Impact of Medicare Advantage Supplemental Benefit Expansion on Startup Funding

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Abstract

In 2018, the Center for Medicare and Medicaid Services (CMS) announced that they would expand the supplemental benefits that can be included in Medicare Advantage (MA) plans. The goal was to encourage insurers to innovate and test new benefit offerings that could improve health outcomes and reduce healthcare spending. A key player in this transformation is the MA vendor that provides supplemental benefit offerings to insurance plans, but this market is rather underdeveloped. To assess the implementation of this supplemental benefit expansion, this study examines the flow of funding into the emerging market of MA vendors. This paper uses a longitudinal approach and Crunchbase data on funding for 79,004 firms from 2014 to 2018 to determine whether there is a significant jump in funding toward MA vendors with supplemental benefit services following the policy change. The results show that both the average amount of funding per deal and the number of deals a MA vendor firm receives significantly increased following the expansion when compared with all other firms. This suggests that the policy may have been successful in promoting the development of the MA vendors market and the innovation of benefit offerings as more funding goes towards these companies.

JEL classification: I1; I11; I18

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1. Introduction

In the past decade, the percentage of the eligible Medicare population enrolled in Medicare Advantage (MA) plans has more than doubled from 19% to 48% and is projected to continue to grow (Freed et al., 2022). MA plans are private health plans that offer supplemental benefits in addition to traditional Medicare coverage, posing as an attractive alternative to traditional Medicare. As the MA market expands, it is prime for innovation. In April 2018, the Centers for Medicare and Medicaid Services (CMS) announced that supplemental benefits would be expanded to include adult day health services, home-based palliative care, in-home support services, and other offerings (Centers for Medicare and Medicaid Services, 2018). Then the CMS announced in April 2019 that supplemental benefits would be further expanded for chronically ill enrollees to include meals (beyond a limited basis), food and produce, transportation for non-medical needs, and other services (Centers for Medicare and Medicaid Services, 2019). The motivation behind the expansion was to allow MA plans to innovate and test new benefits that could potentially improve health outcomes for vulnerable beneficiaries and bring cost savings to Medicare (Rizer & Benzing, 2022).

With the adoption of these supplemental benefits increasing, the industry of supplemental vendors grows in turn (Gondi & Gebremedhin, 2021). As an emerging market segment, the vendor landscape is fragmented and inconsistent, with many vendors unable to meet the rigorous standards of insurers (Gondi & Gebremedhin, 2021). Thus, making it prime for disruption. Startup vendors are taking advantage of this opportunity and targeting MA plans, looking to facilitate the adoption of their supplemental benefits services (Gondi & Gebremedhin, 2021). This expansion unlocked the untapped customer base of MA plans and created a positive demand effect in the market for supplemental benefit services. Subsequently, theory suggests that the expected profits of these startup companies would

increase, and they would perform better in fundraising as investors are more confident in their business model.

This study explores how this policy change impacted the funding of startup companies that serve or aim to serve as vendors of supplemental benefits for MA plans. This could be a proxy measure for the effectiveness of these startups in facilitating insurer adoption of the additional supplemental benefits because adoption is directly linked to the success of these businesses. Thus, examining the flow of dollars toward MA vendor startups can be an indication of how effective this policy change is in promoting implementation and eventually achieving its goals.

2. Literature Review

Current literature on the recent Medicare Advantage supplemental benefit expansion has focused primarily on the insurer and enrollee perspective, with emphasis on insurer adoption and enrollee access (Crook et al., 2021; Meyers et al., 2020; Rowen et al., 2022). There have yet to be any major reports on the effect of these new supplemental benefits on health outcomes. A key player in this industry that has been relatively overlooked in the literature is the vendor for these expanded supplemental benefits. The goal of this policy change was to encourage innovation in benefit offerings for Medicare enrollees, and vendors are an innovation vehicle for insurers (Rizer & Benzing, 2022; Gondi & Gebremedhin, 2021). Government strategies to spur healthcare innovation can be categorized into three major categories: gatekeeping market entry, changing the standards of payment, and funding projects with high potential. The second approach is a policy-driven demand shock and is thus most analogous to this expansion of supplemental benefits that increase insurer demand for MA vendor services. Although the payment

standard changes are the most comparable, I analyze all three strategies for a comprehensive picture of policy impacts on innovation.

Given that healthcare is a highly regulated industry, changes in exclusivity and payment criteria can directly impact and shape markets. There is some literature on healthcare innovation and the role that public policy plays in that dynamic (Clemens & Rogers, 2020; Eisenberg, 2006; Finkelstein, 2004; Xue & Ouellette, 2020). In the market for pharmaceuticals, the FDA's role as a regulator has enabled it to structure the incentives for innovation (Eisenberg, 2006). By providing protection against generic entry and reviewing clinical trial data on drugs, the FDA encourages companies to develop drugs with high R&D costs and to invest in the safety and efficacy of their drugs (Eisenberg, 2006). The FDA's function in the pharmaceutical industry is evidence of how gatekeeping and other regulatory mandates can impact innovation. Examining the vaccine industry, a study found that public health policies that increase the vaccination rate against specific diseases, subsequently increasing the expected revenue of vaccine development for those diseases, are associated with a statistically significant increase in the number of new vaccine clinical trials for those diseases (Finkelstein, 2004). This mechanism can be considered a change in the standard of payment because the government is influencing the basis of consumer purchasing behavior.

A more direct example of a payment criteria change would be the Civil War and World War I procurement programs that were linked with a large increase in the number of prosthetic device patents, providing additional evidence that demand shocks can have a strong effect on innovation (Clemens & Rogers, 2020). Under the procurement program, the government specifies the reimbursement process that can influence the types of innovation (Clemens & Rogers, 2020). During the Civil War, innovations that reduce costs of production substantially increased, potentially fueled by the period's cost-conscious

procurement model (Clemens & Rogers, 2020). There is historical evidence that innovation in both vaccines and prosthetic devices has been driven by changes in the standards of payment.

Shifting away from the demand side, governments can also stimulate innovation by directly funding projects. The COVID-19 pandemic highlighted the underinvestment in vaccine research, leading to calls for innovation policies that make vaccines more profitable and widely accessible such as prizes, subsidies, grants, and tax incentives (Xue & Ouellette, 2020). Although there appears to be a lack of incentives for innovation in the vaccine market, there are R&D grants and loans for early-stage companies and substantial literature on their impacts on subsequent firm performance. Overall, the literature has found that direct government funding via grants or loans increases the probability that a startup receives subsequent funding (Zhao & Ziedonis, 2020; Howell, 2017; Santoleri et al., 2020; Lerner, 1999). Recipients of such funding are also positively associated with other performance outcomes such as firm survival, revenue, sales, assets, employment, and patenting funding (Zhao & Ziedonis, 2020; Howell, 2017; Santoleri et al., 2020; Lerner, 1999). However, such a relationship was not replicated in a study of China's Innofund program that uses a similar grant award mechanism, potentially due to the challenges of a developing economy (Wang et al., 2017). Thus, there is considerable evidence that direct government funding for startups can have a positive impact on firm performance and therefore innovation.

The expansion of Medicare Advantage supplemental benefit is a demand shock driven by a change in the payment criteria because the government has now allowed insurers to pay MA vendors for their services. Leveraging the framework put forth in the demand side literature, I utilize the methodologies put forth in the direct funding literature to assess the impacts of the MA supplemental benefit expansion on vendor startup performance. Combining the ideas from healthcare and general innovation literature, this study aims to provide evidence on how government regulation in healthcare

insurance markets spurs innovation in the private sector. My hypothesis is that the expansion of Medicare Advantage supplemental benefits leads to increased funding for start-up firms that supply those benefits.

3. Data

3.1 Data Source

To assess the funding of these companies, I use Crunchbase as my main data source. Crunchbase is a crowdsourced database of startups that gathers its data from the monthly portfolio updates of more than 4,000 global investment firms and their active community of executives, entrepreneurs, and investors. The accuracy of such data is self-validated through Crunchbase's artificial intelligence and machine learning algorithms, as well as manual data validation.

3.2 Data Panel Construction

From Crunchbase, I pulled the data using their API on January 29th, 2023 and primarily worked with the organizations.csv and funding.csv files. The organizations.csv file includes descriptive information on each firm that is up to date as of the data pull, and the funding.csv file includes time series data on funding deals for a firm. First, I identified organizations based in the United States whose primary role is a company and not an investor. Using the date of the first CMS announcement (April 27th, 2018) as the target of this event study, I chose an 8-year window covering 4 years before the announcement and 4 years after (April 27th, 2014 to April 27th, 2022), following firms that existed at baseline (April 27th, 2014) and new firms that were created during this window. Firms that were missing a found year and were 10 years or older at baseline were excluded from this study because they are likely large, established firms if they have survived this long. Previous literature has used the same age restriction for startup classification (Schulte-Althoff et al., 2021; Steigertahl et al., 2018). Although

the organization.csv file includes information on the closure status of the firm, there is a lack of closure dates and so existence at baseline was more difficult to determine. For firms with closure dates, I excluded them if they closed before the baseline. As for firms missing a closure date, I assumed that a firm was closed if it had not raised any funding in the 3 years prior to the baseline (from April 27th, 2011 to April 27th, 2014). This assumption is based on the existing literature about the length of time between funding rounds for startups (Żbikowski & Antosiuk, 2021). At the end of this process, 79,004 firms were included in the final sample.

To construct a panel, I created a longitudinal data set from the set of firms specified above so that each firm has an observation for every year of the observation period. Companies that were created after the beginning of the study period were marked as missing in the years before their creation. Since I am analyzing funding on an annual basis, I included funding rounds for the entire year of 2014 and 2022 despite the window being from April 27th, 2014 to April 27th, 2020 for consistency purposes.

3.3 MA Vendor Identification

I developed a system to identify firms that would possibly benefit from the supplemental benefit expansion based on data from Crunchbase describing a firm's industry functions and refer to these firms as "MA vendors." Crunchbase tags each firm with category group tags and category tags. Each category is associated with one or multiple category groups, and a category group has multiple categories associated with it. For example, 'Neuroscience' is a category, and it is mapped to the 'Biotechnology' and 'Science and Engineering' category groups. Both the 'Biotechnology' and 'Science and Engineering' category groups have multiple other categories mapped to them. A firm can be tagged with multiple category group tags and multiple category tags. To identify startups that are or would be MA vendors, I identified category group tags that would fit within the guidelines of the CMS announcements for supplemental benefit expansion. Such category tags are listed in Exhibit A.1 of the Appendix.

Startups are categorized as MA vendors if they have the appropriate category tag and a 'Health Care' category group tag. All category group tags are listed in Exhibit A.2 of the Appendix. In the end, this approach classified 4,757 firms as MA vendors. Although this approach is not the most precise, I tried a more precise approach where I identified potential keywords that would classify a firm as a MA vendor and then searched for those keywords in the firm's short description. However, this method only identified a small subset of the firms that could potentially be impacted by the policy change.

3.4 Summary Statistics

Table 1 summarizes the mean funding per deal that a firm receives in a year for the full sample, the MA vendors treatment group, and the control group that includes all other U.S.-based firms in the Crunchbase dataset. This mean amount is conditional on a firm receiving funding that year and having an associated value for that deal, excluding all other observations. Looking at the annual percentage changes, MA vendors appear to slightly outperform other firms on the mean funding received in the latter half of the observation window, after the expansion of supplemental benefits.

Table 1. Mean funding (Real 2014 $USD) per deal a firm receives by year

	All	Annual Δ%	MA Vendors	Annual Δ%	Non-MA Firms	Annual Δ%
2014	8,557,042 (57,274,610)	---	5,749,952 (22,480,453)	---	8,711,885 (58,592,368)	---
2015	9,911,543 (57,730,658)	16%	7,455,336 (21,731,029)	30%	10,065,635 (59,261,758)	16%
2016	10,691,448 (69,540,527)	8%	5,964,439 (13,481,818)	-20%	11,033,468 (71,909,380)	10%
2017	13,686,823 (1.090e+08)	28%	7,467,387 (20,652,892)	25%	14,133,832 (1.127e+08)	28%
2018	24,739,624 (9.273e+08)	81%	11,472,948 (36,566,743)	54%	25,726,880 (9.611e+08)	82%
2019	15,790,809 (76,715,443)	-36%	9,872,336 (26,297,350)	-14%	16,271,105 (79,396,337)	-37%
2020	19,590,107 (1.098e+08)	24%	15,796,209 (50,238,606)	60%	19,902,628 (1.133e+08)	22%
2021	29,500,644 (1.354e+08)	51%	25,201,907 (75,839,735)	60%	29,846,580 (1.390e+08)	50%
2022	24,368,198 (1.239e+08)	-17%	13,661,160 (40,864,253)	-46%	25,154,608 (1.279e+08)	-16%
N	95,333		6,486		88,847	

Note: Standard deviations in parentheses

Table 2 summarizes the number of funding deals that all firms, MA vendor firms, and all other firms received in a year. Examining the annual percentage changes, the MA vendors do not appear to noticeably outperform other firms on the number of funding deals received in the years following the expansion of supplemental benefits.

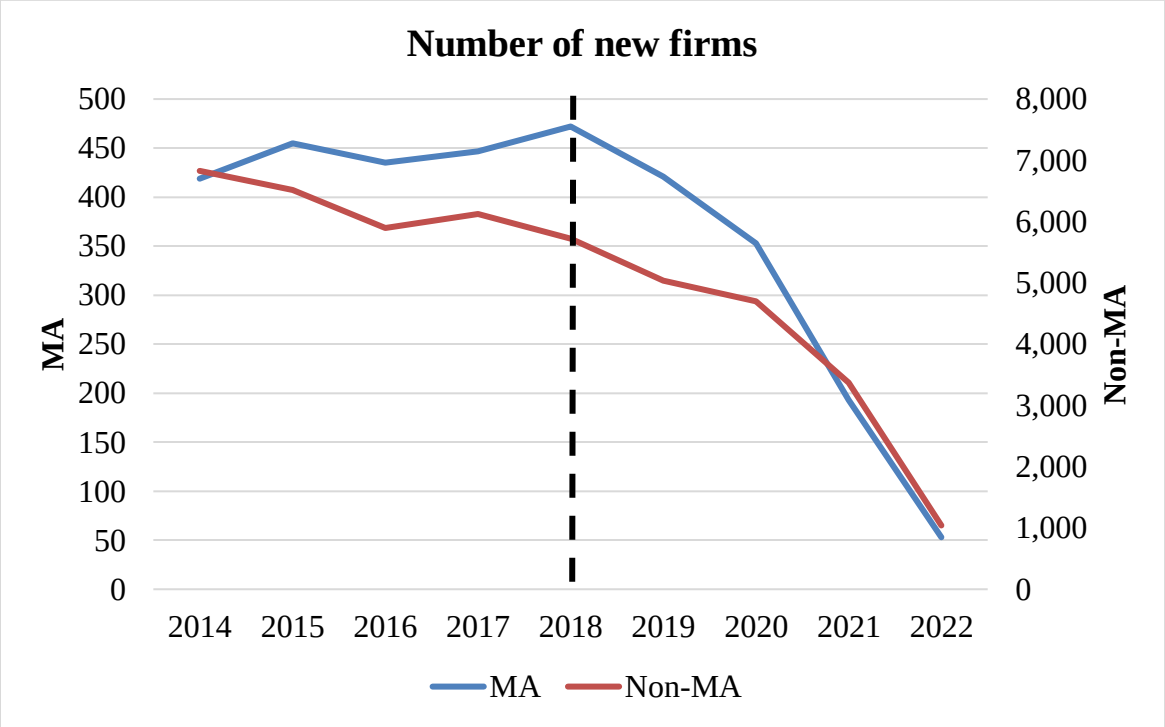
Table 2. Annual number of funding deals a group receives

	All	Annual Δ%	MA Vendors	Annual Δ%	Non-MA Firms	Annual Δ%
2014	14,957 (33%)	---	758 (33%)	---	14,199 (33%)	---
2015	15,439 (29%)	3%	889 (32%)	17%	14,550 (29%)	2%
2016	14,283 (24%)	-7%	982 (29%)	10%	13,301 (24%)	-9%
2017	15,070 (22%)	6%	1,071 (27%)	9%	13,999 (22%)	5%
2018	16,736 (22%)	11%	1,234 (27%)	15%	15,502 (22%)	11%
2019	17,161 (21%)	3%	1,357 (27%)	10%	15,804 (21%)	2%
2020	17,646 (20%)	3%	1,395 (25%)	3%	16,251 (19%)	3%
2021	21,080 (22%)	19%	1,626 (28%)	17%	19,454 (22%)	20%
2022	16,725 (18%)	-21%	1,176 (21%)	-28%	15,549 (18%)	-20%
Total	149,097		10,488		138,609	

Note: Percentage of firms in that group receiving a deal that year in parentheses

Analyzing Exhibit A.3 of the Appendix, the mean funding per deal noticeably jumps for MA vendors after the supplemental benefit expansion whereas the upward trend for non-MA firms is less pronounced. Exhibit A.4 of the Appendix shows that the number of funding deals for both MA vendors and non-MA firms has generally remained stagnant following the policy change. Another consideration is the creation of new MA vendors after the supplemental benefit expansion compared to the formation of other firms. Figure 1 shows that firm entry for both the MA vendor market and other sectors declines over the study period. MA vendor entry appears to decrease at a slightly lower rate than other firms after the policy change. This study chooses to focus on examining startup funding as opposed to entry but includes firm entry considerations in the analysis.

Figure 1. Number of new firms entering the MA vendor market and other sectors



4. Methods and Empirical Framework

4.1 Empirical Foundation

With the CMS announcement of the expansion of MA supplemental benefit expansion, the expected profit of MA vendor startups likely increased as they could soon sell their services to MA plans. As such, we could draw a parallel between this policy change and government grants to these startups. Although MA vendors are not receiving funding directly from the government, the policy change allows for an indirect flow of funds from the government to MA plans to MA vendors. Consequently, the same empirical approach for government grants can be applied to this case. There is substantial research into the impacts of government funding on VC funding (Zhao & Ziedonis, 2020; Howell, 2017; Santoleri et al., 2020; Wang et al., 2017; Lerner, 1999). One study examined the Michigan R&D loan program and found a positive impact of this subsidy program on startup survival

and follow-on VC investments (Zhao & Ziedonis, 2020). My analysis of firm funding is grounded in the empirical approach put forth in that study. Specifically, I am examining funding for startups at both the intensive (the amount of funding) and extensive (the number of deals) margins.

4.2 Regression Models

Employing a difference-in-difference design, I am comparing funding for MA vendors and all other firms before and after the benefit expansion, conditional on a firm's existence at baseline or entry during the observation window. The main hypothesis is that the supplemental benefit expansion increases the amount of funding a MA vendor receives relative to other firms. In the data set, there are 53,764 funding deals without an associated funding amount and so I included a second hypothesis for the number of funding deals to capture this missing funding information. This additional hypothesis is that the policy change increases the number of funding deals a MA vendor receives when compared to other firms. Below is the linear regression model that I employ for the primary hypothesis:

$$(1)\ \ln(Amount\ of\ Funding + 1)_{it} = \beta_0 + \beta_1 MA_i + \beta_2 Post_t + \beta_3 MA_i * Post_t + \beta_4 Health_i + \beta_5 Health_i * Post_t + \beta_6 Previous\ Funding\ Count_{it} + \beta_7 Age_{it} + \beta_8 Age^2{}_{it} + \beta_9 Year_t + Sector_i + State_i + \mu_{it}$$

For Eq. (1), $\ln(Amount\ of\ Funding + 1)_{it}$ is the log transformation of the total amount of funding a firm i receives in year t. If a funding deal had occurred in that year but did not have an associated amount, then the funding amount was marked as missing. If a funding deal did not occur in that year, then the funding amount was considered 0. MA_i is a binary variable indicating whether firm i is a MA vendor (1 = MA vendor; 0 = other firms), $Post_t$ is a binary variable indicating the whether the funding deal was announced before or after the policy change (0 = before 2018; 1 = 2018 and after), $Health_i$ is a binary variable indicating whether firm i has a 'Health Care' category group tag and is not

classified as a MA vendor, $Previous\ Funding\ Count_{it}$ is a continuous variable that is the cumulative count of previous funding deals for firm i at year t, Age_{it} is the age of the firm at year t, $Age^2{}_{it}$ is the age of the firm at year t squared, and $Year_t$ is a continuous variable for the year t. $Sector_i$ is a fixed effect for the different category group tags listed in Exhibit A.2 of the Appendix, $State_i$ is a state fixed effect for firm i, and μ_{it} are the unobservable characteristics of firm i.

Similar to Eq. (1), the following is the linear regression model that I use to test the secondary hypothesis:

(2) $Funding\ Deal\ Count_{it} = \beta_0 + \beta_1 MA_i + \beta_2 Post_t + \beta_3 MA_i * Post_t + \beta_4 Health_i + \beta_5 Health_i * Post_t + \beta_6 Previous\ Funding\ Count_{it} + \beta_7 Age_{it} + \beta_8 Age^2{}_{it} + \beta_9 Year_t + Sector_i + State_i + \mu_{it}$

In Eq. (2), $Funding\ Deal\ Count_{it}$ is the number of funding deals a firm i receives in year t, MA_i is a binary variable indicating whether firm i is a MA vendor (1 = MA vendor; 0 = other firms), $Post_t$ is a binary variable indicating the whether the funding deal was announced before or after the policy change (0 = before 2018; 1 = 2018 and after), $Health_i$ is a binary variable indicating whether firm i has a 'Health Care' category group tag and is not classified as a MA vendor, $Previous\ Funding\ Count_{it}$ is a continuous variable that is the cumulative count of previous funding deals for firm i at year t, Age_{it} is the age of the firm at year t, $Age^2{}_{it}$ is the age of the firm at year t squared, and $Year_t$ is a continuous variable for the year t. $Sector_i$ is a fixed effect for the different category group tags listed in Exhibit A.2 of the Appendix, $State_i$ is a state fixed effect for firm i, and μ_{it} are the unobservable characteristics of firm i.

To remove the effect of the COVID-19 pandemic accelerating health innovation, I ran an additional regression on a restricted sample with funding data ranging only from 2014 to 2019. Firm fixed effects were originally included but eventually dropped because they were only capturing variable

deviations from time averages as there were too few funding observations per firm. As result, firm fixed effects would cause MA_i and other variables that remain constant for a firm to be dropped. Since MA_i is a key variable of interest, firm fixed effects were dropped and thus subjecting the regressions to more omitted variable bias. To mediate this, I implement sector and state fixed effects, information that would have been captured in the firm fixed effects. Time fixed effects were originally included but eventually dropped due to collinearity with $Post_t$. The usage of a time trend instead of time fixed effects dampens the ability of the regressions to capture macro shocks to funding activity that could cause it to fluctuate each year such as the COVID-19 pandemic and the subsequent recessionary conditions. Regressions with time fixed effects and without $Post_t$ as a separate covariate are included as a supplement to capture such shocks.

5. Results

5.1 Amount of Funding

Table 3 shows the random effects GLS estimates for classification as a MA vendor, observation after the policy change, the interaction effect between those two, and six control variables on the amount of funding a firm receives. Holding all firm-level controls constant, a MA vendor is expected to receive more funding than other firms ($p<0.001$). After the supplemental benefit expansion, all firms are expected to receive less funding than before the policy change ($p<0.001$). These findings make sense because the macro trend of an increasing elderly population has been a tailwind for MA vendors, and the years following the policy change include recessionary conditions that negatively impact funding. The interaction between MA vendor status and observation after the policy change dummy variables yields a statistically significant positive coefficient ($p<0.01$). Thus, MA vendors are expected to receive a greater amount of funding (18% increase) after the policy change than other firms on average. This supports the

intuition presented in the first hypothesis that MA vendors would receive larger amounts of funding than other firms due to the growth in expected profits following the supplemental benefit expansion. Such an increase in the amount of funding can be driven by 1) existing firms receiving a larger amount of funding and 2) new firms entering the market and getting funding. I cannot differentiate between the effects of these two drivers because Eq. (1) does not capture the possible entry of new firms as a consequence of the supplemental benefit expansion.

Each increase in the number of previous funding rounds for a firm is associated with an increase in the amount of funding for the firm ($p<0.05$). Serving as an indicator of firm quality, the number of previous funding rounds captures investor interest in the firm and thus is expected to be positively correlated with the amount of funding. The age of the firm is expected to have a negative effect on the amount of funding received ($p<0.001$). Such a finding makes sense because more mature firms raise money less often and thus have more zeros for funding raised in a year. There is a significant positive yearly time trend for the amount of funding ($p<0.001$). Healthcare firms (excluding MA vendors) are expected to receive more funding than other firms ($p<0.001$). A healthcare firm is expected to receive a greater amount of funding (19% increase) after the supplemental benefit expansion than other firms on average. Given the COVID-19 pandemic's acceleration of healthcare innovation across the board this result is in line with expectations. By isolating the effect of being a healthcare firm from being a MA vendor, the treatment effect (interaction of MA vendor status and observation after the policy change) is not confounded by factors impacting the healthcare sector in general. This estimated 19% increase in the amount of funding for healthcare firms is larger than the estimated 18% increase in the amount of funding for MA vendors after the policy change, suggesting that the funding increase was an overall healthcare sector trend and may not be specific to MA vendors. These increases in the amount of funding are not significantly different from each other, as determined by a Wald test.

The estimated coefficients for the restricted sample regression yield the same directional results as the full sample regression except for the after policy change and year time trend covariates. Since the restricted sample regression is excluding funding data from the COVID-19 years (2020 to 2022) from the full sample, it is not surprising that the coefficients of time-dependent variables have changed. Unlike the full sample regression results, the estimated increase (20%) in the amount of funding for MA vendors is larger than the estimated increase (19%) for healthcare firms after the supplemental benefit expansion. A Wald test found no significant difference between the increase in the amount of funding for MA vendors and healthcare firms. Although the estimated funding amount increase is larger for MA vendors in the period after the policy change and before the COVID-19 period, the increase in funding amount for MA vendors cannot be differentiated from the overall increase in funding amount for the healthcare sector at large. For the time fixed effects regression, the results in Exhibit A.5 are directionally consistent with the results of the full sample regression. All coefficient interpretations are made assuming all other variables are held constant.

Table 3. Amount of funding a startup receives before and after the supplemental benefit expansion conditional on Medicare Advantage vendor status

RE	ln(Amount of funding + 1)	
	Full Sample	Restricted Sample
Predictors	Estimates	
MA vendor, 1=yes	0.6524*** (0.0655)	0.8175*** (0.0718)
After policy change, 1=yes	-0.1708*** (0.0299)	0.5996*** (0.0371)
MA vendor * After policy change	0.1817** (0.0679)	0.2047* (0.0857)
Healthcare firm, 1=yes	0.4709*** (0.0437)	0.6505*** (0.0496)
Healthcare firm * After policy change	0.1922*** (0.0420)	0.1924*** (0.0532)
Number of previous funding rounds	0.0126* (0.0058)	0.0241** (0.0085)
Age, years	-0.4998*** (0.0067)	-0.3017*** (0.0105)
Age^2	0.0149*** (0.0005)	0.0073*** (0.0009)
Year	0.0375*** (0.0059)	-0.3565*** (0.0109)
Constant	-72.4833*** (11.8671)	720.5002*** (22.0208)
Sector fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Observations	525,167	304,917
R^2	0.0602	0.0474

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

5.2 Number of Funding Deals

Table 4 shows the random effects GLS estimates for status as a MA vendor, observation after the policy change, the interaction effect between those two, and the six control variables on the number of funding deals a firm receives. A MA vendor is expected to receive a higher number of funding deals than other firms (p<0.001), consistent with the intuition that MA vendor performance has been fueled by

the increasing elderly population. Being observed in the period following the supplemental benefit expansions does not have a statistically significant effect on the count of funding deals for a firm, suggesting that the policy change had no significant impact on the overall number of funding deals. The interaction term between MA vendor classification and observation after the policy change has a statistically significant positive coefficient ($p<0.001$). MA vendors are expected to have 0.0235 more funding deals per year after the policy change than other firms on average. This finding provides evidence that supports the second hypothesis that MA vendors would receive more funding deals than other firms due to an increase in expected profit after the supplemental benefit expansion. Since Eq. (2) does not capture the possible entry of new firms after the policy change, an increase in the number of funding deals for MA vendors can be the result of existing firms getting a greater amount of funding and new firms entering the market and receiving funding. The effects of existing firms and new firms on funding deal count cannot be isolated.

With every increase in a firm's number of previous funding rounds, there is an associated increase in the number of funding deals ($p<0.001$), in line with the expectation that firms with more previous funding rounds are of higher quality and thus more likely to receive additional funding deals. As the age of a firm increases, the number of funding deals is expected to decrease ($p<0.001$), consistent with the intuition that more mature firms need to raise funding less often. There appears to be no significant yearly time trend for the number of funding deals. A healthcare firm is expected to get more funding deals than other firms ($p<0.001$) and to receive 0.0166 more funding deals per year after the policy change than other firms on average ($p<0.001$). These results are in line with the intuition on healthcare firms presented in the previous section with the COVID-19 pandemic fueling healthcare innovation. Including a control for status as a healthcare firm, the treatment effect is not confounded by omitted variables that impact the entire healthcare sector. Such an estimated 0.0166 increase in the

number of funding deals for healthcare firms is less than the estimated 0.0235 increase in the number of funding deals for MA vendors after the supplemental benefit expansion. A Wald test found no significant difference between the increase in the number of funding deals for healthcare firms and MA vendors. Thus, the increase in the number of funding deals for MA vendors after the policy change cannot be isolated from the general trend of increasing funding deals for all healthcare firms.

The estimated coefficients for the restricted sample regression produce the same directional results as the full sample regression except for the after policy change, number of previous funding rounds, and year time trend covariates. It is unsurprising that the coefficients of the time-dependent variables (after policy change and year time trend) have changed because the sample has been limited to the pre-COVID years (2014 to 2019). In the restricted sample regression, an increase in the number of previous funding rounds is expected to decrease the number of funding deals a firm receives. This is inconsistent with the previously presented intuition and suggests that the relationship between the number of previous funding rounds and the number of funding deals is different during the restricted time period. The difference between the estimated 0.0347 increase in the number of funding rounds for MA vendors and the estimated 0.0213 increase in the number of funding rounds for healthcare firms after the policy change is marginally significant ($p<0.15$). In the period before the COVID-19 pandemic, the supplemental benefit expansion is associated with an increase in the number of funding deals for MA vendors that is likely beyond the overall increase in the number of funding deals for the healthcare sector. The results of the time fixed effects regression in Exhibit A.6 are directionally consistent with the full sample regression. All coefficient interpretations are made assuming all other variables are held constant.

Table 4. Number of funding deals per year a startup receives before and after the supplemental benefit expansion conditional on Medicare Advantage vendor status

RE	Funding Deal Count	
	Full Sample	Restricted Sample
Predictors	Estimates	
MA vendor, 1=yes	0.0615*** (0.0058)	0.0761*** (0.0068)
After policy change, 1=yes	-0.0002 (0.0029)	0.0574*** (0.0035)
MA vendor * After policy change	0.0235*** (0.0064)	0.0347*** (0.0080)
Healthcare firm, 1=yes	0.0351*** (0.0038)	0.0521*** (0.0047)
Healthcare firm * After policy change	0.0166*** (0.0040)	0.0213*** (0.0051)
Number of previous funding rounds	0.0136*** (0.0005)	-0.0116*** (0.0008)
Age, years	-0.0710*** (0.0006)	-0.0442*** (0.0010)
Age^2	0.0027*** (0.00004)	0.0017*** (0.00008)
Year	0.0009 (0.0006)	-0.0312*** (0.0010)
Constant	-1.4393 (1.1186)	63.1037*** (2.0783)
Sector fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Observations	555,513	324,277
R^2	0.0773	0.0495

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

6. Discussion

The results provide evidence that the expansion of Medicare Advantage supplemental benefits was associated with an increase in the number of funding deals and the average size of those deals toward Medicare Advantage vendor firms. Thus, the supplemental benefit expansion has been shown to have a significant impact on funding at both the intensive (the amount of funding per deal) and extensive

(the number of deals per firm) margin. However, the same result was also observed for the healthcare sector as a whole and thus it is possible that the MA vendors were just riding off the general healthcare sector trend.

With an increased flow of funding towards MA vendors, insurers will have a larger and higher quality pool of vendors to work with when implementing these new supplemental benefits. Such an observation demonstrates that the expansion of supplemental benefits has stimulated innovation in insurer benefit design as the MA vendor market becomes more and more developed with the influx of capital. This suggests that future health insurance policies can indirectly fund and promote innovation through demand shocks to further its goals. Although it is still unclear whether these new supplemental benefits will bring about cost savings, the increased flow of funding toward these MA vendors is a promising sign and will at least bring about experimentation with different benefits.

Tracking the funding towards MA vendors indicates that there is investor interest in the companies, theoretically driven by the increased expected profits. Still, this does not inherently mean that the additional offerings of the MA vendors are innovative. There is some correlation as more innovative companies have a competitive advantage and attract more investor interest, but it is not a direct measurement of additional innovation on the part of MA vendors. Another limitation is that a causal relationship cannot be definitively proven between the expansion of supplemental benefits and increased funding towards MA vendors, especially given that funding increased for all healthcare firms. After the expansion of supplemental benefits, the increase in the amount of funding and number of funding deals for MA vendors cannot be isolated from the funding trend in the overall healthcare sector. A possible explanation for such a finding is that another event impacted all healthcare firms at around the same time, such as the COVID-19 pandemic. When the effects of the COVID-19 pandemic were removed, there did appear to be a short-term increase in the number of funding rounds for MA vendors

following the policy change that can be separated from other healthcare firms. Another possible explanation is that the supplemental benefit expansion affected the entire healthcare sector, but this is unlikely given the specific nature of the policy change. One more possibility is that the MA vendor identification approach is unable to properly discern MA vendors from other healthcare firms. The results do suggest some casual inference between the supplemental benefit expansion and funding for MA vendors, strengthened by the study of a discrete event and the inclusion of a time trend. However, the lack of deviation from the increase in funding for the whole healthcare sector after the policy change adds some uncertainty.

To operationalize being impacted by the policy change, the regression simply utilized the year of the policy change and so the effect of the policy change could be confounded by other events with long-term impacts that occurred in 2018. Such events were controlled for by the inclusion of a time trend but that does not completely address the concern. The COVID-19 pandemic was a major event that occurred during the period following the expansion and could have biased the results as healthcare innovation was majorly accelerated during the time. This was controlled for by running a restricted sample regression for funding data pre-COVID and including a healthcare sector dummy, but this also substantially limits the post-period. Lastly, the regressions employed in this study have low explanatory power as demonstrated by the low R-squared values and so the models are not accounting for a larger majority of the variation in the data.

7. Conclusion

Overall, the findings presented in this paper are consistent with existing literature on policies leveraging demand shocks to encourage innovation. The main contribution of this study is applying the framework developed in the existing innovation policy studies to the relatively understudied MA vendor

market that resulted from the recent supplemental benefit expansion. This analysis documents how the government funding of private firms that are purchasers of innovation can be another mechanism to drive innovation in healthcare markets, proving to be another application of the changing standards of payment strategy. Such a finding is especially important in the healthcare sector because the government is one of the largest payers for healthcare and thus an important part of the innovation ecosystem. As a result, this research also preliminary assesses the implementation of the new supplemental benefit expansion in private markets. Because the MA vendor industry was rather underdeveloped, increased funding towards those firms will enable insurers to have a bigger and higher quality group of vendors to partner with when adopting the supplemental benefits. Before the jury is out on health outcomes, an understanding of the policy execution is paramount. Based on the findings discussed in the paper, increased funding towards MA vendors suggests that innovation in supplemental benefits is likely to be positively impacted by the policy change.

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Appendix

Exhibit A.1 List of category tags for MA vendor classification

- Language Learning
- Recreation
- Assisted Living
- Transportation
- Organic Food
- Personal Finance
- Restaurants
- Rehabilitation
- Outdoors
- Home Renovation
- Psychology
- Fitness
- Alternative Medicine
- Retirement
- Elderly
- Communities
- mHealth
- Personal Health
- Nutrition
- Diabetes
- Elder Care
- Wearables
- Delivery
- Ride Sharing
- Food Delivery
- Car Sharing
- Home Services
- Home Improvement
- Wellness
- Residential
- Grocery
- Home and Garden
- Assistive Technology
- Food and Beverage
- Taxi Service
- Building Maintenance
- Extermination Service
- Housekeeping Service
- Home Health Care

- Social Assistance
- Nursing and Residential Care

Exhibit A.2 List of all category group tags

- Administrative Services
- Advertising
- Agriculture and Farming
- Apps
- Artificial Intelligence
- Biotechnology
- Clothing and Apparel
- Commerce and Shopping
- Community and Lifestyle
- Consumer Electronics
- Consumer Goods
- Content and Publishing
- Data and Analytics
- Design
- Education
- Energy
- Events
- Financial Services
- Food and Beverage
- Gaming
- Government and Military
- Hardware
- Health Care
- Information Technology
- Internet Services
- Lending and Investments
- Manufacturing
- Media and Entertainment
- Messaging and Telecommunications
- Mobile
- Music and Audio
- Natural Resources
- Navigation and Mapping
- Other
- Payments
- Platforms
- Privacy and Security

- Professional Services
- Real Estate
- Sales and Marketing
- Science and Engineering
- Software
- Sports
- Sustainability
- Transportation
- Travel and Tourism
- Video

Exhibit A.3 Mean funding (Real 2014 $USD) per deal a firm receives by year

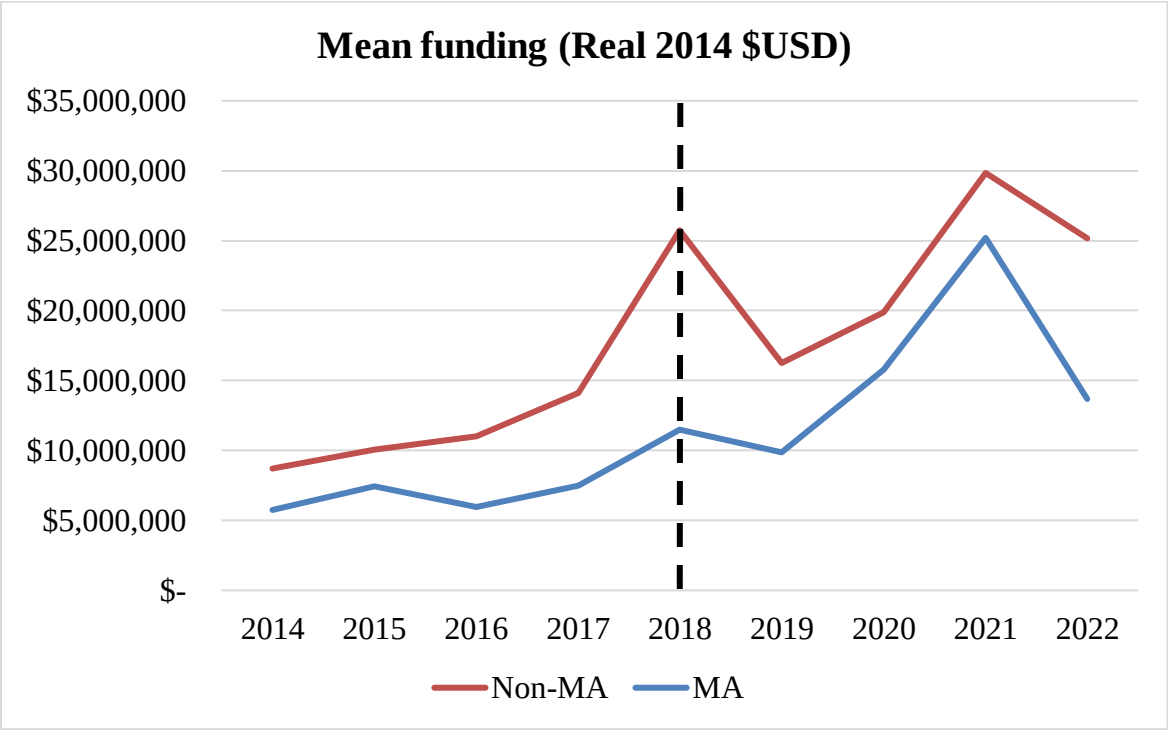

Exhibit A.4 Annual number of funding deals a group receives

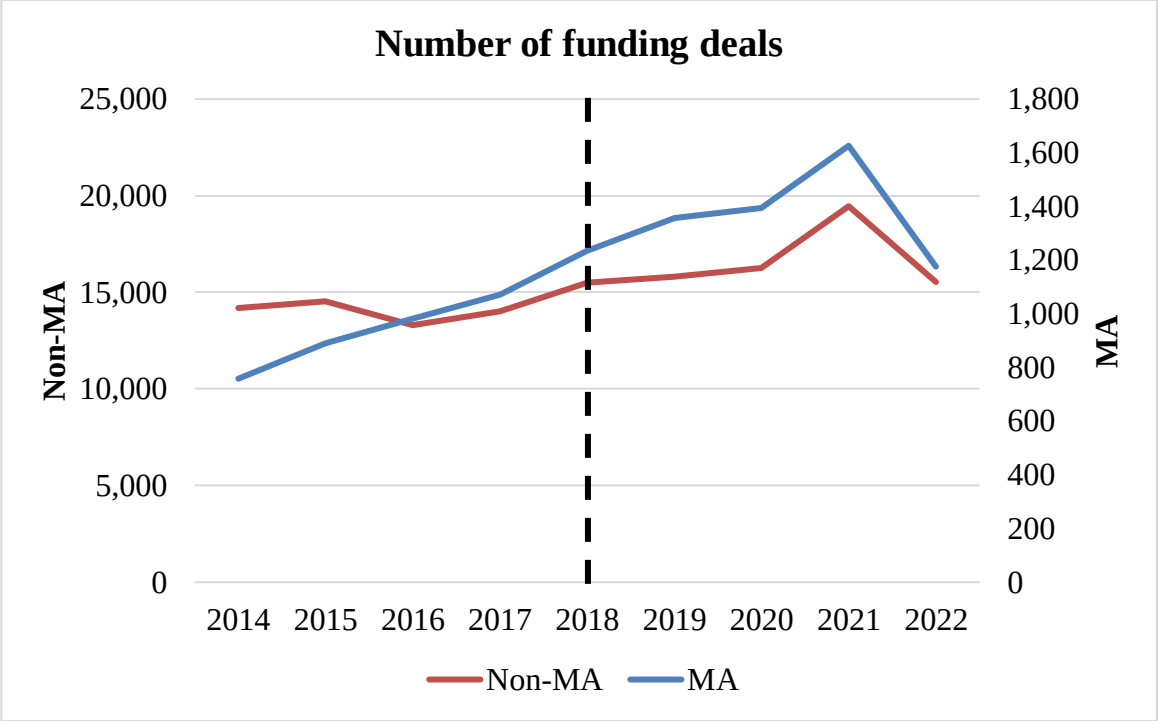

Exhibit A.5 Amount of funding time fixed effects regression results

RE	ln(Amount of funding + 1)
Predictors	Estimates
MA vendor, 1=yes	0.6652*** (0.0652)
After policy change, 1=yes	---
MA vendor * After policy change	0.1618* (0.0679)
Healthcare firm, 1=yes	0.4668*** (0.0434)
Healthcare firm * After policy change	0.1991*** (0.0419)
Number of previous funding rounds	0.0289*** (0.0057)
Age, years	-0.4905*** (0.0067)
Age^2	0.0139*** (0.0005)
Constant	3.7427*** (0.3957)
Year	---
Year fixed effects	Yes
2015	-0.3619*** (0.0377)
2016	-0.9121*** (0.0369)
2017	-0.9195*** (0.0363)
2018	-0.8274*** (0.0369)
2019	-0.8562*** (0.0369)
2020	-0.8684*** (0.0371)
2021	-0.2060*** (0.0375)
2022	-0.4034*** (0.0383)
Sector fixed effects	Yes
State fixed effects	Yes
Observations	525,167
R^2	0.0642

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001

Exhibit A.6 Number of funding deals per year time fixed effects regression results

RE	Funding Deal Count
Predictors	Estimates
MA vendor, 1=yes	0.0628*** (0.0057)
After policy change, 1=yes	---
MA vendor * After policy change	0.0212** (0.0064)
Healthcare firm, 1=yes	0.0349*** (0.0038)
Healthcare firm * After policy change	0.0167*** (0.0040)
Number of previous funding rounds	0.0157*** (0.0005)
Age, years	-0.0706*** (0.0006)
Age^2	0.0027*** (0.00004)
Year	---
Constant	0.4527*** (0.0324)
Year fixed effects	Yes
2015	-0.0369*** (0.0036)
2016	-0.0855*** (0.0035)
2017	-0.0882*** (0.0035)
2018	-0.0754*** (0.0035)
2019	-0.0727*** (0.0035)
2020	-0.0666*** (0.0035)
2021	-0.0143*** (0.0035)
2022	-0.0490*** (0.0035)
Sector fixed effects	Yes
State fixed effects	Yes
Observations	555,513
R^2	0.0804

Standard errors in parentheses
* p<0.05, ** p<0.01, *** p<0.001