Poisson pseudo-maximum-likelihood regressions. The dependent variable *Cumulative capital invested*$_{j,i,t}$ is the cumulative invested raised by each investor j in firm i up to quarter t, based on a pro-rata split of the overall deal amount. *Foreign investor*$_j$ is and indicator variable that takes value one if the investor is not headquartered in the United States. *Non-specialist investor*$_j$ is an indicator variable that takes value one if cryptocurrency is not a sector that the investor typically targets. *Small investment firm*$_j$ is and indicator variable that takes value one when the investor has less than five investment professionals. Regressions include investor × firm- and industry × time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

4.3.3 Robustness tests

In this section we discuss a number of robustness checks. Certain industries within the crypto sector might have become more attractive to investors over time. To control for any such difference we re-estimate Equation 2 replacing time- fixed effects with industry × time fixed effects, which would capture any such difference. Results from Table 6 are very similar to the ones presented in Table A1, thus confirming that they are not driven by unobservable time-varying industry characteristic.

Table 6: Controlling for time-varying industry characteristics

	Dependent Variable: Cumulative capital raised$_{i,t}$			
Explanatory Variables	(I)	(II)	(III)	(IV)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i]$	0.469	0.357	0.879**	−0.432
	(0.52)	(0.47)	(0.44)	(0.73)
$\mathbb{1}\left[\text{Young}_{i,t}\right]$		−0.295		
		(0.33)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}\left[\text{Young}_{i,t}\right]$		0.255		
		(0.27)		
$\mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Young}_{i,t}\right]$		−1.617***		
		(0.47)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Young}_{i,t}\right]$		1.065**		
		(0.46)		
$\mathbb{1}\left[\text{Start-up}_{i,t}\right]$			−0.440	
			(0.28)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}\left[\text{Start-up}_{i,t}\right]$			−0.144	
			(0.37)	
$\mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Start-up}_{i,t}\right]$			−1.341***	
			(0.40)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Start-up}_{i,t}\right]$			2.110***	
			(0.57)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Low-collateral}_i]$				0.387
				(0.25)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Low-collateral}_i]$				1.466**
				(0.67)
Observations	2,455	2,455	2,455	2,455
Pseudo R^2	0.897	0.899	0.898	0.899

NOTE: Firm-level data for the 8 quarters before to the 8 quarters around the introduction of the New York DFS BitLicense ie Sep 2013 to Sep 2017. The table reports the coefficients of Poisson pseudo-maximum-likelihood regressions. The dependent variable *Cumulative capital raised*$_{i,t}$ is the cumulative capital raised by firm i up to period t. $Young_{i,t}$ is and indicator variable that takes value one when firm age is less than 2 years. $Low-collateral_i$ is and indicator variable that takes value one when primary business group is Software (Aboody and Lev, 2000; Trester, 1998). Regressions include firm- and industry $\times$ time fixed effects to control for time-varying unobservable characteristics at the industry level. Controls are firm age, CEO-gender and education level, deal type, firm- status and number of deals, and $CryStIn_{s,t-1}$. Regressions are weighted by CEM weights. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

We also run a falsification test where the fictitious treatment group corresponds to firms based in California, instead of firms based in New York.[24] Notably, none of the coefficients of interest in Table 7 –ie the triple-interaction terms –are statistically significant.

[24] We select California as another VC Hub following Howell (2020). Results for Massachusetts, a smaller VC hub considered in Howell (2020), are similar to the California falsification test.

Table 7: Falsification test using California

	Dependent Variable: Cumulative capital raised$_{i,t}$					
Explanatory Variables	(I)	(II)	(III)	(IV)	(V)	(VI)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{CA}_i]$	0.208	0.340**	0.227	0.130	−0.102	0.453**
	(0.17)	(0.16)	(0.15)	(0.44)	(0.14)	(0.20)
$\mathbb{1}[\text{Young}_{i,t}]$		−0.695*			−0.570**	−0.585
		(0.38)			(0.29)	(0.48)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Young}_{i,t}]$		0.594***			0.687***	0.516**
		(0.22)			(0.25)	(0.21)
$\mathbb{1}[\text{CA}_i] \times \mathbb{1}[\text{Young}_{i,t}]$		−0.223			0.542*	−0.457
		(0.44)			(0.32)	(0.54)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{CA}_i] \times \mathbb{1}[\text{Young}_{i,t}]$		0.162			−0.232	0.358
		(0.23)			(0.18)	(0.22)
$\mathbb{1}[\text{Start-up}_{i,t}]$			−0.604**			
			(0.27)			
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Start-up}_{i,t}]$			0.619			
			(0.40)			
$\mathbb{1}[\text{CA}_i] \times \mathbb{1}[\text{Start-up}_{i,t}]$			−0.523			
			(0.34)			
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{CA}_i] \times \mathbb{1}[\text{Start-up}_{i,t}]$			−0.645			
			(0.39)			
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Low-collateral}_i]$				0.137		
				(0.47)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{CA}_i] \times \mathbb{1}[\text{Low-collateral}_i]$				0.087		
				(0.47)		
Sample of firms	All	All	All	All	Eventually bankrupt	No bankruptcy
Observations	2,839	2,839	2,839	2,839	714	2,125
Pseudo R^2	0.894	0.896	0.895	0.894	0.763	0.909

NOTE: Firm-level data for the 8 quarters before to the 8 quarters after the introduction of the New York DFS BitLicense ie Sep 2013 to Jun 2017. The table reports the coefficients of Poisson pseudo-maximum-likelihood regressions for a falsification test where the treated companies are located in California (instead of New York). The dependent variable *Cumulative capital raised*$_{i,t}$ is the cumulative capital raised by firm i up to period t. $Young_{i,t}$ is and indicator variable that takes value one when firm age is less than 2 years old. $Start-up_{i,t}$ is and indicator variable that takes value one in the year the firms is founded. $Low-collateral_i$ is and indicator variable that takes value one when primary business group is Software (Aboody and Lev, 2000; Trester, 1998). Regressions include firm- and time fixed effects. Controls are firm age, CEO- gender and education level, deal type, firm- status and number of deals, and $\text{CryStIn}_{s,t-1}$. Regressions are weighted by CEM weights. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

We also re-estimate Equation 2 using a different definition for the control group. Specifically, instead of considering crypto firms based in states other than New York, we use firms within the state of New York but active in the fintech sector (excluding the crypto industry). Fintech firms constitute a correct comparison, as argued in Babina et al. (2022), as some of the technology they employ is similar to that of crypto-firms, but are covered by very different regulation. Furthermore, and particularly relevant for our setting, fintech firms not active in the crypto industry are not subject to the BitLicense. The fintech comparison allows us to rule out that certain state-specific unobservable characteristics drive our results in the main analysis. By focusing on firms in New York, we ensure that both treatment and control firms are exposed to the same generic state shocks. Table 8 reports the results. Overall, the results are consistent with the evidence from Table 3 and Table 4: it is unlikely that our results are driven by unobservable characteristics at the state level.

Furthermore, as VC is a particularly information-sensitive industry (Gompers, 1995; Howell, 2020), we control that our findings persist when removing the few transactions belonging to other types of private deals (as described in Section 2). Results from Table 9 confirm our previous findings. Furthermore, coefficients from columns III and IV are somewhat larger in absolute value compared to the ones from Table 3 columns VI and VIII, respectively. Overall, our findings are consistent with the notion that a more stringent regulatory environment eases information asymmetries between firms and investors.

Table 8: Using New York fintech firms as control group

	Dependent Variable: Cumulative capital raised$_{i,t}$						
Explanatory Variables	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Treated}_i]$	0.424***	0.512***	0.438***	0.446***	−1.627***	0.037**	0.531***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.49)	(0.02)	(0.00)
$\mathbb{1}[\text{Young}_{i,t}]$		−1.397***				0.739***	−1.400***
		(0.00)				(0.05)	(0.00)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Young}_{i,t}]$		1.555***				0.942***	1.433***
		(0.00)				(0.05)	(0.01)
$\mathbb{1}[\text{Treated}_i] \times \mathbb{1}[\text{Young}_{i,t}]$		−1.218***				1.076***	−1.422***
		(0.05)				(0.28)	(0.00)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Treated}_i] \times \mathbb{1}[\text{Young}_{i,t}]$		0.088***				−2.072**	0.268***
		(0.00)				(0.88)	(0.00)
$\mathbb{1}[\text{Start-up}_{i,t}]$			−1.400***				
			(0.00)				
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Start-up}_{i,t}]$			1.929***				
			(0.01)				
$\mathbb{1}[\text{Treated}_i] \times \mathbb{1}[\text{Start-up}_{i,t}]$			−0.661***				
			(0.01)				
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Treated}_i] \times \mathbb{1}[\text{Start-up}_{i,t}]$			0.889***				
			(0.01)				
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Low-collateral}_i]$				1.171***			
				(0.00)			
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Treated}_i] \times \mathbb{1}[\text{Low-collateral}_i]$				0.709***			
				(0.00)			
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Survival}_i]$					−0.125***		
					(0.01)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Treated}_i] \times \mathbb{1}[\text{Survival}_i]$					5.883***		
					(0.47)		
Sample of firms	All	All	All	All	Young	Eventually bankrupt	No bankruptcy
Observations	2,462	2,462	2,462	2,462	1,368	320	2,085
Pseudo R^2	0.894	0.905	0.899	0.901	0.841	0.870	0.909

NOTE: Firm-level data for the 8 quarters before to the 8 quarters after the introduction of the New York DFS BitLicense ie Sep 2013 to Jun 2017. The table reports the coefficients of Poisson pseudo-maximum-likelihood regressions. The treatment group corresponds to firms based in the state of New York and active in the crypto space. The contrl group corresponds to firms based in the state of New York and active in the fintech space (ie excluding crypto). The dependent variable *Cumulative capital raised*$_{i,t}$ is the cumulative capital raised by firm i up to period t. $Young_{i,t}$ is and indicator variable that takes value one when firm age is less than 2 years old. $Start-up_{i,t}$ is and indicator variable that takes value one in the year the firms is founded. $Low-collateral_i$ is and indicator variable that takes value one when primary business group is Software (Aboody and Lev, 2000; Trester, 1998). Regressions include firm- and time fixed effects. Controls are firm age, CEO- gender and education level, deal type, firm- status and number of deals, and CryStIn$_{s,t-1}$. Regressions are weighted by CEM weights. Standard errors in parentheses are clustered by city: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Table 9: Regulatory stringency and information asymmetries for VC deals

	Dependent Variable: Cumulative capital raised$_{i,t}$			
Explanatory Variables	(I)	(II)	(III)	(IV)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i]$	0.591	0.542	0.909	0.155
	(0.63)	(0.57)	(0.57)	(0.75)
$\mathbb{1}[\text{Young}_{i,t}]$		−0.291		
		(0.36)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Young}_{i,t}]$		0.240		
		(0.27)		
$\mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Young}_{i,t}]$		−1.369***		
		(0.35)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Young}_{i,t}]$		0.872***		
		(0.31)		
$\mathbb{1}[\text{Start-up}_{i,t}]$			−0.429	
			(0.32)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Start-up}_{i,t}]$			−0.182	
			(0.38)	
$\mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Start-up}_{i,t}]$			−1.303***	
			(0.33)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Start-up}_{i,t}]$			2.111***	
			(0.38)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Low-collateral}_i]$				−0.432
				(0.48)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Low-collateral}_i]$				1.305**
				(0.53)
Observations	2,571	2,571	2,571	2,571
Pseudo R^2	0.897	0.900	0.899	0.899

NOTE: Firm-level data for the 8 quarters before to the 8 quarters around the introduction of the New York DFS BitLicense ie Sep 2013 to Sep 2017. The sample includes only firms financed by venture capital. The table reports the coefficients of Poisson pseudo-maximum-likelihood regressions. The dependent variable *Cumulative capital raised*$_{i,t}$ is the cumulative capital raised by firm i up to period t. $Young_{i,t}$ is and indicator variable that takes value one when firm age is less than 2 years. $Low - collateral_i$ is and indicator variable that takes value one when primary business group is Software (Aboody and Lev, 2000; Trester, 1998). Regressions include firm- and time fixed effects. Controls are firm age, CEO- gender and education level, deal type, firm- status and number of deals, and $CryStIn_{s,t-1}$. Regressions are weighted by CEM weights. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Finally, we corroborate the evidence from the investor-firm analysis using different definitions of the dependent variable. In Table 10, the dependent variable is computed by splitting the overall deal amount among all the investors participating to the deal proportionally to the number of investment professionals of each investor. In Table 11 the dependent variable is an indicator variable that takes value one if a given investor

j invests in firm i in quarter t and zero elsewhere. Overall, the evidence from Table 10 and Table 11 confirm the results from Table 5 suggesting that tighter regulation leads to lower information asymmetries and consequently more fund raising.

Table 10: Investors' characteristics and informational asymmetries: cumulative capital

	Dependent Variable: Cumulative capital invested$_{j,i,t}$			
Explanatory variables	(I)	(II)	(III)	(IV)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i]$	0.425***	0.252	0.156	0.308**
	(0.15)	(0.16)	(0.15)	(0.15)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Foreign investor}_j]$		0.282		
		(0.30)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Foreign investor}_j]$		0.576*		
		(0.33)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Non-specialist investor}_j]$			0.104	
			(0.12)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Non-specialist investor}_j]$			0.404***	
			(0.13)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Small investment firm}_j]$				−0.393***
				(0.15)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Small investment firm}_j]$				1.095***
				(0.21)
Observations	16,499	16,499	16,499	16,499
Pseudo R^2	0.767	0.768	0.767	0.768

NOTE: Investor-firm level data for the 8 quarters before to the 8 quarters after the introduction of the New York DFS BitLicense ie Sep 2013 to Jun 2017. The table reports the coefficients of Poisson pseudo-maximum-likelihood regressions. The dependent variable *Cumulative capital invested*$_{j,i,t}$ is the cumulative capital invested by each investor j in firm i up to quarter t, based on a split of the overall deal amount with weights proportional to the number investment professionals of each investor. *Foreign investor*$_j$ is an indicator variable that takes value one if the investor is not headquartered in the United States. *Non-specialist investor*$_j$ is and indicator variable that takes value one if cryptocurrency is not a sector that the investor typically targets. *Small investment firm*$_j$ is and indicator variable that takes value one when the investor has less than five investment professionals. Regressions include investor × firm- and industry × time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Table 11: Investors' characteristics and informational asymmetries: any capital raised

	Dependent Variable: Dummy capital raised$_{j,i,t}$			
Explanatory variables	(I)	(II)	(III)	(IV)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i]$	0.019^{***}	0.010	-0.013^{*}	0.008
	(0.01)	(0.01)	(0.01)	(0.01)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}\left[\text{Foreign investor}_j\right]$		0.029^{**}		
		(0.01)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Foreign investor}_j\right]$		0.036^{***}		
		(0.01)		
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}\left[\text{Non-specialist investor}_j\right]$			-0.031^{***}	
			(0.01)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}\left[\text{Non-specialist investor}_j\right]$			0.047^{***}	
			(0.01)	
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{Small investment firm}_j]$				-0.022^{***}
				(0.00)
$\mathbb{1}[\text{Post}_t] \times \mathbb{1}[\text{NY}_i] \times \mathbb{1}[\text{Small investment firm}_j]$				0.026^{***}
				(0.00)
Observations	22,627	22,576	22,576	22,627
R^2	0.045	0.047	0.046	0.046

NOTE: Investor-firm level data for the 8 quarters before to the 8 quarters after the introduction of the New York DFS BitLicense ie Sep 2013 to Jun 2017. The table reports the coefficients of panel OLS regressions. The dependent variable *Dummy capital raised*$_{j,i,t}$ is an indicator variable that takes value one if investor j invests in firm i in quarter t, and zero elsewhere. *Foreign investor*$_j$ is an indicator variable that takes value one if the investor is not headquartered in the United States. *Non-specialist investor*$_j$ is and indicator variable that takes value one if cryptocurrency is not a sector that the investor typically targets. *Small investment firm*$_j$ is and indicator variable that takes value one when the investor has less than five investment professionals. Regressions include investor × firm- and industry × time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

5 Conclusions

In this paper, we study the effects of the introduction of a new regulatory framework on the development of an innovative industry, using the cryptocurrency ecosystem as a testing ground. We make three main contributions to the literature.

First, we develop an index of regulatory stringency for the crypto industry in the United States at the state-month level, based on a comprehensive review of legislation and official publications by regulatory authorities. We make the index available for future research, thereby contributing to the understanding of how crypto is regulated in the United States.

Second, using the index, we document a positive association between the amount of capital raised and the level of regulatory stringency across states. The result is entirely driven by "financial hubs" ie those states where a large financial sector is present.

Finally, we provide evidence –using the introduction of the BitLicense in New York– that is consistent with the reduction of information asymmetries being the mechanism driving our results. We show that young firms, including start-ups, and firms in industries characterised by low collateral raised significantly more capital in New York after the introduction of the BitLicense than their counterparts. We also verify that investors facing higher information asymmetries before the BitLicense (foreign, not specialized in crypto, and smaller), allocate more money to these crypto ventures following the stricter regulatory framework.

Our results shed light on the nuanced relationship between regulation and the financing of novel, high-risk ventures. Importantly, our research underscores that regulation can act as a catalyst for venture financing and the development of a new industry, and this synergy is most pronounced in states with a more active financial sector. The key mechanism at play involves mitigating information asymmetries between investors and entrepreneurs. Policymakers should thus consider regulation and the development of young firms in the target ecosystem concurrently, recognizing the potential for complementarities when formulating policy.

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A Instrumenting crypto regulation

Our specification studying the association between crypto regulation and VC funding could suffer from endogeneity, biasing our coefficients upwards or downwards: on the one hand, state legislators may pass crypto related laws because they expect more VC investment into crypto-related ventures. On the other hand, they may pass laws aimed at curbing VC investment if they worry about VC encouraging an uncontrolled development of the industry. Additionally, there might be factors that change at the state-time level that are not captured by our fixed effects.

We address this issue by leveraging the geographic variation in the index. More similar states may have closer attitudes towards crypto regulation. The literature normally recognizes as more *similar* states those geographically closer (Acemoglu et al., 2019; Barth et al., 2013). In our case, as argued in Section 1, the crypto sector does not rely on geographical proximity to producers nor consumers to operate, which makes spatial correlation in regulatory requirements (changes in one state impacting its geographic neighbours) unlikely. Therefore we consider similar states based on the ranking of total VC funding over the period 2000–2009.[25]

Specifically, we instrument the index with the one period lag of the average of the index in similar states. The logic behind this instrument is that more similar states have a shared level of regulatory stringency that is independent of crypto VC funding in one particular state. Therefore, changes in peers' regulations only impact crypto VC funding in a state through the impact they have on the CrystIn of that specific state.

[25] A potential concern could be that the level of VC funding before the crypto sector took off is correlated with the level of regulatory stringency that states enact ex-post. However, the correlation between ex-ante VC funding and crypto regulatory stringency is very low.

Denote by $\mathcal{S}_{s,p}$ the set of states, excluding state s, that contains the $p \in \{10, 15\}$ closest states above and below state s in terms of the total VC capital raised over the period 2000–2009. Therefore, our instrument is $\overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}}$.

The first stage regressions are:

$$\begin{aligned} \text{CryStIn}_{s,t} =& \psi \overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}} + \eta \overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}} \times \mathbb{1}\left[\text{Fin Hub}_s\right] \\ &+ \omega_s + \tau_t + \upsilon_{s,t} \end{aligned} \tag{4}$$

$$\begin{aligned} \text{CryStIn}_{s,t} \times \mathbb{1}\left[\text{Fin Hub}_s\right] =& \zeta \overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}} + \lambda \overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}} \times \mathbb{1}\left[\text{Fin Hub}_s\right] \\ &+ \kappa_s + \iota_t + u_{s,t} \end{aligned} \tag{5}$$

where ω_s, κ_s are state fixed effects and τ_t, ι_t are month-year fixed effects.

Table A1: Regulatory stringency and deal-making activity: instrumental variable regressions

	Dependent variables			
	$\ln(\text{capital raised})_{s,t}$		$\ln(\text{number of deals})_{s,t}$	
Explanatory variables	(I)	(II)	(III)	(IV)
$\text{CryStIn}_{s,t}$	−0.785	−0.483	−0.354	−0.238
	(0.60)	(0.41)	(0.31)	(0.26)
$\mathbb{1}\left[\text{Fin Hub}_s\right] \times \text{CryStIn}_{s,t}$	1.983**	1.944**	1.215**	1.229**
	(0.78)	(0.85)	(0.50)	(0.55)
Observations	7,595	7,595	7,595	7,595
Number of closest states in the average	±10	±15	±10	±15
F-stat	3.42	2.81	3.19	2.74
Weak-IV Anderson-Rubin test, statistic	10.658	9.087	11.049	9.795
Weak-IV Anderson-Rubin test, p-value	0.005	0.011	0.004	0.007

NOTE: Monthly data from 2010 to 2022. The sample includes all states except for Alaska and Mississippi, for which there is no information on VC crypto activity. Fin Hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 above the median of the distribution. The entries denote the second-stage coefficients of a panel-IV regression where $\text{CryStIn}_{s,t}$ is instrumented with the one period lag of the out-of-state average of CryStIn over the states that rank $p \in \{10, 15\}$ positions above and below s in the ranking of total venture capital raised for the period 2000–2009. Regressions include state- and time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

The coefficients from Table A1 are positive and statistically significant for financial hub states and negative and non-statistically significant for non financial hub states.[26] The estimates for the IV coefficients are larger in magnitude than those of the OLS, suggesting that our OLS coefficients are biased downwards, potentially due to omitted variable bias arising from confounding factors varying at the state-time level.[27]

Overall, our findings are consistent with a stricter regulatory environment in a nascent sector being conducive to the funding of innovative firms, rather than constraining it, but only in states where the financial system is well developed. The results signal that there is a role for public intervention in the VC market to promote and sustain the growth of start-ups.

Robustness tests

We also verify that our instrument is robust to different compositions of the sample used to calculate the out-of-state average. Rather than relying just on states that are similar to each other, we instrument CryStIn with the lagged average of all the other states in our sample. The idea behind this instrument is that there is an underlying nationwide level of crypto regulatory stringency that is not correlated with state-level unobserved factors. Results from Table B4 confirm the robustness of our findings. The positive and statistically significant coefficients for the interaction term in column I and column IV confirm that a more stringent regulatory stance leads to more fundraising activity in financial hub states. Consistent with our previous findings, when splitting the sample in financial hubs and non-financial hubs, the effect persists for the former, while for the

[26]To address the concerns about the robustness of our inference potentially stemming from a weak instrument, we report the weak IV Anderson-Rubin test, which supports the robustness of our results. For further details see Andrews I, and Stock JH. 2018. Weak Instruments and What To Do About Them or Andrews et al. (2019). Table B1 report the results of the corresponding first stage regressions.

[27]For example, state-level legislation (such as environmental or remote working regulation) can be passed in batches. If several regulatory changes take place in a state, our index could capture some of that variation.

latter it is not statistically different from zero (columns II and III). The evidence on the number of deals is qualitatively similar but is somewhat weaker as indicated by the Anderson-Rubin test in column V.[28]

Finally, we use a different instrument for the regulatory stringency of the cryptocurrency ecosystem at the state level.[29] Specifically, we exploit the fact that the U.S. Department of Justice offers states funding opportunities to train officials and develop technical expertise, conduct research or collect national statistics, thereby improving the legal system of each state.[30] We instrument CryStIn with the one period lag of the total amount of grants awarded by the U.S. Department of Justice, Office of Justice Programs (DOJ-OJP) to each state. We posit that DOJ funding contributes to the development of higher quality regulation, which is likely correlated with better regulation of the crypto sector, but uncorrelated with the amount of capital raised in each state. The exclusion restriction relies on capital raised by crypto firms being only influenced by the state regulatory quality through the state-specific cryptocurrency regulation. Under this identifying assumption, the coefficients can be interpreted causally.

The results are consistent with our baseline in Table A1. Specifically, coefficients from Table B6 column I support our finding that a more stringent regulation leads to more capital raised in financial hub states.[31] The effect for non-financial hubs is not statistically significant. The results remain consistent and somewhat stronger, when using the number of deals instead of the capital invested as dependent variable (column II).

[28]Table B5 reports the results of the first-stage regressions.

[29]We are grateful to a number of seminar participants for helpful suggestions on potential alternative instruments.

[30]For further information see U.S. Department of Justice –Grants and U.S. Department of Justice, Office of Justice Programs –Grants/Funding.

[31]Table B7 reports the results of the respective first-stage regressions. These results are somewhat weak with the only coefficient for the interaction term $\mathbb{1}\,[\text{Fin Hub}_s] \times \ln(\text{DoJ grants})_{s,t}$ in column II being significant at the 10.5% level.

B Appendix - Tables

Table B1: Regulatory stringency and deal-making activity: instrumental variable regressions, first-stage results

	Dependent variables			
	$\text{CryStIn}_{s,t}$	$\mathbb{1}[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$	$\text{CryStIn}_{s,t}$	$\mathbb{1}[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$
Explanatory variables	(I)	(II)	(III)	(IV)
$\overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}}$	-0.825	-0.076	-1.425^{**}	-0.131
	(0.56)	(0.22)	(0.57)	(0.28)
$\mathbb{1}[\text{Fin Hub}_s] \times \overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}}$	1.253^{*}	0.868^{**}	1.534^{**}	0.942^{**}
	(0.62)	(0.35)	(0.59)	(0.40)
Observations	7,595	7,595	7,595	7,595
Number of closest states in the average	± 10	± 10	± 15	± 15

Note: Monthly data from 2010 to 2022. The sample includes all states except for Alaska and Mississippi, for which there is no information on VC crypto activity. Fin hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 above the median of the distribution. The entries denote the first-stage coefficients of a panel-IV regression where $\text{CryStIn}_{s,t}$ is instrumented with the one period lag of the out-of-state average of CryStIn over the states that rank $p \in \{10, 15\}$ positions above and below s in the ranking of total venture capital raised for the period 2000–2009. Regressions include state- and time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Table B2: Regulatory stringency and deal-making activity: instrumental variable regressions with a different definition of financial hub

	Dependent variables			
	$\ln(\text{capital raised})_{s,t}$		$\ln(\text{number of deals})_{s,t}$	
Explanatory variables	(I)	(II)	(III)	(IV)
$\text{CryStIn}_{s,t}$	-1.062	-1.306	-0.573	-0.740
	(0.79)	(1.23)	(0.45)	(0.74)
$\mathbb{1}[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$	3.544^{*}	3.694^{*}	2.225^{*}	2.291^{*}
	(1.78)	(2.07)	(1.17)	(1.31)
Observations	4,960	4,960	4,960	4,960
Number of closest states in the average	± 10	± 15	± 10	± 15
F-stat	1.97	1.80	1.81	1.75
Weak-IV Anderson-Rubin test, statistic	9.467	9.443	9.736	9.125
Weak-IV Anderson-Rubin test, p-value	0.009	0.009	0.008	0.010

NOTE: Monthly data from 2010 to 2022. The sample includes all states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 falling in the top- or bottom tercile of the distribution, with the exception of Alaska and Mississippi, for which there is no information on VC crypto activity. Fin hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 falling in the top- (ie financial hubs) or bottom tercile of the distribution. The entries denote the second-stage coefficients of a panel-IV regression where $\text{CryStIn}_{s,t}$ is instrumented with the one period lag of the out-of-state average of CryStIn over the states that rank $p \in \{10, 15\}$ positions above and below s in the ranking of total venture capital raised for the period 2000–2009. Regressions include state- and time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Table B3: Regulatory stringency and deal-making activity: instrumental variable regressions with a different definition of financial hub, first-stage results

	Dependent variables			
	$\text{CryStIn}_{s,t}$	$\mathbb{1}\,[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$	$\text{CryStIn}_{s,t}$	$\mathbb{1}\,[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$
Explanatory variables	(I)	(II)	(III)	(IV)
$\overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}}$	-1.353^{*}	-0.292	-1.759^{**}	-0.514
	(0.71)	(0.23)	(0.80)	(0.36)
$\mathbb{1}\,[\text{Fin Hub}_s] \times \overline{\text{CryStIn}_{\mathcal{S}_{s,p},t-1}}$	1.581^{*}	0.765^{*}	1.789^{**}	1.022^{**}
	(0.82)	(0.40)	(0.83)	(0.47)
Observations	4,960	4,960	4,960	4,960
Number of closest states in the average	±10	±10	±15	±15

NOTE: Monthly data from 2010 to 2022. The sample includes all states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 falling in the top- or bottom tercile of the distribution, with the exception of Alaska and Mississippi, for which there is no information on VC crypto activity. Fin hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 falling in the top- (ie financial hubs) or bottom tercile of the distribution. The entries denote the first-stage coefficients of a panel-IV regression where $\text{CryStIn}_{s,t}$ is instrumented with the one period lag of the out-of-state average of CryStIn over the states that rank $p \in \{10, 15\}$ positions above and below s in the ranking of total venture capital raised for the period 2000–2009. Regressions include state- and time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Table B4: Regulatory stringency and deal-making activity: instrumental variable regressions with a different instrument

	Dependent variables					
	$\ln(\text{capital raised})_{s,t}$			$\ln(\text{number of deals})_{s,t}$		
Explanatory Variables	(I)	(II)	(III)	(IV)	(V)	(VI)
$\text{CryStIn}_{s,t}$	-1.036^{**}	0.202^{**}	-0.054	-0.787^{**}	0.103^{*}	-0.040
	(0.51)	(0.09)	(0.04)	(0.37)	(0.06)	(0.03)
$\mathbb{1}\,[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$	2.012^{**}			1.480^{**}		
	(0.88)			(0.64)		
Observations	7,595	3,875	3,720	7,595	3,875	3,720
Sample	Pooled	Fin hub	Non fin hub	Pooled	Fin hub	Non fin hub
F-stat	2.60	4.95	1.57	2.66	3.43	1.92
Weak IV Anderson-Rubin test, statistic	11.395	2.700	1.571	13.617	2.141	1.387
Weak IV Anderson-Rubin test, p-value	0.003	0.100	0.210	0.001	0.143	0.239

NOTE: Monthly data from 2010 to 2022. The sample in columns I and IV includes all the states except for Alaska and Mississippi, for which there is no information on VC crypto activity. The sample in columns II and V includes financial hub states only. The sample in columns II and VI includes non financial hub states only. Fin hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 above the median of the distribution. The entries denote the second-stage coefficients of a panel-IV regression where $\text{CryStIn}_{s,t}$ is instrumented with the one period lag of the out-of-state average of CryStIn ie $\overline{CryStIn}_{s,t-1}$. Regressions include state- and time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Table B5: Regulatory stringency and deal-making activity: instrumental variable regressions with a different instrument, first-stage results

	Dependent variables			
	$\text{CryStIn}_{s,t}$	$\mathbb{1}[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$	$\text{CryStIn}_{s,t}$	$\text{CryStIn}_{s,t}$
Explanatory variables	(I)	(II)	(III)	(IV)
$\overline{\text{CryStIn}}_{j\neq s,t-1}$	-47.290^{***}	-26.840^{***}	-47.314^{***}	-47.281^{***}
	(0.12)	(5.56)	(0.12)	(0.23)
$\mathbb{1}[\text{Fin Hub}_s] \times \overline{\text{CryStIn}}_{j\neq s,t-1}$	0.007	0.730^{**}		
	(0.02)	(0.31)		
Observations	7,595	7,595	3,875	3,720
Sample	Pooled	Pooled	Fin hub	Non fin hub

NOTE: Monthly data from 2010 to 2022. The sample in columns I and II includes all the states except for Alaska and Mississippi, for which there is no information on VC crypto activity. The sample in columns III includes financial hub states only. The sample in columns IV includes non financial hub states only. Fin hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 above the median of the distribution. The entries denote the second-stage coefficients of a panel-IV regression where $\text{CryStIn}_{s,t}$ is instrumented with the one period lag of the out-of-state average of CryStIn ie $\overline{CryStIn}_{s,t-1}$. Regressions include state- and time fixed effects. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Table B6: Regulatory stringency and deal-making activity: using Grants awarded by the US Department of Justice as instrument

	Dependent Variable	
	$\ln(\text{capital raised})_{s,t}$	$\ln(\text{number of deals})_{s,t}$
Explanatory Variables	(I)	(II)
$\text{CryStIn}_{s,t}$	-0.249	-0.304
	(0.79)	(0.30)
$\mathbb{1}\,[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$	2.137^{*}	0.745^{**}
	(1.10)	(0.34)
Observations	7,595	7,595
F-stat	1.87	2.64
Weak IV Anderson-Rubin test, statistic	8.159	8.159
Weak IV Anderson-Rubin test, p-value	0.017	0.017

NOTE: Monthly data from 2010 to 2022. The sample includes all states except for Alaska and Mississippi, for which there is no information on VC crypto activity. Fin hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 above the median of the distribution. The entries denote the second-stage coefficients of a panel-IV regression where $\text{CryStIn}_{s,t}$ is instrumented with the one period lag of the natural logarithm of the amount of grants awarded by the US Department of Justice, Officer of Justice Programs in each state ie DOJ $\text{grants}_{s,t-1}$. Regressions include state- and year fixed effects to account for the fact the the DOJ publishes grant opportunities on a fiscal year basis. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.

Table B7: Regulatory stringency and deal-making activity: using Grants awarded by the US Department of Justice as instrument, first-stage results

	Dependent Variable	
	$\text{CryStIn}_{s,t}$	$\mathbb{1}\,[\text{Fin Hub}_s] \times \text{CryStIn}_{s,t}$
Explanatory Variables	(I)	(II)
$\ln(\text{DOJ grants})_{s,t-1}$	0.009	−0.012
	(0.01)	(0.01)
$\mathbb{1}\,[\text{Fin Hub}_s] \times \ln(\text{DOJ grants})_{s,t-1}$	0.003	0.027
	(0.02)	(0.02)
Observations	7,595	7,595

NOTE: Monthly data from 2010 to 2022. The sample includes all states except for Alaska and Mississippi, for which there is no information on VC crypto activity. Fin hub is an indicator variable that takes value one for states with aggregate sectoral GDP for the Finance and Insurance sector for the period 2000–2009 above the median of the distribution. The entries denote the first-stage coefficients of a panel-IV regression where $\text{CryStIn}_{s,t}$ is instrumented with the one period lag of the natural logarithm of the amount of grants awarded by the US Department of Justice, Officer of Justice Programs in each state ie $\text{DOJ grants}_{s,t-1}$. Regressions include state- and year fixed effects to account for the fact the the DOJ publishes grant opportunities on a fiscal year basis. Standard errors in parentheses are clustered by state: * $p < .10$; ** $p < .05$; and *** $p < .01$.