A Brief History of Financial Risk

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Abstract:
The starting point of nearly every asset pricing model is that investors understand that stocks outperform bonds due to their risk characteristics. We show that this was not a well understood concept until recently. Early 20th century articles often categorically rejected equities as being "speculations" that were not even an investment, or viewed stocks versus bonds as primarily being about inflation. The idea of risk as standard deviation of single period returns was pioneered by Markowitz (1952), and not popular until the 1960s. Before this, "risk" meant the chances of losing money in the long run, consistent with applying metaphors from bond defaults, and a prevailing focus on long-run cumulative performance rather than single-period performance. This conception of risk means that higher mean returns make an asset safer, not riskier, and leads to inconsistent evaluations of the same assets at different horizons (both of which we document examples of). The idea of risk as coming from correlation with consumption was absent until Lucas (1978). The necessary ideas about equity risk in Mehra and Prescott (1985)'s equity premium puzzle were almost totally absent until shortly before the article itself.

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One great virtue of academic citations is to allow one to chart the history of ideas. When each new work documents the previous sources it draws on, one can track down the original works, and find out when each was discovered, and by whom. This may seem obvious almost to the point of tautology. But it has a flipside that is less obvious. The concepts where *no citations* are given, or none are considered needed, are precisely those whose history is most likely to be misunderstood. These tend to be ideas that are considered so obvious and intuitive that they must either be eternal, or whose origins must be lost in the mists of time. Sometimes this may be true – "when did people discover arithmetic" is probably hard to answer cleanly. But other times, the dates and citations can be found, but the work of understanding them has not been done.

The equity premium puzzle, described in Mehra and Prescott (1985), refers to the finding that the returns on equities are sufficiently high relative to bonds, that even after taking into account the risk of each asset, it is hard to understand why people hold bonds. Importantly, every single underlying concept needed to formulate this puzzle is in the "no citation needed" category. That is, suppose academics were required to write down an original citation for concepts like:

1. How do you measure the return on a portfolio of stocks?
2. Do stocks have higher returns than bonds?
3. How do you measure risk for a portfolio of stocks?
4. Is the higher return on stocks compensation for them having higher risk?

We submit that most would have no idea what to include, and quite possibly would revert to citing Mehra and Prescott (1985) itself (even though it only quantified numerically the tradeoffs involved in these concepts, rather than inventing the concepts themselves). This raises the very real and important question – are these *actually* timeless and eternal concepts, or are they instead surprisingly recent discoveries whose origins have simply been forgotten?

We focus on the third and fourth questions above – how did people think about the risk of equities at different points in history, and did they view this risk as requiring higher expected returns?[1] The contemporary understanding of the risk in equities emphasizes their correlations with consumption growth (e.g Cochrane 2009), while earlier versions in the CAPM considered the

[1] In our companion paper, Hartzmark and Solomon (2026), we consider in detail the history of the first two questions of how people actually measured returns, and the knowledge about the level of returns of each asset.

standard deviation of market returns as being the central risk input to computing a market price of risk. The idea that risk and return ought to trade off at the asset class level is nowadays so fundamental as to be almost taken for granted. This can be seen by the fact that in common usage academics frequently use as *synonyms* several terms which are actually quite conceptually distinct, namely i) "expected excess return on equities" (the purely statistical measure), ii) "equity premium" (the idea that stocks have *higher* returns on average than bonds), and iii) "equity risk premium" (the idea that the higher returns on stocks are compensation for their higher risk).

But did people always have this understanding? Rather than just being a matter of historical curiosity, this goes to the very heart of asset pricing models. Mehra and Prescott (1985), like the vast majority of asset pricing models, is a rational expectations model. The parameters, concepts, and tradeoffs are all assumed to be understood by participants during the period of returns being examined. This assumption is common in most of the papers that tried to explain the puzzle that Mehra and Prescott (1985) presented, either by modifying the model of risk, or using the same model of risk but with additional frictions or assumptions.[2] Very few papers have considered the possibility that people had radically simpler or incorrect assessments of risk, or simply didn't understand any of the parameters at all.

Our paper proceeds from a simple premise – to understand what people *thought* in past periods, one should carefully examine what they *wrote* in past periods. We undertake a close reading of a large number of 20th century academic finance articles and practitioner books to answer the question of how these participants thought about the risk of equities. We describe how close such views were to modern interpretations of risk, and the intellectual developments that led to changes in mindset. Our results document a largely forgotten history of very alien and confused ideas about risk that persisted until at least the 1960s.

Merely reviewing papers and describing what they say runs several risks as a method of inquiry. First, the search may be incomplete, and the attitudes in the books and articles examined may differ from other omitted sources, like newspapers, magazines, less prominent periodicals, or foreign language sources. Second, many key players did not write such articles, or may not

[2] Examples of different models of risk include external habits (Cambell and Cochrane (1999)) and long run risk (Bansal and Yaron (2004)). Other model variations include rare disasters (Barro (2006), Gabaix (2012)), limited participation (Constantinides, Donaldson, and Mehra (2002)), liquidity (Pastor and Stambaugh (2003)), and others.

have said everything they knew to preserve their trading advantages. Finally, what the authors wrote is only a subset of what they thought, and we may misinterpret their statements.

However, our position becomes much stronger if we can understand *why* they thought what they did. Many seemingly puzzling early attitudes towards risk in equities become much less puzzling in the light of several key facts described in Hartzmark and Solomon (2026), namely

i) Equities as an asset class in the US were very small and unpopular during the first half of the 19th century, so most people had little experience with them

ii) Historical data was difficult to come by, so most people had little idea about basic performance facts

iii) The concrete understanding of how to compute that performance was similarly confused until approximately the 1960s

iv) Performance was mostly considered as long-horizon cumulative outcomes, rather than as a distribution of single-period returns

Given these facts, how might investors have thought about the risk in equities? Our central insight, given the facts above, is that early 20th century investors attempted to make sense of stocks initially by imperfect analogies to bonds, which were an older and more common asset classes they understood better. There are a number of predictions about how simplifying assumptions could be applied to understand stocks via approximate analogies to bonds:

-Risk will be principally considered as the chances of something like “default” at a long horizon. The “chance of default” in the analogy is most likely the chance the investor loses money relative to an initial investment, but may be the chance the business fails or something similar

-Stock expected returns will be thought of like bond yields, in terms of comparing price today to final value for an investor who holds for a long period (there is no definite “maturity”, so “a long period” may be substituted)

-To the extent that intermediate risk is considered, it will be the riskiness of intermediate dividends (which analogize to coupons), not intermediate price changes

-Intermediate price changes will tend to get ignored, or will be the area where people display the least understanding or theory of how they ought to work (because intermediate bond price changes mean revert in the absence of default, but stock price changes do not).

We find evidence of all of these aspects of incomplete understanding in the history of thought about risk in the 20th century.

Some of the earliest views actually rejected the comparison between stocks and bonds outright, rather than trying to integrate them into any kind of single framework. As described in Smith (1924), the prevailing attitude towards equities in the early 20th century was that they were just gambling and speculation, and not even an investment (unlike bonds). That is to say, the assessment of their risk amounted to a categorical objection at the asset class level, such as in Chamberlain (1911). The closest modern equivalent would perhaps be the attitude of many towards cryptocurrency today – highly uncertain, full of scams and frauds, and best avoided.

The main alternative view was the Fisher (1912) argument that the difference between stock and bond performance was mostly due to inflation – in periods of high inflation, stocks will outperform bonds, due to bonds suffering lower real cash flows, but dividends being mostly immune. Fisher himself was surprisingly ambivalent on the overall risk differences between stocks and bonds, saying in Fisher (1912) that there was not much to judge between them, just different types of risks (as dividends were inherently uncertain, but bond coupons could decline sharply in real terms under inflation). This comparison of risks came from a qualitative consideration of scenarios where one could lose money, rather than any framework to quantify or aggregate them. Fisher described a general intuition that risk should be related to expected returns in Fisher (1906), and that this depended on individual attitudes to risk. But this risk adjustment in Fisher (1906) was only in the context of comparing today's price with future cash flows, and did not include any concept of risk adjusting future price changes. Finally, risk was also considered as purely subjective – there was not a clean sense of how risk should be measured, and no sense of how it should be priced. This lack of clarity persisted in later work, such as Fisher (1925).

While the principal context in Fisher (1912) was that bonds will do worse under a period of high inflation, the logic implies something Fisher (1912) never stated directly, which is that stocks should do *worse* than bonds under periods of deflation. This view was called into question in Smith (1924), who was the first to discover the equity premium, as Hartzmark and Solomon (2026) describe in detail. Smith (1924) documented that stocks also did better than bonds in

deflationary periods in the late 19th century. He took this to be a refutation of the existing theory, which he had previously himself believed when he designed his tests.

Our most surprising finding is that nobody appeared to describe or conceive of risk as being the standard deviation of single-period returns until Markowitz (1952). Markowitz himself did not understand this point, and actually (wrongly) asserts the opposite, namely that when past authors referred to "risk", one can usually substitute in "variance" without loss of meaning. Hartzmark and Solomon (2026) describe in detail the second, subtler innovation - that returns were single-period outcomes over which the variance was measured. The measure of returns was not actually defined, as returns definitions did not solidify until Miller and Modigliani (1961).

The earlier conception of performance as long-run, cumulative outcomes (like a bond yield) meant that these authors did not generally *see* single period outcomes, and so did not think in terms of their volatility. This is trivially true for papers like Smith (1924) and Fisher (1912) that only computed a single long horizon outcome, but also for papers that computed index outcomes by year, none of which report or discuss standard deviations of returns (e.g. Jackson (1928), Cowles (1938)). We are unable to locate a standard deviation of market returns that was published before Lintner (1965), which occurred in the context of the CAPM. As Miller (1960) notes, the contribution of Markowitz (1952) was still almost entirely theoretical in 1960, due to the difficulty of obtaining data before the CRSP tapes were produced, and so the idea of measuring return standard deviations took some time to develop. The lack of reported standard deviations of returns is not because earlier authors were unfamiliar with standard deviations in general, or found them too burdensome to compute – both Jackson (1928) and Cowles (1938) compute standard deviations in their paper for *other* purposes, but not as applied to market returns. Jackson (1928) refers briefly to common stocks swinging more wildly that preferred stocks, but makes no attempt to quantify this.

In principle, a focus on long horizon outcomes does not preclude an investor from correctly applying a mean-variance framework. However, in practice, something like "the variance of 30 year returns" is mostly an abstract concept that one forms by taking the variance of monthly or annual returns (which one *can* observe, if it is actually computed) and extrapolating it to an understanding of how it will apply at long horizons. But someone who starts with these long

horizon returns under a short dataset that features many overlapping periods (as Fisher and Lorie (1964) did) will not actually *see* variation in independent long-horizon returns.

Instead, what these authors tended to consider as risk was the chance that the investment lost money at a long horizon. This is something that *is* easy to observe, and on its face is not an inherently unreasonable idea of risk to a long-term investor, especially under an analogy to bond default risks. Like long-horizon returns that went with it, this conception also lasted a surprisingly long time. For instance, Fisher (1965) studies stock level risk, and computes this as the probability of losing money for various random choices of stock and time horizon as the risk outcome. He does not compute or discuss standard deviations anywhere. Earlier papers also argued for this conception of risk explicitly. Graham and Dodd (1934), for instance, is widely described as pioneering value investing, but they maintained the same pre-Smith (1924) focus on "investments" versus "speculations", and differed mostly in arguing that not *all* stocks were speculations, nor all bonds investments. They define an investment to be an asset that produces a reasonable rate of return, as well as "safety of principal", requiring that it was unlikely to decline in value under all reasonably probable circumstances. Not only does this reinforce the "risk as probability of long term loss" concept, but this statement is impossible to interpret *except* as applying to long term outcomes, otherwise there would be no stocks that one could possibly say would only decline in price in "the most unlikely contingencies". Their most prominent discussion of this risk of loss in stocks comes explicitly in the context of a comparison with bond defaults.

This aspect of "risk as probability of loss" and "focus on long horizon outcomes" also highlights a number of problems with the early 20th century mindset, that make the views difficult to reconcile not just with modern asset pricing models, but with almost any rational asset pricing model. Risk as probability of loss manages to combine both mean returns and the standard deviation of returns, thus conflating different parts of the modern risk/return tradeoff. This leads to interpretations that are deeply at odds with standard assessments, because higher expected returns reduce the chances of losing money at long horizons, and so *make a stock safer*, rather than being compensation for risk. Indeed, this is the argument that Graham and Dodd (1934) make for value stocks, the exact opposite of the Fama and French (1993) view. Second, because higher mean returns both increase reward and decrease risk, there is not even a clear sense of

what a risk/return tradeoff looks like. Instead, the asset with the higher expected return is often simply described as "better". This is true in Jackson (1928) (comparing common versus preferred stocks) and Smith (1924) (comparing stocks versus bonds).

Third, when risk is considered as the probability of loss, even when returns are i.i.d, this probability declines as the horizon gets longer. On a long enough time frame, the mean effect dominates the variance effect for determining loss probability. The *variance* of cumulative returns goes up, so long horizon returns are indeed still risky, but the *probability* of a negative outcome nonetheless goes down (with these being reconciled by the fact that the outcomes which are negative are worse by larger amounts). This has the effect that the riskiness of the same asset appears different at longer versus shorter horizons. This fact was observed by both Jackson (1928) and Fisher (1965), who viewed stocks as less risky at longer horizons than shorter horizons.

This perception of risk as declining with horizon seems likely to produce expected utility violations as described in Samuelson (1963). He proved that it cannot be rational to take the same identical bet many times in a row if it is irrational to take it once. That is, if stock returns are i.i.id, an investor cannot rationally think it is a good idea to hold stocks for a long horizon if the same stocks are deemed too risky to invest in at a one month or one year horizon. These kinds of mistakes, however, are surprisingly subtle. Samuelson (1963) was motivated by a discussion with a colleague of his who wanted to make exactly this form of "many bets with a 50/50 tradeoff of $200 vs -$100, but not a single such bet". Even long after Samuelson (1963), Paul Samuelson wrote a follow-up piece (Samuelson (1979)) arguing against blindly applying rules like the Kelly Criterion and thinking that this also eliminated risk, but felt the mistake was so obvious that the article was written in words of only one syllable. The fact that many professors kept making the mistake, even 16 years after the original article, suggests it may not be so obvious after all.

Meanwhile, consistent with a bond mindset, another set of papers argued for the existence of a risk/reward tradeoff primarily through the lens of the riskiness of cash flows. Both Fisher (1906) and Williams (1938) considered the idea of equity yields that compared price today with estimated cash flows, and argued that the lower price relative to expected cash flows reflected the higher return to cash flow risks. Fisher (1906) even went as far as to speculate that standard deviation may at some point be a useful way to measure this. However, this applied to

the uncertainty of future cash flows over different states, which is hard to observe. As a result, this argument was mostly one of revealed preference – a given price could inform what the discount was for risk under certain assumptions about average cash flows, but not say how one ought to construct such a discount in theory, or whether a given discount was too high or too low. Future prices tended to be left out of the discussion altogether.

An analysis of risk-related terms used in New York Times and Wall Street Journal financial markets columns confirms the broad changes in focus described above. Early in sample, most discussion of risk either directly described pure investor psychology, or emotionally neutral but qualitative descriptions of ongoing risk. There were relatively few terms directly describing price movements. For instance, in the category of pure psychology, during the ten years ending in 1915 "Fear" and "Panic" were 22% and 19% of all risk words (ranks 2 and 4). By the end of the sample, "Fear" (which can be rational or irrational) was still popular, being the top ranked word at 16% in 2007. "Panic", which is generally irrational, had fallen to 14th rank and 1.5% of words.

The focus on terms closer to modern, quantitative ideas of risk only developed around the 1960s. In 1915, "Volatil*" was only the 12th most common risk word, at 0.3% of all risk-related words. In the ten years ending in 1951, just before Markowitz (1952) was published, "Volatil*" was still only the 10th most common risk word, at 2% of risk words. However, by the ten years ending in 1971, it was the 3rd most common risk word, at 11% of risk words. Even the term "Risk" itself was only the 5th ranked word in 1915 at 5% of risk-words. By 1981, it was the second ranked word, at 16%. Early discussion of risk was more qualitiative. In 1915, "Uncertainty" and "Fluctuat*" were ranked first and third, at 23% and 21% of risk words. By the end of the sample, uncertainty was only ranked 6th (6% of risk words), while "Fluctuat*" was down to 15th, and less than 1% of risk words. The risk in terms related to quantitative versions of risk is mirrored by the rise of terms describing past price movements. "Sell-Off" peaked in 1963 as the top-ranked risk word, at 17% of risk words. "Correction" peaked in 1964 as the second-ranked risk word, at 13.

Finally, the focus on risk as coming from the correlation of asset returns with consumption growth is even more modern. There is no discussion of this idea prior to the pioneering work of Lucas (1978). Meanwhile, the fact that the intertemporal CAPM of Merton (1973), which has many possible state variables an investor may care about in partial equilibrium, reduces to a single

concern with consumption correlations when solved in general equilibrium, was not known until Breeden (1979). Earlier empirical work that considered how equity returns correlated with macro variables, such as Smith (1969), did not consider consumption growth as an important macro variable to evaluate. The first empirical consumption correlation was not published until Grossman and Shiller (1981). In other words, the focus on consumption correlations as the measure of risk was developed only a few years before Mehra and Prescott (1985) itself.

Academics have a strong tendency to assume that when a new idea is published, the academics themselves are, in some sense, "the last to know", and other market participants already understood the basic relationship. This assumption is often treated as almost innocuous. We argue that this is wrong – not only is this assumption non-obvious, it is both *falsifiable*, and largely *falsified.* The publication of return anomalies is shown to affect those anomalies themselves in McLean and Pontiff (2016), indicating that the market *didn't* know about these findings until they were published. The "last to know" assumption, when stretched to its limit, becomes almost nonsensical - if every investor already knew what a Sharpe Ratio was, why did Sharpe (1964) win a Nobel Prize? Intuitively, papers that garner thousands of citations probably do so because they were less than fully obvious almost by definition. The equity premium puzzle fits firmly in this tradition. It is likely that Mehra and Prescott (1985) were probably the first people to think about the equity premium puzzle in the terms they did.

In sum, the prevailing views on risk in equities were that

-Equities were not even an investment until 1924, or at best were considered a way to bet on inflation

-Equity risk mostly meant the chances of losing money at long horizons from the early 1920s until 1952 at a bare minimum, and realistically until the mid-1960s

-Equity risk switched to meaning the standard deviation of market returns only in the mid 1960s

-Equity risk as consumption growth correlation only emerged in the late 1970s and early 1980s

The history that we document presents deep challenges to current theoretical models of learning. Most theoretical models are static with respect to the understanding of the agents in the model – they understand both the structure and the parameters of the model at the beginning of the period (e.g. the CAPM in Sharpe (1964), consumption asset pricing in Lucas

(1978), habits in Campbell and Cochrane (1999)). In models where learning does occur, it tends to either be in the form of parameter uncertainty, or model uncertainty. In the former, agents understand the structure of the model, but only update slowly about the parameters that drive it (e.g. Dufresene, Johannes and Lochstoer (2016)). In model uncertainty, agents understand the structure of both models at the beginning of the period, but are unsure which model is the true one (e.g. Hansen and Sargent (2001), Hansen (2007)). By contrast, the history we document is better characterized as *model discovery* – there is a concrete point in time before which a model is not known, and after which it is. Such understanding could be viewed as a complete structural break where participants shift from one model to another, a gradual diffusion of understanding, or the introduction of uncertainty as to the correctness of the two models. We commend such study to theorists, because it seems to accurately describe an entire class of learning that actually occurs in the real world, yet is almost totally unexplored in theoretical terms.

1. How to Use This Paper

Because the structure and methodology of this paper is unusual relative to most finance papers, it is worth explaining in more detail than usual what the concrete contributions are of a paper with no tables and no equations, and what we hope that a reader will get out of it.

The narrowest contribution is to give a history of thought from a period that is almost totally forgotten today. It is very rare to find citations in major finance journals of any finance articles written earlier than Markowitz (1952). (XXX Should we quantify this?). But it is not at all uncommon to analyze stock returns back to 1926 (using CRSP data), or until 1871 (using Robert Shiller's data). This means that considerable parts of the period being analyzed represent decisions made by people whose whole worldview is almost completely alien to most finance academics - both in the sense of being strange and different, and in the sense of being unfamiliar and unstudied. Because decisions made by these people created the prices we are analyzing, we think there is value in trying to take seriously the question of what those people actually thought.

Second, we argue for the methodological importance of close reading of historical articles. Historical writings ought to be considered important evidence of beliefs, in a similar way to how survey evidence was once dismissed out of hand, but now is increasingly viewed as an important way to obtain additional insights that archival data cannot supply. The aspect of "close reading"

here is important. This means not just reading the original articles (instead of just later authors' summaries of them), but also trying to understand the worldview behind the particular statements, which is often more elusive. For instance, our headline claim that early authors mostly thought about stocks using imperfect analogies to bonds is not something that these authors tend to spell out, nor even something they were necessarily aware of themselves. But realizing this is a genuine advancement in our understanding, because it is not known, and not obvious just from the headline summaries of the papers.

In this regard, our paper also has far more than the ordinary number of direct quotations from the papers in question. The practice in finance, unlike in history, is to assert and summarize what previous papers are alleged to have found, usually without any direct quotations at all. This may or may not suffice for standard empirical results, but when the words are the first order subject of inquiry, they must be examined directly. The ordinary space devoted to tables is here instead devoted to quotes, the raw material of our analysis. Readers are encouraged to read them (and, if desired, refer back to the original context) and ask "is this passage actually saying what they claim it is saying? Is there any other way to interpret these statements?". We give our summary and synthesis of what these authors thought, which is important, but readers should not just take our word for it. Our current focus is on the history of risk, but the broad methodology we use is potentially applicable to many other questions. Hartzmark and Solomon (2026) carry out a similar inquiry for how performance is measured.

Third, the changes in thought we document also serve as a cautionary note for authors trying to solve a problem of lack of data in modern periods by collecting additional data from further in the past (e.g. Linnainmaa and Roberts (2018), who study accounting anomalies before the standard Computstat data in the 1960s, Goetzmann and Huang (2018) who study momentum in Imperial Russia). While this task can be valuable, the further back one goes with financial market data, the greater the risk that one is studying periods where the entire understanding of the people involved was radically different. The extent of this problem depends very much on the particular asset, however, and how much it has changed over time. It seems plausible that interest rates on loans made to sovereigns, like in Rogoff, Rossi, and Schmelzing (2024), were probably

more likely to be understood by the participants involved in similar ways across many centuries. Whereas stocks in the 19th century were very different to stocks in the 20th century.

Fourth, our paper is also intended as part critique and part corrective of some of the other papers on the history of financial thought. In particular, there is sometimes a tendency to take a "glass is half full" perspective on historical authors. Papers like Goetzmann and Ibbotson (2005) and Dimand (2007) provide excellent summaries of many of the important historical articles, but sometimes tend to be overly charitable (in our opinion) in terms of filling in gaps and omissions in historical work with contemporary understanding. That is, the main focus is on ways that historical writing can be seen to influence, reflect and presage later formalizations. This is a fine and important task to undertake. However, we argue that it is important to not neglect the second component – understanding the ways that historical authors presented arguments that were either incomplete, confused, or simply wrong according to modern understanding.

Indeed, when done properly, this task is not intended to mock or diminish the contributions of these authors – quite the contrary. It is only by properly understanding past mistakes and past confusion that we can come to appreciate *how hard it is* to make genuine advancements in knowledge. The perspective that all these ideas are vaguely understood going far back into the distant past significantly diminishes the actual importance of studying financial history. One gets the incorrect perspective that the modern understanding is basically fine to apply arbitrarily far back, unless one has a particular curiosity about history. It also makes it more difficult to appreciate which articles represented genuinely difficult innovations. For instance, Irving Fisher is frequently cited in this paper as an author showing early, partially confused ideas. But this is because the scale of his contriubtions was very large, and it was difficult to get the entire answer right the first time. An appreciation of both the insights and the mistakes leads one to better appreciate certain authors, and the difficulty of what they were trying to undertake.

Finally, studying the mistaken and incomplete understanding of intelligent authors in earlier historical periods is a fertile area for generating new theories in behavioral finance. It is quite likely that various forms of these mistakes still persist to this day, precisely because the mistakes are subtle, both to the people making them, and to outside observers. For instance, the idea that participants may view risk as primarily the chances of losing money at a long horizon is

one with potentially wide applications. It may explain the appeal of structured products, which have very bad negative expected returns (Celerier and Vallee (2017), but often include components like capital guarantees, which would appeal greatly to investors with such an understanding of risk. Second, it may also explain the mindset behind "hodling" in cryptocurrency. Many early cryptocurrency investors hold allocations that are enormous relative to standard portfolio choice guidance, even under very wide ranges of beliefs (e.g. Duchin, Solomon, Tu and Wang (2025)). The standard explanation is that these people are now wealthy enough that they are almost risk neutral. But their behavior tends to be greatly at odds with other similarly wealthy individuals. One alternative explanation is that when one has large initial gains, the probability of a long horizon loss is now very small, so a very large cryptocurrency exposure is not actually perceived as very risky. And the emphasis on long horizon holding ("hodling") as response to the very high risk of cryptocurrency makes sense in a framework like we discussed – doing the same risky thing over long periods can be perceived as safe due to low loss probability, in a way at odds with standard risk aversion and expected utility maximization. The application of other similar concepts, especially those arising from analogies between stocks and bonds, is another promising angle for future research.

2. *Markowitz (1952) pioneered thinking about risk as standard deviation.*

While we present a general survey the history of equity risk in the 20th century, we begin with our stated conclusion, and the central novel contribution of our paper. Markowitz (1952) pioneered two aspects of modern finance for which he is not generally given credit. The first, and most important, is that risk should be measured as the standard deviation of single-period returns. The second, and more subtle, is that performance should be conceived of as a distribution of single-period returns at all. We describe this second finding in Hartzmark and Solomon (2026), and focus primarily here on the innovation on the risk dimension.

Markowitz (1952) was hugely important for solving the portfolio choice problem for an investor who traded off mean and standard deviation. That Markowitz (1952) examines the standard deviation of returns as the measure of risk is obvious. That this was a substantive and novel contribution to finance goes unnoticed, likely in large part because Markowitz himself did not realize it was a contribution! Indeed, he explicitly *disclaims* it as a contribution:

"The concepts 'yield' and 'risk' appear frequently in financial writings. Usually if the term 'yield' were replaced by 'expected yield' or 'expected return,' and 'risk' by 'variance of return,' little change of apparent meaning would result."

We argue that Markowitz (1952) is completely wrong on this substantive claim. His assessment seems best understood as an example of the curse of knowledge, as in Camerer, Loewenstein and Weber (1989). That is, people who know something have genuine difficulty credibly putting themselves in the shoes of someone who does not know it. They will tend to assume that what they know ought to be able to be inferred by other people, when it often cannot. Markowitz foresightedly thought of risk in terms of standard deviation of returns, and assumed that this must have been the obvious thing everyone else thought. We will first show that even where this substitute *could* be made, it was *not* made. More importantly, many authors wrote in terms that cannot be reconciled with the idea of risk as the volatility of single-period returns at all.

In Hartzmark and Solomon (2026), we explore the second novel contribution of Markowitz (1952) – to consider performance as a distribution of single-period returns *at all*. This is indeed related to the second misconception above – *yield*, as mostly used by early authors, was like a bond yield – a long horizon cumulative outcome. This perspective also hampered the development of standard deviation as a measure of risk. A focus on long-horizon empirical measures meant that many authors did not even *see* intermediate returns, and so did not think in terms of their volatility. Second, in the case of bonds, intermediate price changes mean revert in the absence of default (which is the only way cash flows can change). This further pushes one away from thinking about their volatility in the framework of a distribution of i.i.d. returns.

3. Concepts of Equity Risk When the Equity Premium Was Discovered in 1924

The equity premium was discovered in Smith (1924). Before its publication, it seem very likely that people were not generally aware that stocks beat bonds *at all*. That is, they were not just uncertain of whether the higher returns to stocks were worth the risk, or how large the overall outperformance was. They were generally incorrect in their belief about the *sign* of the equity premium. While Hartzmark and Solomon (2026) describe the aspects of this paper relating to

measurement of performance and the knowledge of the equity premium, Smith (1924) is also useful for our purposes as a snapshot of different views about risk that prevailed before this time.

The two principal views about risk that prevailed before Smith (1924) were i) that stocks were not even an investment, and instead were "speculations", or ii) that the return on stocks versus bonds was mostly about inflation, most associated with Fisher (1912). Smith (1924) summarizes these views, and considers the finding of an equity premium to be a refutation of both.

4. Stocks as Speculations

Smith (1924) characterizes what he considers to be the prevailing view at the time, that stocks were *not even investments*, but were in a different class of "speculations". As he writes:

> *"When the topic of conservative investment is under discussion, high grade bonds hold an unassailable position in the minds of most people, and the discussion usually resolves itself into weighing the relative merits of different issues of such bonds or how far it is safe to stray away from the most highly secured bonds in an effort to obtain a higher income return. Those who venture to suggest preferred stocks sometimes feel that they have gone as far as conservative opinion will support them. Common stocks are ordinarily left out of the discussion altogether.*
>
> *'Common Stocks are, in general, regarded as a medium for Speculation-not for Long Term Investment. Bonds, on the other hand, are generally held to be the best medium for Long Term Investment-free from the hazards of Speculation.'*
>
> *Is this view sound? What are the facts?"*

One can find this view repeated in other early authors such as Chamberlain (1911):

> *"Common stocks as such, are not superior to bonds as long-term investments, because primarily they are not investments at all. They are speculations."*

This category of "speculations" tended to be viewed as a species of gambling, with the attendant social ills that this caused. Hardy (1937) describes a historical attitude that is vastly at odds with the idea of equity investment as a safe and responsible activity suitable for the average man:

> *"The American attitude toward public gambling is strongly colored by our inheritance from Puritan New England, and consequently stock exchanges have always had to contend with*

a type of criticism which is almost unknown in Continental Europe and relatively weak in England. Since the World War there has been some relaxation of standards in this regard. But I see no evidence that this liberalizing tendency has helped the stock exchange. Its alleged gambling character is still brought forward as a blanket indictment against the whole system of stock speculation in every Congressional hearing....

This attitude was reinforced in the early 20th century by the much larger number of frauds and scams that accompanied stock trading before the passage of the Securities and Exchange Act (1934). As Hardy (1937) describes, this included

"[A]dmitted abuses such as manipulation, wash sales, trading against customers' orders, bucketing, and the dissemination of false news. Everyone agrees that these practices are against the social interest…"

The situation was especially bad on the less reputable New York Curb Exchange. Cowles (1938) justified why it excluded all quotes from this exchange, despite it making up 17% of volume, due to it having no guarantees that the prices quoted were actually genuine, or even necessarily that the securities themselves were genuine. We describe this situation in more detail in the Appendix.

It is against this backdrop of general perceptions of fraud and scams that Hardy (1923) ("Risk and the Management of Capital") describes different ways to profit from financial transactions, among others:

"- Purchase of securities with a view to obtaining income from interest or dividends.

- Purchase or sale of securities, land, contracts, or commodities, with a view to profit from price changes.

- Gambling transactions."

As noted in Hartzmark and Solomon (2026) when discussing the history of returns, the price change component and the interest/dividend component are viewed as *separate* ways to profit, rather than components in a single return measure. But there is also a surprisingly large focus on the category of gambling transactions, and the extent to which all trading on price changes is essentially the same thing. Hardy (1923) views the difference between gambling and speculation not in terms of risk-seeking and negative expected returns (as we might today), but based on whether the trading relates to some inherent and unavoidable risk that already exists, or whether

the risk was created for the purposes of trading. Importantly, the price change component is discussed nearly entirely as a zero sum component, and trading based on any expectation of price changes is described in ways that only make sense as descriptions of active trading, rather than an expectation that stock prices might rise in general over time:

> *"Success in all these lines depends on ability to forecast price changes, which in turn depends upon ability to weigh the importance of complicated and conflicting indications of the movement of demand and supply, not with absolute accuracy, but with greater accuracy than someone else does it. All speculation on the price of anything is essentially a contest."*
>
> *"For this reason all speculative activity is often condemned by moralists as a species of gambling. Speculation is not a form of gambling; gambling is a form of speculation. All forms of speculation are attempts to secure profits, to get for the service of one's capital (or labor) more than they are worth; that is, more than they would be worth if the relevant facts were generally known and account taken of them"*

Indeed, by modern standards it is striking that when Hardy (1923) is discussing "Risk and the Management of Capital" (the paper's title), he gives considerably more space and discussion to the gambling transactions, and the extent to which speculation should be considered different, than the role of price changes in stocks themselves. He views the social utility of stock market transactions as coming from the useful social functions behind the transactions:

> *"The only thing which differentiates gambling from " legitimate " speculation (hereafter referred to for brevity's sake as speculation) is that in speculation the risks are inherent risks of industry, and must be borne by someone if production is to go on."*
> *"[M]ost of the arguments which have been advanced to show that gambling is socially harmful apply also to other forms of business just in so far as they make one's income depend on luck in guessing the unknown. In varying degree, they apply to all forms of speculation and speculative business."*

The parallels that Hardy (1923) draws between gambling and stock market trading are not idle or incorrect, and are consistent with various recent strands of recent research. The huge rise in retail options trading has become concentrated in zero-day-to-expiry options (Bogousslavsky

and Muravyev (2024)), which seem most likely to meet the Hardy (1923) concept of side bets that are not related to inherent economic risks that have to be borne by someone. Meanwhile, Gao and Lin (2015) document that in Taiwan, exogenous variation in lottery jackpot amounts is related to retail stock trading activity, consistent with the two activities being substitutes.

Rather, what is most striking about Hardy (1923)'s description is that price changes are not discussed in any context *except* these zero-sum gambling transactions. This is especially important, because it occurs before the pre-Smith(1924) revelation that stock prices went up on average. Without this insight (which depending on significant data collection and analysis), the zero sum aspect of stock trading was apparently extrapolated to mean that winning was as likely as losing, and the whole enterprise was just about superior information. The risks of stock price changes were not viewed as something likely to be associated with higher expected returns, but viewed as a reason to avoid trading in anticipation of price changes altogether:

> *"Except for the few who possess superior information concerning the probabilities of a favorable outcome, gambling enterprises involve a greater risk than is justified by the chance of profit"*

It was in the context of this mindset that many people applied a blanket categorization of stocks as being risky forms of gambling that ought to simply be avoided.

5. Inflation and Irving Fisher

The chief alternative to the categorical assessment of equities as "not an investment" in the early 20th century was the view that stock versus bond performance was mostly a function of inflation. This is most associated with Fisher (1912), a volume in which authors evaluated different investment choices in periods of rising inflation. It argued that stocks were a superior investment versus bonds during such times. As Fisher describes in the introductory essay of Fisher (1912)

> "*In a general way during the recent decade and a half of upward prices, it is the bondholder who has lost and the stockholder who has gained. This is clearly shown in several of the papers which constitute this book. We may say roughly that the rise in prices has subtly robbed the bondholder for the benefit of the stockholder."*

Like Fisher's analysis of risk in general (e.g. Fisher (1906)), which we shall discuss below, this principally concerned the effect of inflation on the cashflows of stocks and bonds, rather than the effect of inflation on the price of each asset. For instance, in Fisher (1925), he again considers the various factors recommending stocks versus bonds in general, and gives as headings:

1. *The Stockholder has a chance of gain* (that earnings increases accrue to stockholders, but not bondholders)
2. *Steadiness of income* (that conservative earnings reinvestment and dividend policies mean dividends are actually fairly smooth)
3. *The Bondholder loses on rising prices* (that inflation reduces the real value of bond payments, but stockholders are protected, or do better)
4. *Diversification of stock investments* (that stock failure is idiosyncratic and can be diversified, but inflation hurts all bonds at once)

As can be seen from this list, the only aggregation of these factors is a qualitative one of synthesizing various possible outcomes – there is not a concrete metric of risk that can be measured. This is apparent in his claim that stocks have mostly idiosyncratic risk which can be diversified away, an idea incompatible with contemporary asset pricing models or any analysis of stock return correlations. Based on a similar, qualitative comparison, in his introductory essay in Fisher (1912), he writes that he sees stocks and bonds as having essentially *similar* levels of risk:

> *"There is not much to choose between the risks run by investing in stocks and risks run by investing in bonds. The two risks are, it is true, of different kinds, one being the risks of particular industries, and the other the risks of changes in the value of the gold dollar. But they are both real risks."*

Much of the argument in Fisher (1912) focuses on the effect of inflation on cash flows of each asset (point 3 above in Fisher (1925)). In Fisher (1925), he argues that stockholders actually benefit in terms of cashflows during periods of inflation, rather than just doing better than bonds:

> *"So when prices are rising-that is, when the dollar is shrinking - the bondholder complains of a "high cost of living ," which is another way of saying that he is losing real income and principal though nominally they remain the same. Moreover , since , during rising prices , the world is getting no poorer (but rather richer) in real goods , what the bondholder loses*

the stockholder must gain. Thus we find that the bondholder's "steady income" is a delusion and a snare , so long as we do not stabilize our dollar."

The title of the volume in Fisher (1912) is "How to invest when prices are rising", which shows the preoccupation with one side of the relationship – that high inflation benefits stockholders, and hurts bondholders. Strangely, the book does not directly address the likely opposite implication – that in periods of unexpectedly *low* inflation (or high *deflation*), bondholders will benefit at the expense of stockholders. However, the logic laid out in the book implies this result. This is the reason that Smith (1924) took it as the prevailing viewpoint that he went to test in the data, and that he himself previously believed – do bonds actually do better in periods of deflation? The answer, surprising both to him and the logic of Fisher (1912), was that they still do worse. Smith took this as a rejection of the Fisher worldview, though he does not mention Fisher by name in his book, nor indeed provide any references at all). As Smith (1924) describes in the introduction

"*These studies are the record of a failure - the failure of facts to sustain a preconceived theory. This preconceived theory might be stated as follows : While a diversity of common stocks has, without doubt, proved a more profitable investment than high-grade bonds in the period from 1897 to 1923, during which dollars were depreciating, yet with the upturn in the dollar, bonds may be relied upon to show better results than common stocks, as they did in the period from the close of the Civil War to 1896, during which the dollar was constantly increasing in purchasing power."*

"[These studies] failed, because, quite unexpectedly, they demonstrated that the premise upon which the preconceived theory rested, namely, that high-grade bonds had proved to be better investments during the period of appreciating dollars, could not be clearly sustained by any evidence available. The preconceived theory was, therefore, abandoned."

But even after Smith (1924) argued that the high returns of stocks refuted the inflation-centered view, Fisher (1925) never addresses the question of whether bonds ought to have higher returns during periods of deflation. He notes the Smith(1924) paper, and argues that bonds do not seem like great investments overall. He glosses the Smith (1924) result as claiming that *"during the falling prices following the Civil War stocks and bonds are about equal as to yield",* in

contrast to Smith's own assessment that they are higher. But he does not describe whether he views the Smith result as consistent with the earlier view, or a refutation of it.

Finally, Fisher (1925) has an interesting perspective on the causes for low bond returns – he attributes it to excess behavioral demand for bonds, which seems quite plausible even today:

> *"Thus the bondholder pays dearly for his supposed safety and regularity of income. True, the stockholder buys a lottery, but the facts show that the price he pays is below the mathematical chance . The lottery tickets are unduly cheap , just because of the traditional preference for bonds, which are unduly dear."*

However, aside from his empirical claims, his statements that bonds and stocks have similar risks imply that they ought to earn *similar* returns, a perspective that almost nobody believes today.

6. Market Standard Deviations Not Computed Until 1965

6.1 Early papers did not compute standard deviations before Markowitz (1952)

The complete absence of standard deviation of returns as a measure of risk was not just a feature of theoretical papers discussing risk, but also the early empirical papers studying market performance. Contradicting the Markotwitz (1952) claim that risk nearly always meant the variance of returns, none of the papers which studied market performance in the early 20th century ever computed a standard deviation of returns. Most of the papers do not even use terms like volatility, standard deviation, variance, or other related concepts, and in the rare cases where these terms appear, they are always in entirely unrelated contexts, not referring to returns or price changes. These papers also do not correctly compute single period returns either, as described in detail in Hartzmark and Solomon (2026). But even when considering other performance measures, standard deviation as a measure of risk was entirely absent.

Fisher (1922) computed various different index methods for stock prices. His "ideal index" ends up being similar to value-weighting, but uses the average of market cap at both the beginning and end of the return period. While he computes a cross-sectional standard deviation of index price levels across different types of index, he does not compute a time-series standard deviation for any of them. This is consistent with the general focus on long-horizon outcomes, rather than single-period outcomes.

Smith (1924) compares the long-run performance of stocks and bonds, taking starting and ending period prices, adding in cumulative dividends (not reinvested), and comparing this to total bond coupons. He only computes a single outcome, and no intermediate outcomes (which are a precondition to conceiving of returns in any form, let alone their standard deviation)

Jackson (1928) is arguably the first paper to compute some version of an equal-weighted portfolio return. But while he obliquely notes that

"the common stocks swing far more wildly than preferred stocks for the same class"

He never quantifies this concept. Despite computing gross returns for an index, he never computes a standard deviation nor discusses the concept.

Cowles (1938) computes a value-weighted index level. He too does not compute a standard deviation, nor use any terms relating to it.

A modern reader might be apt to wonder if this is simply due to an incomplete search, and that there may have been dozens of other papers doing similar exercises that we have failed to uncover. The principal reason this seems unlikely is that the amount of work required to compute a value-weighted return, in data collection and manual calculations, was enormous, and so the number of people attempting it was relatively small. As Cowles (1938) describes:

"*Some idea of the magnitude of the project may be indicated by the fact that over 1,500,000 work sheet entries were made, requiring 25,000 computer hours"*

"Computer" here in 1938 means "a person who computes", not the modern version of a mechanical or electronic device. As a matter of pure historical fact, none of the papers prior to Markowitz (1952) where one might expect to see a standard deviation of returns computed ever do so, and generally do not discuss the concept at all, even in vague or imprecise terms.

One way to see how non-obvious the standard deviation of returns measure is that it is not computed in many empirical papers even *after* Markowitz (1952). Dirks (1958) computes an "overall yield" for portfolios of stocks, albeit one that uses inconsistent dividend reinvestment assumptions, and still does not compute a standard deviation. This was not just a function of people not having a clear idea of what a return was. Even the early CRSP papers, which set many of the standards still used today on how to measure returns and deal with corporate events, *still* did not compute a standard deviation. Fisher and Lorie (1964), which introduced the CRSP dataset

and computed the performance of a value-weighted market index as its main empirical contribution, does not compute a standard deviation of single-period returns, nor describe the concept anywhere. Most surprising of all, Fisher (1965) was explicitly devoted to measuring the distribution of single-stock outcomes at different holding periods as a way of considering risk, and even that paper does not compute a standard deviation of returns anywhere, nor does it discuss the concept. As we will discuss later, Fisher (1965) is in fact strong evidence for the durability of the conception of risk as being the chance of losing money at a long horizon.

5.2 Lintner (1965) is the first published market standard deviation

While the concept of standard deviation of returns as a risk measure was developed in Markowitz (1952), it did not seem to occur to people to apply it to the market portfolio until the development of the CAPM. The earliest example of a computed standard deviation for any kind of market performance measure that we were able to locate was in Lintner (1965), one of the seminal papers developing the CAPM. The two other papers most famous for the CAPM, Sharpe (1964) and Mossin (1966), have no data at all, not even numerical examples or indications of market returns. Lintner (1965) was an exception, containing an empirical section that computed a standard deviation of the S&P Price Index (which is not actually a return), and compared this with individual stock returns (failing to note the difference of units). This market standard deviation result is only contained in a footnote, however, and is not a major focus of the paper

After Lintner (1965), stock-level and market-level standard deviations were computed with some regularity, though Like Lintner (1965) they were mostly not the main point of the paper. Grubel (1968) computes market standard deviations over a five year period for estimating the gains to international diversification under portfolio theory. Smith (1969) computes the standard deviations of various market indices for a version of portfolio theory that uses index correlations to generate covariances. Lintner (1970) computes market means and standard deviations to compute the market price of risk under a CAPM, though only using quite imprecise measures.

The first paper to consider the mean and standard deviation of market returns as a principal focus was Friend and Blume (1975). This was in the context of a similar risk/return quantification exercise to Mehra and Prescott (1985), but using mean and standard deviation

outcomes instead of consumption. They describe the relative lack of interest in market return properties up to that point in the very first sentence of their paper:

> "*A striking characteristic of recent research on capital asset pricing is the paucity of empirical work on the determinants of the market price of risk in contrast to the abundance of work on the interrelationships of the risk premiums among different risky assets.*"

Most of the research focus up to this point had been on the cross-sectional implications of the CAPM, such as tests of the relationship between market betas and expected returns in Fama and Macbeth (1973) and Black, Jensen and Scholes (1972). The focus on market risk-to-reward ratios only developed gradually. The CAPM model is often taught today by noting that the slope of the Capital Allocation Line is the Sharpe Ratio of the market, and this reflects the reward-to-risk ratio of the market itself. But this description is not contained in Sharpe (1964), and the Sharpe Ratio is only introduced in Sharpe (1966) for evaluating mutual fund performance, and is placed alongside (and deemed less useful than) the Treynor ratio of Expected Excess Return / Beta.

The conclusion of all the above is that, as a matter of historical fact, there was no conceptual discussion of the standard deviation of returns before Markowitz (1952), and no empirical quantification of it until Lintner (1965). We next consider why this was the case.

6. Alternative Conceptions of Risk – Probability of Long Horizon Loss, and Bond Analogies

A number of the early empirical papers that failed to compute a standard deviation of returns as a risk measure also primarily reported long horizon cumulative returns as their main object of interest (Cowles (1938), Fisher and Lorie (1964)). We argue that this is unlikely to be a coincidence. In particular, thinking of performance as a distribution of short period outcomes is a necessary step towards thinking about the standard deviation of that distribution, or even the arithmetic mean (which the empirical papers above also fail to compute).

In principle, one could conceive of a distribution of long-horizon returns, and still think of their standard deviation. Indeed, this is how most modern readers would conceptualize long horizon performance. But an idea like "the standard deviation of 30 year returns" is a very abstract concept if one were, say, Fisher and Lorie (1964), who only have a single 30 year period of data to examine. Mostly, one comes to this idea by taking standard deviations of short period

returns (which one *can* see in an intuitive way), and then extrapolating known statistical properties about how standard deviation changes with horizon under various assumptions about the distribution in question. If one has not started with this framework, it is quite intuitive to do what Fisher and Lorie (1964) do, and take the single long horizon outcome and treat it as a singular event, perhaps with some interest in sub-periods. We submit, however, that one is unlikely to think about the volatility of 30-year returns (which they could not even compute) if one has not first thought about the volatility of one-year or one-month returns (which they could compute, but which we can concretely observe that they failed to do).

If risk in this context is *not* the standard deviation of returns, then what might it be? We argue that risk instead meant "the chance of bad outcomes at long horizons" – in other words, "the probability of losing money". This kind of conception of counterfactuals is also very intuitive even without a full notion of return distributions and how they interact at multiple horizons. The probability of bad outcomes is an extremely old and intuitive concept – when Caesar crossed the Rubicon, he said to his men "*alea iacta est*", meaning "let the die be cast". It seems likely that "the probability of a bad eventual outcome" would not have been far from his mind. By contrast, the earliest form of the normal distribution was not studied until de Moivre (1738), Gauss (1809) and Laplace (1774). Even in those versions, Gauss and Laplace parameterized the distribution with a precision parameter, h, which operated like the inverse of variance. The specific modern statistical version of standard deviation dates only to Pearson (1894).

Second, the idea of risk as "the chance of eventually losing money" is also very intuitive under another framework, namely bonds. "Bond default" and its associated probability is still today the chief risk metric under which bonds are evaluated. While bond default could also be analogized to corporate bankruptcy, there are many ways for equity holders to experience adverse events short of bankruptcy. If the metaphor is to bonds, a key question is what quantity will take the place of the bond's face value for defining what counts as default?

This can be thought of from an investor's perspective, and from an accounting or corporate finance perspective. From an investor's perspective, the most obvious version is losses relative to one's initial investment, or principal. In this conception, loss is continuous, like in bond default, and may correspond to a complete default (e.g. the equity holder being wiped out in the

case of bankruptcy) or a partial recovery (losing some money relative to one's initial stake, but not the whole amount). From an accounting or corporate finance perspective, one needs some notional amount of money that the stock corresponds to that is the same across all investors.

An accounting analogy to bonds the best explanation for the frequent early discussion of "par value" for a stock – the notional amount of paid-in capital that investors may or may not have completely contributed on the initial subscription. Indeed, the entire focus on stock par-value is baffling without this perspective. Economically, par value has no real ongoing importance, as equity holders own the firm's current assets, whatever they are worth. The historical initial investment has no equivalent future relevance like the bond's face value, which is a concrete obligation to pay back in the future. But if one is searching for common, accounting-type value to attach to all shares that functions vaguely like a bond's face value, this can serve the purpose.

Par value was most often discussed in the context of measuring a stock's performance in terms of dividend yields or income yields. While sometimes these were quoted in the more economically useful sense of scaling by price, such as in Scott (1910) and Robinson (1930), often they were quoted as a percentage of the stock's par value, such as in Mitchell (1910) and Matherly (1923). Since bond coupons are typically quoted relative to face value, the search for some stock-level equivalent caused people to quote dividend yields that way too, even though the calculation doesn't reflect any meaningful economic reality.

However, we are principally interested in the alternative version – risk as the chances of losing money on one's investment at a long horizon. We shall find evidence for such perspectives, and that part of the rationale is from comparisons with bonds.

7. Risk as the chances of losing money at long horizons

7.1 Graham and Dodd (1934) and probability of loss

Graham and Dodd (1934) marks an interesting transition between the early categorical rejection of stocks as "not an investment", and the later view of stock risk as the chances of losing money over a long horizon. The book shows aspects of both kinds of thinking. Graham and Dodd (1934) was famous for popularizing, if not inventing, the idea of value investing. It is cited as a major inspiration by Warren Buffett, and features considerably in Morrison (1949)'s summary of the history of investment thought.

Graham and Dodd (1934) is very difficult to reconcile with the Markowitz claim that risk generally meant variance. Despite being an enormous work, even in the sixth edition, it has no mention of the word "volatility" (later authors writing preface sections use it, but not Graham and Dodd themselves). The word "variance" occurs only for ideas that are "at variance" with some facts. "Standard deviation" does not appear anywhere. This is neither an accident, nor due to the idea going by a different name.

Importantly, Graham and Dodd (1934) *maintained* the importance of categorical distinctions between investments and speculations, but they no longer drew these boundaries at the asset class level. Instead, they argued for categorization based on the chances of losing money at long horizons, but held that the categorization itself was nonetheless still important. Chapter 4 of the book is titled *"Distinctions Between Investment and Speculation*". They departed from previous authors by arguing that not *all* bonds should be considered as investments, and not *all* stocks should be considered speculations. They explicitly criticize Smith (1924) and the "investment theory" writers that followed for failing to maintain this distinction, and for arguing for an unconditional equity premium rather than a high returns only for value stocks:

> "*[T]two theoretical weaknesses that could and did result in untold mischief. The first of these defects was that they abolished the fundamental distinctions between investment and speculation. The second was that they ignored the price of a stock in determining whether or not it was a desirable purchase."*

The appeal of value investing was to find stocks that met the criteria of "investments":

> *"[W]e suggest the following definition of investment: as one in harmony with both the popular understanding of the term and the requirements of reasonable precision:*
>
> *An investment operation is one which, upon thorough analysis, promises safety of principal and a satisfactory return. Operations not meeting these requirements are speculative."*

They shortly follow up on this concept of what risk and safety mean:

> "*The safety sought in investment is not absolute or complete; the word means, rather, protection against loss under all normal or reasonably likely conditions or variations. A safe bond, for example, is one which could suffer default only under exceptional and highly*

improbable circumstances. Similarly, a safe stock is one which holds every prospect of being worth the price paid except under quite unlikely contingencies."

First, this passage represents perhaps the sharpest example of not only thinking of stock risk in terms of chances of losing money, but explicitly doing so by analogy to bond defaults. In other words, they are trying to find "bond-like" stocks that count as investments, and formalize exactly what it is one is seeking with a bond. As with much early work, on its own this is not necessarily a bad idea as a broad investment goal. The problem, rather comes from the components that are missing under the metaphor – most notably, intermediate price changes.

Second, this passage is also very important for helping us understand how horizon interacts with risk conception. It is worth pondering - for what definitions of terms could it possibly make sense to expect to find a stock *"which holds every prospect of being worth the price paid except under quite unlikely contingencies"?* Can someone look at stock prices, even just in a cursory sense, and hope to find an equity which "*holds every prospect*" of never declining in price, or never having a negative total return (the definition of "return" is not made precise), except under *"quite unlikely contingencies*"? If short horizon returns are being referred to, this seems either outright impossible or nonsensical. But if the concept being measured is long horizon price changes and long horizon cumulative returns, then it becomes at least more conceivable.

The conception of value investing is altogether alien to the Fama and French (1993) view. Graham and Dodd (1934) did not think that value stocks earned higher returns because they were *riskier*. They thought that they earned higher average returns, *and that made them safer. T*hese high expected returns would produce their definition of an investment, namely something that would not decline in price except under very unlikely contingencies. In Chapter 43, describing stocks selling below their liquidation value, the idea that value metrics can be associated with safety, which means low chances of losing money, is made explicit:

"Common stocks that (1) are selling below their liquid-asset value, (2) are apparently in no danger of dissipating these assets, and (3) have formerly shown a large earning power on the market price, may be said truthfully to constitute a class of investment bargains. They are indubitably worth considerably more than they are selling for, and there is a reasonably good chance that this greater worth will sooner or later reflect itself in the

market price. At their low price these bargain stocks actually enjoy a high degree of safety, meaning by safety a relatively small risk of loss of principal."

By conceiving of risk as the chance of losing money at a long horizon, risk is an amalgamated idea that contains both mean and standard deviation. It is very hard to reconcile with trading off means versus standard deviations of single-period asset returns.

7.4 Risk in Dirks (1958)

Risk as "long horizon chances of losing money" continued to be a popular even after Markowitz (1952). Dirks (1958), which also measured the returns of stock indices, is another example like Smith (1924), Jackson (1928a), and Cowles (1938), of a paper not greatly concerned with risk, but rather with average performance. The word "risk" appears twice in the article, however. In both cases, it indicates an understanding of risk as long-horizon underperformance.

"Is there a substantial risk that, in reaching for better-than-average returns, the eventual outcome may be less than average?"

and

" Some of those investors who have turned to dollar averaging through time, as a device for minimizing the risk of relative losses over the cycle, may well wonder whether the logic of their attitude does not point to dollar averaging with respect to industry also."

7.5 Risk in (Lawrence) Fisher (1965)

The strongest evidence for "risk as chances of long-horizon loss" even after Markowitz (1952) is Fisher (1965), the second CRSP paper. Unlike Dirks (1958), where we must infer views on risk from a small number of statements, the risk of stock returns is here the central focus.

All three early CRSP papers (Fisher and Lorie (1964), Fisher (1965) Fisher (1966)) focus on properties of newly-created market and stock returns, but do not compute a standard deviations of returns. For Fisher and Lorie (1964), the first on market returns, one might charitably assume it is because the authors were not *discussing* risk, but if they had, they would measure it as standard deviation. This view becomes untenable when seeing how risk is treated in Fisher (1965). The paper studies individual stock returns over every possible period of multi-year performance, and reports the geometric average returns. These geometric averages are just

described as "the rate of return", singular, for each combination of stock and time-period, consistent with the long-horizon, cumulative focus for performance. The conclusion of Fisher (1965) makes clear that i) "returns" are geometric average returns over long horizons, and ii) risk is considered as the discrete category of losing money over this horizon:

> "*The variability of rates of return is much greater for short-term investments than it is for long-term investments. Therefore, on the average the probability of a gain on a long-term investment was substantially greater than 78 per cent. … The results of this study do not conflict with the belief that there is a substantial chance of losing money on an investment in an individual common stock. There were, however, few long periods during the last forty years when the chances of loss were as great as the chances of gain; and the factors which have made investment in competing media less risky in recent years have also operated to make investment in common stocks less risky."*

These statements emphasize that *"probability of a gain on long term investments"* is the main conception of risk. While Fisher (1965) plots the distribution of geometric average returns, risks are primarily evaluated in probability-of-loss terms at different horizons. This is true both in the stated conclusions, and in the choice of numbers displayed. Table 3 reports the fraction of stocks at a gain at each horizon. Variability is considered only as the interquartile range of the geometric averages. The variability of single-period returns is not measured or discussed anywhere.

Finally, the first sentence above is especially telling – that "*the variability of rates of return is much greater for short-term investments than long-term investments"*. Superficially, Fisher (1965)'s discussion of the variability of returns might lead one to believe that he is following some kind of Markowitz (1952) conception of risk as standard deviation. This view has to contend with the fact that the only numbers displayed are averages, and the variability of the average, not the variability of the single period returns. More strikingly, his claimed result that variability is higher at short horizons than long ones *only* makes sense for average returns, not single-period returns. If stock returns are i.i.d., then the standard deviation of single-period returns does not increase or decrease on average depending on how many years' data are used.[3] If cumulative returns are

[3] That is to say, sample standard deviations themselves get less volatile as n increases, but they do not increase or decrease on average as the number of observations changes.

used, standard deviations of returns are *larger* at long horizons than short horizons. But if the average return is used, then the variance of these averages indeed decreases with the square root of the number of observations, according to the Central Limit Theorem (formally for arithmetic averages, but also for geometric averages). Hence the Fisher (1965) conception – "variability of returns" decreases at long horizons. There is no way to reconcile this claim with thinking of "variability of returns" as the standard deviation of single-period returns.

8. Likely Mistakes flowing from "Risk As Probability of Long-Horizon Loss" Framework

8.1 Confused or Entirely Absent Risk Return Tradeoff

While viewing risk as the probability of loss seems intuitive and reasonable, it is actually quite difficult to reconcile with modern asset pricing, and liable to produce confusion, if not outright mistakes. The first of these is described above is that higher expected returns both increase average performance *and* decrease risk. This is evident in the Graham and Dodd (1934) arguments about how value stocks also produce safety of principal. But it also speaks to a more basic question – is there a perceived risk/return tradeoff at all? This may seem almost impossible to imagine, but if a high enough mean produces both a greater reward, and a lower risk (probability of loss at long horizons), assets with high expected returns may simply be viewed as dominating choices. And indeed, we can find statements consistent with this. For instance, consider again the quote in Smith (1924) describing the inflation hypothesis he was testing

> "*While a diversity of common stocks has, without doubt, proved a more profitable investment than high-grade bonds in the period from 1897 to 1923, during which dollars were depreciating, yet with the upturn in the dollar, bonds may be relied upon to show better results than common stocks, as they did in the period from the close of the Civil War to 1896, during which the dollar was constantly increasing in purchasing power."*

In one period of time stocks will be *"more profitable"* than bonds, but in the other period of time, bonds will *"show better results"*. The parallel construction of these two clauses suggests they are essentially synonyms. There is not a sense of a higher return coming as compensation for higher risk. Higher returns just make an asset *better*.

Jackson (1928b) is another example. It was a follow-up paper to Jackson (1928a), focusing on how to construct cumulative return outcomes. When it comes to evaluating these returns, it has the same mindset as above, here in the context of common versus preferred stocks:

> *"In the first place, neither common nor preferred stocks have an overwhelming advantage. Taking the group as a whole, the results show an advantage in favor of the common stocks when the period is taken as one year. When several years are combined to form a longer period, the common stocks are quite generally the better."*

In other words, the *advantage* is simply which asset gives greater cumulative returns. There is no sense of trading off risk. Higher returns simply make an asset better.

8.2 Time Inconsistent Assessments of Risk and Asset Tradeoffs

The second major problem with "risk as the probability of loss" framework is that it is likely to produce time-inconsistent choices when applied across different horizons. If risk is considered as the "probability of losing money at a given horizon", when the horizon gets long enough, *the riskiness of stocks converges to zero*. This is because the probability of stocks losing to bonds also converges to zero. This does not actually make stocks safe, however. In particular, at any horizon, there is a decreasing probability of loss, but an increasing magnitude of those losses when they do occur. A standard risk-averse agent will still be worried about the utility of those states, even as the probability of encountering them diminishes. But if one views risk just as the probability of loss, one will not see this tradeoff. Risk will just decrease with horizon. We have already seen statements that note this relationship. For instance, Jackson (1928b)

> *"[T]he results show an advantage in favor of the common stocks when the period is taken as one year. When several years are combined to form a longer period, the common stocks are quite generally the better."*

And again in Fisher (1965)

> *"The variability of rates of return is much greater for short-term investments than it is for long-term investments."*

Given the metrics being computed, these statements themselves are not *mistakes* – they are accurate descriptions of the empirical patterns in the variables they are studying. However, this kind of mindset is very likely to produce mistakes when applied to risk/return tradeoffs at

different horizons. If equity risk is considered to decrease with horizon the equity premium puzzle becomes almost infinitely large at a long enough horizon. The problem is that the same sequence of bets is apt to appear wildly different if considered as a group, versus one at a time. This point is quite subtle, and was a focus of Samuelson (1963) around the same time:

> "*A few years ago, I offered some lunch colleagues to bet each $200 to $100 that the side of a coin they specified would not appear at the first toss. One distinguished scholar… gave the following answer: 'I won't bet because I would feel the $100 loss more than the $200 gain. But I'll take you on if you promise to let me make 100 such bets."*

The rest of the article was demonstrating that this statement was actually inconsistent not just with modern asset pricing, but with expected utility *in any form*:

> *"If you would always refuse to take favorable odds on a single toss, you must rationally refuse to participate in any (finite) sequence of such tosses."*

But despite his proof, the appeal of the fallacy – that as long as the probability of loss goes to zero with a large number of bets, the risk goes to zero – is quite strong. So much so, that in 1979, frustrated with continued examples of this mistake, Samuelson restated a variation of the same result in an article written only in words of one syllable (Samuelson (1979)):

> *"When you lose – and you sure can lose – with N large, you can lose real big. Q.E.D. …*
>
> *If it does not pay to do an act once, it will not pay to do it twice, thrice,…, or at all"*

Viewing risk in terms of probability of loss is likely to produce time-inconsistent decisions depending on which horizon is focused on. The fact that the original motivation for the mistake was Paul Sameulson's colleagues at MIT in the 1960s suggests that the mistake is not at all an obvious one. If it does not pay to choose stocks over bonds for one month or one year, and stock and bond returns are i.i.d., it does not pay to choose stocks over bonds for 30 years either.

The application to stock returns is complicated slightly by the fact that they are not generally i.i.d., certainly for individual stocks (see Jegadeesh (1990) for one-month reversals, Jegadeesh and Titman (1993) for 2-12 month momentum, DeBondt and Thaler (1985) for 3-5 year reversals, Heston and Sadka (2008) for calendar seasonality, and many others). These patterns are weaker for market returns, where the assumption of i.i.d. returns is more reasonable.

The point is less that stock market returns are i.i.d. and the Samuelson (1963) argument holds exactly (e.g. it is impossible under expected utility to like stocks at a ten-year horizon if you don't also like them at a one month horizon). But conceiving of risk as a horizon-dependent chance of losing money is likely to lead to confusion and actions inconsistent with expected utility, like Samuelson (1963)'s interlocutor – risky actions taken again and again are somehow believed to become safe. Any investor thinking in such terms is likely to have a fundamentally confused conception of risk and return from the perspective of a standard risk aversion.

9. Early Risk/Reward Tradeoff Focused Only on Cash Flows, Ignored Intermediate Price Outcomes

The articles described earlier that straightforwardly describe higher expected return assets as being better (Smith (1924), Jackson (1928b)) are perhaps the most striking examples of failing to conceive of a risk/reward tradeoff. But not *all* early authors failed to perceive a risk-reward tradeoff. A separate strand of early research argued for the broad concept of risk being related to expected returns, and in some cases even perceived that risk as relating to standard deviation. Crucially, however, these arguments only applied to the risk of future *cash flows.* These were nearly all theoretical works comparing the price today and future cash flows. They lacked any framework for thinking about future price changes, or the risk they involve. Empirical papers which attempted to measure price changes displayed more of the fundamentally confused intuition described earlier, where high expected return assets as just viewed as superior.

This focus on cash flow risks also can be understood in terms of bond metaphors. Because intermediate price changes will mean revert in the absence of default, holding period returns tend to be less often discussed relative to bond yields. An investment mindset that began with this conception was likely to similarly focus on yields on stocks, comparing price today and future cash flows. Without a framework for thinking about price changes, the role of prices tended to be downplayed or ignored, thus missing a crucial piece of the risk/return tradeoff. Indeed, this perspective will be particularly misleading given the finding in Shiller (1981) that price changes are much more volatile than dividends or earnings, and the largest part of risk will be missed.

9.1 Cash Flow Risk and Reward in Mill (1848)

While most of our focus is on 20th century sources, Mill (1848) presents an interesting general argument that higher risk should earn higher returns (although in contexts outside of traded equities). He discusses a farmer deciding whether to obtain land for a farming project:

> *"…he will probably be willing to expend capital on it (for an immediate return) in any manner which will afford him a surplus profit, however small, beyond the value of the risk, and the interest which he must pay for the capital if borrowed, or can get for it elsewhere if it is his own."*

Optimistically, Goetzmann and Ibbotson (2005) describe the passage above in terms of its relation to the equity risk premium, assessing it as follows:

"Mill thus separates profit into three parts: first, the interest that must be paid for the capital borrowed, determined in terms of alternative opportunity cost of money. This is equivalent to the riskless rate. The second component is the "value of the risk" associated with the investment. This is equivalent to the equity risk premium. Mill's third component is a surplus profit, no matter how small. In modern parlance, the "alpha" – a portion of compensation expected to be small in a competitive market."

But later in the passage, it becomes apparent that Mill is not thinking about a farmer *purchasing* the land, and exposing himself to capital appreciation or loss based on price changes. He is thinking about a farmer *leasing* the land:

"…even when the lease expires, the landlord may be unwilling to grant a necessary diminution of rent, and the farmer, rather than relinquish his occupation, or seek a farm elsewhere when all are occupied, may consent to go on paying too high a rent."

Indeed, for the farmer to obtain an "*immediate return"* suggests that this cannot be a case of the price of the land going up, which is apt to be either long term, or unobservable.

Perhaps more importantly, while Mill (1848) has a general sense of how risk is related to return, there is no attempt to quantify this risk, nor give guidance as to how one should assess it. Even at the intuitive level, it is applied only to cash flows, and not to price changes. Given the context of equity markets in 1848, it is not clear it is intended to apply to equities at all.

9.2 Cash Flow Risk and Reward in Fisher (1906)

Another early example of cash flow risk and reward is Fisher(1906). He is quite far-sighted in several respects regarding risk. He notes that there is a risk/reward tradeoff for the price that investors will be willing to pay for different assets. Most people will pay less than the expected value of a risky gamble, but that there are individual differences in attitudes to risk.

> *"But the actual price which one will pay is not necessarily the mathematical value of the chance . It may be higher ; it is usually lower . The gambler is usually a person who will pay more than the mathematical value of the chance . … The ordinary man is unwilling to pay even the full mathematical value of the chance . He is reluctant to assume any risks , and is , on the contrary , willing to make sacrifices in order to rid himself of them ."*

Fisher's conception of risk is quite different to modern ones, even when applied to cash flows. Risk is fundamentally subjective, and Fisher denies the possibility of objective risk measures:

> " *Chance is subjective. Although one man's estimate may be better than another's through superior knowledge, intuition, or experience, the best estimate is still only an estimate, not a certainty. In the actual world of events there is no uncertainty. Aside from human opinion, there is no such thing as chance."*

Fisher does not describe how to distinguish between quantity of risk, and the individual risk aversion. Both are just lumped into a single discounting measure call the coefficient of caution. The discussions are nearly all bond-centered. The main discussion is about the riskiness of future cash flows, and the effect of risk attached to changes in interest rates. In terms of estimating these risks, Fisher mostly provides simple numerical probabilities attached to different cash flow outcomes. He notes that the practical ability to apply this is probably limited:

> *"Usually, however, the chances involved are so indefinite that the reckoning is made only by rule of thumb. Any further attempt to apply the theory of probability would therefore outrun the exigencies of practice."*

Fisher is, however, foresighted in terms of forecasting a role for standard deviation at all. The above passage, disclaiming the current ability to measure cash flow volatility, contains a footnote:

> "*Nevertheless it is more than conceivable that the time may come when practical brokers will make use of probability computations in the same way that they now make use of bond tables."*

This links to a discussion in the Appendix where he references the work of Norton (1902), who was one of the first authors to use standard deviations in economic settings. Fisher (1906) describes the possibility of using standard deviations to describe the volatility of cash flows. In this sense, his work is very far-sighted. But the crucial limitation is that these are all standard deviations across different possible states at the same future point, all of which are difficult to quantify. There is no sense of the time-series standard deviation of realized cash flows (which could actually be measured), nor of the standard deviation of future price changes. This missing component helps explain how Fisher could later judge stocks and bonds to be of similar risk levels, and miss the principal dimension on which a modern reader considers stocks riskier than bonds, namely their differences in total return, and their difference in price volatility specifically.

As described in Hartzmark and Solomon (2026), Fisher also came close to modern definitions of returns in Fisher (1922), when he compared different index constructions to settle on his "ideal index" which is not quite a return. But this analysis too did not cause him to actually describe or conceptualize one-period price changes, and their associated volatility. Indeed, he did not discuss the volatility of prices at all, and so missed the largest aspect of riskiness of returns.

While Fisher is often discussed for his substantive contributions to understanding inflation, his contributions to asset pricing tend to be largely forgotten. He is most famous, perhaps unfairly, for his remark in 1929 that stocks appeared to have reached "a permanently high plateau". This ought not discredit the many ways his work has genuine novelty and insight (risk aversion, standard deviations, the role of inflation), but it highlights the weakness of relying strongly on informal, qualitative arguments for understanding of asset prices.

9.3 Cash Flow Risk and Reward in Williams (1938).

Williams (1938) provided a much fuller treatment of how to generate expected returns on stocks by comparing today's price with estimated future cash flows. Williams (1938) is referenced in Markowitz (1952) as an example of how one might compute expected returns. Markowitz cites pages 55-75, which is devoted to i) computing present values of stocks and bonds based on forecasts of dividends/coupons and their probabilities of occurring, and discounting them to the present, and ii) converting these to expected returns relative to current prices. Like Fisher (1906), Williams (1938) argues that expected but risky cash flows will be discounted by a risk premium:

"*The customary way to find the value of a risky security has always been to add a "premium for risk" to the pure interest rate, and then use the sum as the interest rate for discounting future receipts.*"

Like Fisher (1906), Williams (1938) also gives no guidance on how to quantify this risk, or what kind of discount it should command. Williams (1938) is also much more explicit than Fisher (1906) that he is not concerned with forecasting price changes themselves. Price is interesting to the extent that it implies information and beliefs about earnings. Williams begins the book (p3) by arguing that a long term investor need not care about price at all, and can focus solely on income:

"[T]he longer a buyer holds a stock or bond, the more important are the dividends or coupons while he owns it and the less important is the price when he sells it. In the extreme case where the security is held by the same family for generations, a practice by no means uncommon, the selling price in the end is a minor matter. For this reason we shall define an investor as a buyer interested in dividends, or coupons and principal, and a speculator as a buyer interested in the resale price. Thus the usual buyer is a hybrid, being partly investor and partly speculator."

But as he notes immediately prior:

"*But speculators as a class can profit only by trading with investors, to whom they can sell only for income; therefore in the end all prices depend on someone's estimate of future income.*"

Second, Williams (1938) is skeptical that price changes have any real information value at all, over and above the current orders that the firm is receiving (p7):

"*In the field of financial speculation more than anywhere else, people seem to believe in a "social mind," or "collective intelligence" – a mind above and apart from the minds of individual traders, and greater than the mind of any one of them in the range of its knowledge and sureness of its judgment. …*

"People say that, since speculative prices move first, business volumes second, and interest rates third, the reason why speculation precedes everything else must be that speculators as a class can foretell, even though speculators as individuals cannot, the course of business months ahead. …"

"[T]here does exist a business series, to be described below, that moves as soon as, or even sooner than stocks, and one which any novice can follow if he can only get access to it. Hence we must conclude that the speculative fraternity as a class has no greater ability to foresee business conditions than have well-informed traders as individuals. …"

"The business series referred to above, which moves even sooner than stocks, and which displays even longer forecasting power, is called "Orders Received," or "Bookings." It leads all other series in the business cycle."

The evidence for this claim, which a modern reader is likely to be skeptical of, is a single graph of a stock price, with no formal tests at all. Profiting off price changes is described as essentially a zero-sum game, with prices as likely to become more incorrect over time as more correct (p33):

"*To gain by speculation, a speculator must be able to foresee price changes. Since price changes coincide with changes of marginal opinion, he must in the last analysis be able to foresee* changes in opinion. *Successful speculation consists in just this. It requires no knowledge of intrinsic value as such, but only what people are going to* believe *intrinsic value to be. Now opinion, when it changes, need not change for the right; it may change for the wrong, and the probability of a change for the wrong is about as great as of a change for the right."*

The conception of expected returns in Williams (1938), and referenced in Markowitz (1952), is one in which the price change component plays a minor, speculative role of little interest to long term investors. Despite the idea of applying a discount for risk to future cash flows based on their expected volatility, none of the authors made much headway on this, because the Fisher (1906) observation was still largely true in 1938 – it was difficult to specify in any precise way what the distribution of future cash flows would be, and thus difficult to describe its standard deviation. The practical use of standard deviation only came later in time series settings when applied to returns, which (unlike subjective cash flow expectations) can be concretely observed.

10. Frequency of Risk-Related Words in New York Times and Wall Street Journal

Most of this paper has focused on language and concepts in books and academic articles. This is both for the considerable information contained in the citations they use, and because

they tend to have extended discussions of specific finance concepts in ways that are especially illuminating for understanding the mindset involved. However, we next confirm that the broad trends we described above are mirrored in discussions in newspapers. For this purpose, we rely on the New York Times Financial Markets column since 1905, and the Wall Street Journal "Abreast of the Market" column since 1926, used in Garcia (2014).

We began with a short list of risk-related words, and asked Claude Code to analyze the articles for these words, and suggest other related terms and synonyms that appear which we had not thought of. From this, we generated a set of search terms, which we classify into broad categories that we think capture related underlying concepts. These are:

1. Pure Psychology = {Fear, Jitter*, Panic, Turmoil}
2. Price-Related & Emotionally Laden = {Crash, Plunge, Whipsaw, Meltdown}
3. Price-Related & Neutral = {Downturn, Sell-off, Correction}
4. Economic Interpretations = {Bubble, Crisis, Contagion}
5. Risk-Related & Neutral = {Fluctuat*, Instability, Uncertainty, Volatil*, Standard Deviation, Turbulence, Risk}

Because our interest is in long-horizon relative frequency of these ideas, we compute the fraction that each word or category of words comprises out of all the above risk-related words, over a rolling ten-year horizon across both newspapers (so the earliest 10 year window ends in 1914, and the last ends in 2007).

In Figure 1, we plot the fraction of words in each of the broad categories above. In terms of broad categories, at the start of the sample the two largest categories were "Risk, Neutral" (49.6% of all risk words) and "Pure Psychology" (41.5% of risk words). The other categories were all small - 3.3% for neutral Price Movement descriptions, 3.3% for economic interpretation terms, and 2.3% for emotionally laded price movement descriptions. By the end of the sample, the last 3 categories had all grown at the expense of the first two. In 2007, neutral risk terms 33.3%, pure psychology was 22.2%, neutral price descriptions were 18.1%, emotionally laden price descriptions were 16.6%, and economic interpretations were 9.7%. The large increase in descriptions of prices began in the 1950s and 1960s, coinciding with the S&P 500 beginning in 1957.

In Figure 2, we plot the frequency of words within each category above. Panel A examines words within pure psychology. At the start of the sample, "Fear" was 52.8% of psychology terms, and "Panic" was 46.3%. Over time, however, "Fear" maintained its overall popularity (being 74.2% of the category in 2007), while "Panic" declined in usage considerably, being only 6.7% of the category by the end. In overall prevalence, "Fear" and "Panic" were 22% and 19% of all risk words (ranks 2 and 4). By the end of the sample, "Fear" was still popular, being the top ranked word at 16% in 2007. "Panic" had fallen to 14th rank and 1.5% of words. Even within terms that capture only aspects of psychology, the baseline understanding shifted from Panic, which is generally irrational, to Fear, which can be rational or irrational depending on circumstance. The early view that market declines were simply panic fits in with the generally low focus on stock prices as sources of important information.

Figure 2 Panel B shows the evolution of terms within the category of neutral risk words. At the start of the sample, the two main neutral risk words were essentially qualitative in nature – "Uncertainty" (47.0% of category) and "Fluctuat*" (41.8% of category). These words were also important overall. In 1915, "Uncertainty" and "Fluctuat*" were ranked first and third among all risk-related, at 23% and 21% of risk words. By the end of the sample, uncertainty was only ranked 6th (6% of risk words), while "Fluctuat*" was down to 15th, and less than 1% of risk words. Even within the category, "Uncertainty" was only 17.4% of words in 2007, while "Fluctuat*" was only 2.7% of the category.

The two terms that grew to replace them were "Volatil*" and "Risk". "Volatil*" grew from only 0.7% of the category in 1915, to 31.4% of the category in 2007. Much of this growth happened in the 1950s and 1960s, consistent with the shift towards risk as standard deviation in Markowitz (1952) at the same time. "Risk" grew from 9.8% of the category in 1915 to 44.8% of the category. Meanwhile, the graph shows the usage of "standard deviation", but this is so infrequent as to not actually be visible.

Conceptually, this shift represented a change from a mostly subjective awareness of price changes and economic outcomes, to terms conceptually closer to modern understandings of risk and standard deviation (volatility, when annualized). The large increase in focus on neutral descriptions of price changes included words like "Correction" and "Sell-Off", shown in Panel C.

"Sell-Off" peaked in 1963 as the top-ranked risk word, at 17% of risk words. "Correction" peaked in 1964 as the second-ranked risk word, at 13.

In terms of emotionally laden descriptions of prices (Panel D), "Crash" and "Plunge" alternative in importance, with "Plunge" becoming more important by the end of the sample. In terms of economic interpretations (Panel E), "Crisis" was always the largest term, with "Bubble" rising in importance towards the end of the sample. Finally, Figure 3 shows the three most frequently used words for each rolling 10 year period. The overall pattern in word use mirrors the trends described earlier – an initial focus that was subjective, or centered around descriptions of investor emotions, rather than actual price movements. The rise in the 1950s and 1960s saw a change in language both towards terms that explicitly referred to recent price movements, and towards language about risk that mapped closer to the statistical measures of risk and volatility that grew in popularity after Markowitz (1952).

11. Aggregate Consumption Risk Was Not Considered Until Lucas (1978)

Finally, while most of our discussion has been of risk as the standard deviation of market returns, the risk measure that Mehra and Prescott (1985) consider is consumption covariances. This last development occurred very late. Even Friend and Blume (1975), which studied the risk/reward tradeoff in the market, did so in Sharpe ratio terms, not consumption covariances. This may seem like a small distinction, but it is quantitatively important – the actual calibration of risk aversion to mean and standard deviation of market returns in Friend and Blume (1975) produces numbers that Mehra and Prescott (1985) judge to be somewhat sensible, and not an obvious puzzle (as opposed to the quantification that comes from a consumption focus).

The reason consumption was not a focus at this stage was that the CAPM in its original form had investors put all their wealth in financial assets for a single period, so the volatility of those assets *was* the volatility of their wealth and their consumption, and thus the driver of market risk. In the intertemporal version of the CAPM in Merton (1973), in a partial equilibrium setting, risk includes not just the market portfolio, but changes in the economic opportunity set arising due to changes in economic state variables (left unspecified in Merton (1973), although changes in interest rates are given as an example)

The idea that the riskiness of financial assets comes from their correlation with aggregate consumption growth comes from the pioneering work of Lucas (1978). The modern understanding of the intertemporal CAPM is that it resolves in general equilibrium to a single aggregate consumption risk variable. But this understanding was only developed in Breeden (1979), and before this it was not clear that if the partial equilibrium setup in Merton (1973) is solved in general equilibrium with market clearing, the multiple economic state variables collapse to aggregate consumption as the only source of risk.

Like for stock market volatility, empirical interest in the correlation between stock returns and aggregate consumption growth only came after theoretical models on the subject. The first empirical examination was in Grossman and Shiller (1981). Earlier papers on correlations between stock returns and economic variables did not consider consumption growth to be important. For instance, Smith (1969) examines correlations with Gross National Product, CPI, Industrial Production, the Business Week Index, and the Money Supply, but not Consumption Growth.

Empirical analysis of the consumption-based model using the newly-developed generalized method of moments was also done in Hansen and Singleton (1982). The absence of this technique prior to this time also helps partly explain the relatively imprecise earlier calibration in Friend and Blume (1975). In other words, the theoretical understanding of the importance of consumption only came about 7 years before the actual posing of the equity premium puzzle. The first empirical measures of the correlation between equity returns and consumption only came out 4 years before the equity premium puzzle.

12. Mehra and Prescott (1985) and the "Last to Know" Assumption

Mehra and Prescott (1985) themselves consider whether equity returns and the risk-free rate from 1889-1978 can be explained by an equilibrium class of consumption-based models. Having found that they cannot, they argue in the introduction:

> "*Our conclusion is that most likely some equilibrium model with a friction will be the one that successfully accounts for the large equity premium.*"

The rational expectations assumption in Mehra and Prescott (1985) presumes that the marginal investor was behaving consistent with the model from the start of the period, in terms of having the necessary data, knowing the relevant parameters, framing the problem correctly, etc. Our

analysis converges on an entirely different answer –Mehra and Prescott (1985) were probably the *first* people to have thought about the equity premium in the terms that they did, and quantify whether it made sense. Previous generations of investors had gotten the answer wrong, because they lacked the basic understanding and data needed to even frame the problem correctly.

It is worth pondering what level of insight and talent was needed to make not just the final step in the intuition, but even intermediate steps. Many of the important intermediate papers were Nobel Prize-level insights - Markowitz, Sharpe, Lucas, Shiller, Hansen and Prescott. The full understanding involved contributions from all of them. It would seem unlikely, even before examining the empirical evidence, that the typical man in the street in decades prior had a level of understanding equal or superior to those authors during the stage that they did their work. In one view, the entire historical analysis we have carried out seems almost superfluous. But it is not superfluous. The framing of the equity premium puzzle treats it as entirely serious – did people decades prior act already in accordance with the understanding of these Nobel Prize winners? The answer, it appears, is no.

We know, trivially, that academics didn't already know about the puzzle, because old and well-understood facts rarely generate 9000-odd citations like Mehra and Prescott (1985) did. The more subtle question is whether investors in markets had a superior understanding. This is not actually obvious – perhaps real market participants were better informed and better incentivized to get the answer correct than academics at the time. However, we already have large sample evidence already on the question of "do academic publications in finance generally come as a surprise to market participants?". McLean and Pontiff (2016) study 97 return anomalies, and show how the returns to the strategies decline after publication. Importantly, they distinguish data mining from the effects of publication by comparing the period after the original sample ends, but before the paper was published. They estimate a 32% decline in returns specifically due to publication alone. Indeed, they argue credibly that this is actually a lower bound on the effects of publication, because some anomalies were publicly disclosed in working paper form even before the publication date, so the earlier decline in returns can also be due to publication.

None of the returns anomalies that McLean and Pontiff (2016) study speak directly to knowledge of the equity premium specifically. But they show quite persuasively that there are

strong reasons to think that academic research may in general *not* be well appreciated by investors, even for return anomalies where the financial incentives for investors to discover and trade on them are enormous. This adds further to the defensibility of the conclusion that investors simply didn't know the things Mehra and Prescott (1985) were testing.

12. Conclusion

The historical perspective we document in our study has several important implications. First, it suggests a cautionary note for tests of asset pricing models that attempt to increase sample sizes by collecting and analyzing data further and further back in time. The further back one goes, the more likely it is that assumptions that people made about financial markets are likely to be radically different. The sources needed to understand this in previous centuries are likely to be more difficult to find than academic articles from the early 20th century (which are largely electronically indexed and searchable). A careful attention to which assumptions people had at each time, and how they might apply to the phenomenon under study, is important.

Second, our study also suggests new possible models to quantify and test regarding their effect on the equity premium. How do returns work if some fraction of investors have a completely categorical aversion to stocks? Presumably the investors *trading* do not have such an objection, but the absence of other investors may matter in the presence of price pressure (Gabaix and Koijen 2021, Hartzmark and Solmon 2025). How does the equity premium work if some fraction of investors, or all investors, view risk as being about long horizon chances of losing money? Do these respective models better fit the data in periods where these views were more common? We have mostly focused on documenting and discussing the relevant changes in thought, and the full working out of their implications is a source of considerable potential.

Third, are the thought processes that we see on display in historical attitudes to equities also present in other modern asset markets? The fact that consumption covariances were not viewed as important until Lucas (1978) is potentially related to the fact that investors overall do not seem to think about them even now (Chinco, Hartzmark and Sussman (2022)). Do the historical risk attitudes we describe also apply today? They have considerable potential for explaining contemporary choices in cryptocurrency. The possibility that many investors apply categorical objections similar to the "not even an investment" category may explain the puzzling

reluctance to both take long *or* short positions described in Duchin, Solomon, Tu and Wang (2025). Meanwhile, the fact that cryptocurrency enthusiasts tend to have unusually *large* allocations, despite cryptocurrency's enormous volatility, may be partially explained by investors having risk assessments relating to the chances of losing money at a long horizon relative to equities (in terms of which cryptocurrencies may appear safer due to very high mean returns).

Finally, we also argue for the broader point underlying our analysis – a close reading of historical articles is a useful and underappreciated tool for helping distinguish between different models, especially in historical contexts. This has some conceptual overlap with surveys, which for a long time were treated as lacking any credibility, but instead have enabled a much sharper understanding of expectations formation processes and resulted in important models based on them (Shleifer and Greenwood (2013), Bordalo, Gennaioli and Shleifer (2018)). Historical articles have weaknesses for not knowing how representative individual authors views were. But these can be partially overcome by examining multiple sources, and like expectations data, can reveal insights that are difficult to obtain from other methods.

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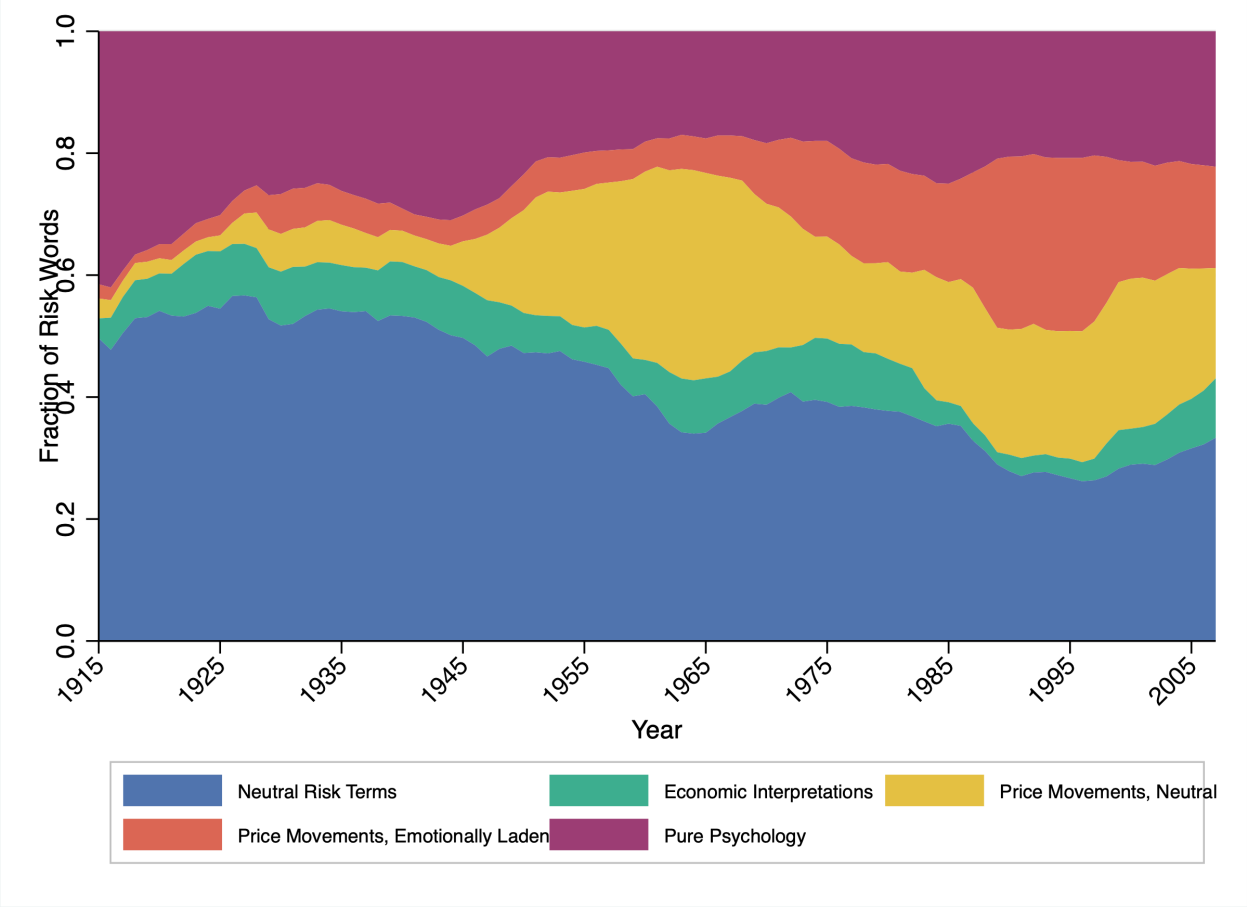
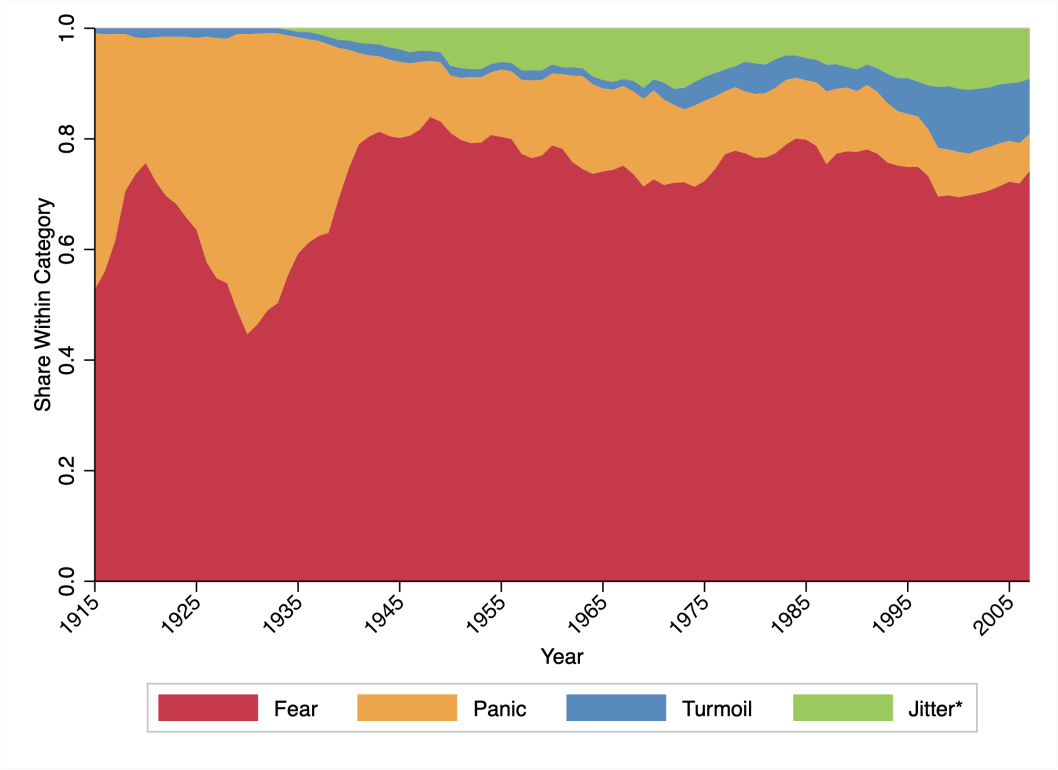
Figure 1 – Frequency of Risk-Word Categories in New York Times and Wall Street Journal



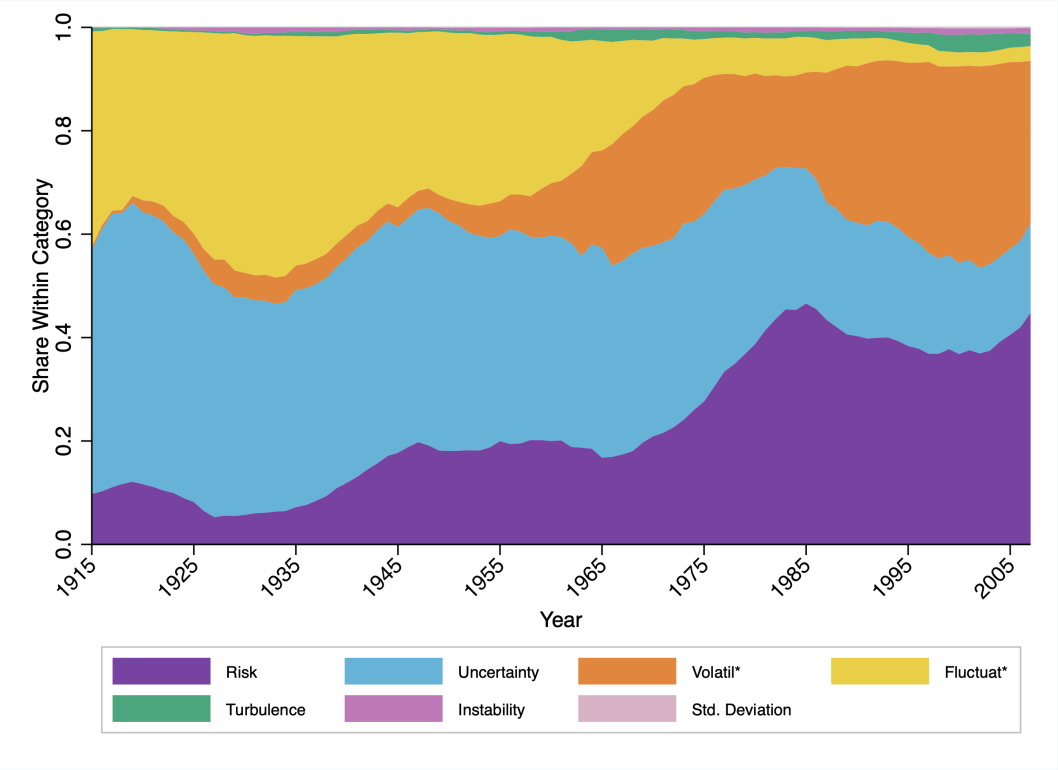
This Figure plots the fraction of all risk-related words across different categories of risk-terms appearing in the Wall Street Journal "Abreast of the Market" or New York Times Financial Markets column, between 1905 and 2007. Each plotted year takes the previous ten years' worth of articles across both papers, and computes the category total fraction of all risk-related words (i.e. all words in any category). The categories and words corresponding to them are: "Pure Psychology" = {Fear, Jitter*, Panic, Turmoil}, "Price-Related & Emotionally Laden" = {Crash, Plunge, Whipsaw, Meltdown}, "Price-Related & Neutral" = {Downturn, Sell-off, Correction}, "Economic Interpretations" = {Bubble, Crisis, Contagion}, "Risk-Related & Neutral" = {Fluctuat*, Instability, Uncertainty, Volatil*, Standard Deviation, Turbulence, Risk}

Figure 2 – Frequency of Risk-Related Words Within Each Category in NYT and WSJ

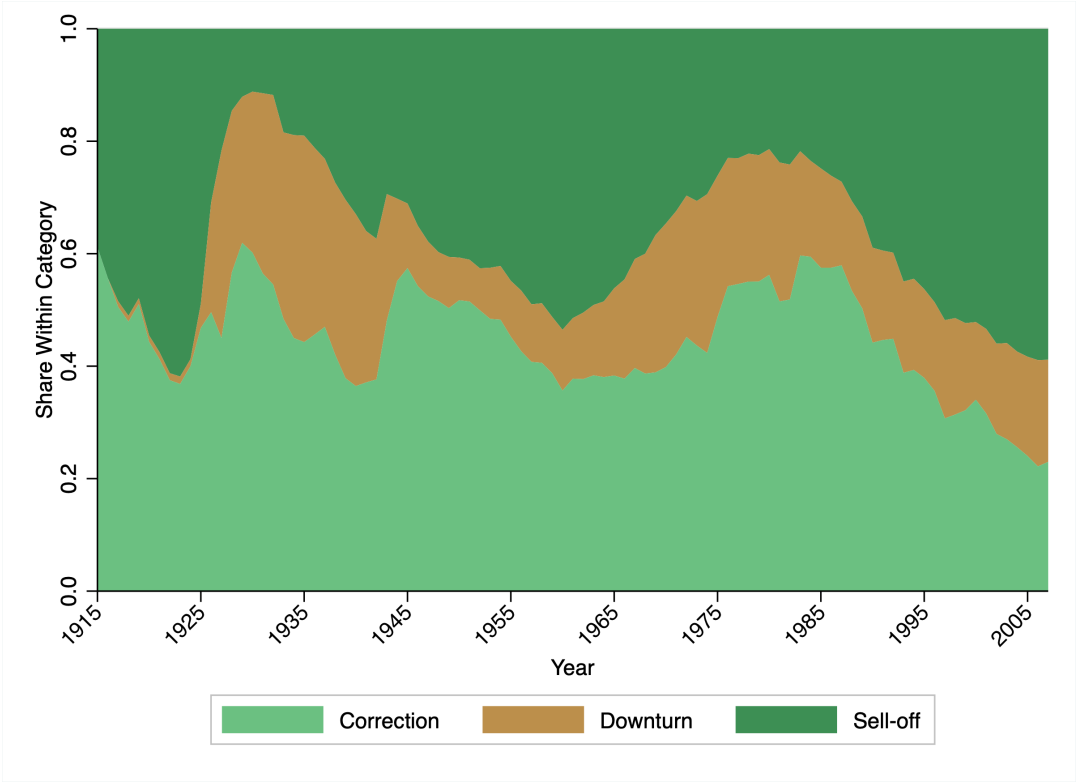
Panel A – Pure Psychology



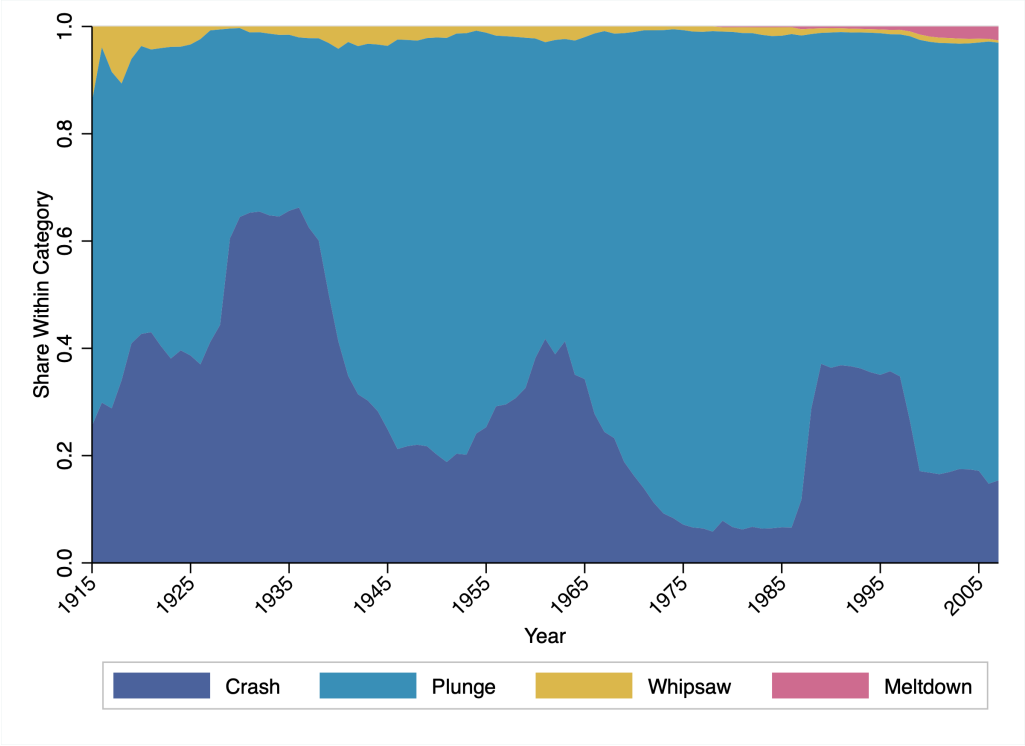
Panel B – Neutral Risk Terms



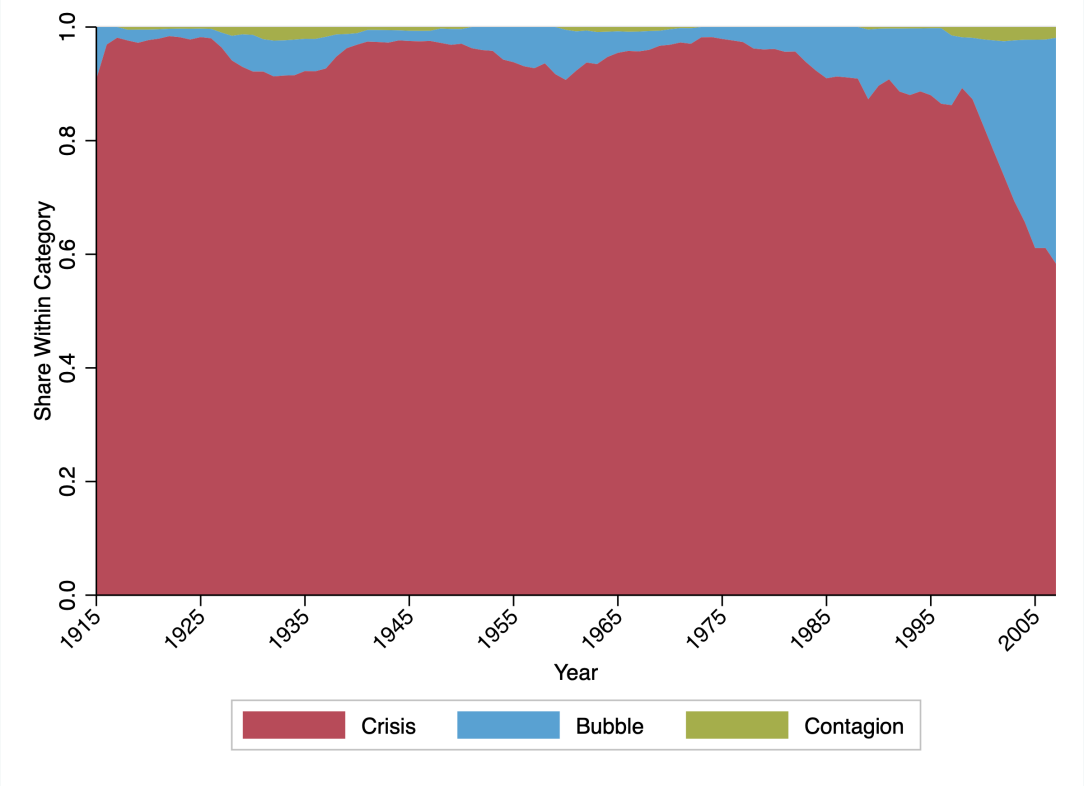
Panel C – Neutral Descriptions of Price Changes



Panel D – Emotionally Laden Descriptions of Price Changes

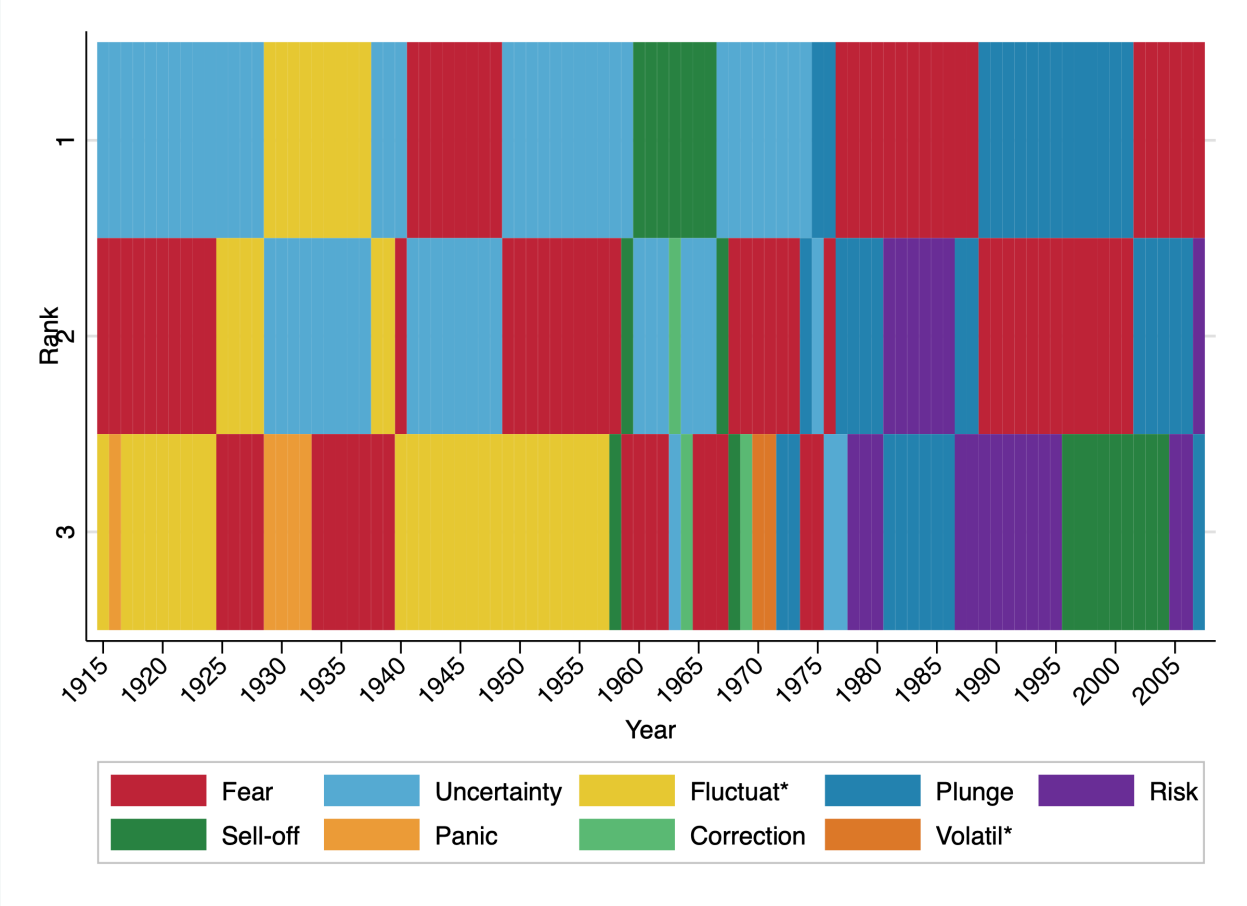


Panel E – Economic Interpretations



This Figure plots the fraction of all risk-related words within each different categories of risk-terms appearing in the Wall Street Journal "Abreast of the Market" or New York Times Financial Markets column, between 1905 and 2007. Each plotted year takes the previous ten years' worth of articles across both papers, and computes the category total fraction of all risk-related words (i.e. all words in any category). The categories and words corresponding to them are: "Pure Psychology" = {Fear, Jitter*, Panic, Turmoil} (Panel A), "Risk-Related & Neutral" = {Fluctuat*, Instability, Uncertainty, Volatil*, Standard Deviation, Turbulence, Risk} (Panel B), "Price-Related & Neutral" = {Downturn, Sell-off, Correction} (Panel C), "Price-Related & Emotionally Laden" = {Crash, Plunge, Whipsaw, Meltdown} (Panel D) and "Economic Interpretations" = {Bubble, Crisis, Contagion} (Panel E).

Figure 3 – Most Popular Risk-Related Words By Year



This Figure plots the three most common risk-related words appearing in the Wall Street Journal "Abreast of the Market" or New York Times Financial Markets column, between 1905 and 2007 (with each year representing ranks over the previous rolling 10 years). Rank 1 is the most common word, and rank 3 is the third most common.