

**Online Appendix to:  
NFT Disruption in Platform Competition: Evidence from  
Trading Card Collectibles**

**Table 1 Overview of Appendices.**

#	Concern	Check	Appendix
<b>Shock Date Validity</b>			
1	Is the focal estimate merely a seasonal effect?	Test for Granger causality Control for major sports events Exclude the period of a major sports event Employ trajectory balancing and look-ahead sampling only on basketball cards Analyze the eBay data for the same time window but a year later (2022) Use basketball cards from 2022 as the control group Analyze the period after the shock	1.A 1.A 1.A 2.C.1 1.D.2 1.D.2 1.H.1
2	Does the shock date selection affect the focal estimates?	Analyze PWCC data from June 19, 2020 to April 9, 2021 Analyze the results by trying out 40 different days around our original shock date	1.B 1.C
<b>Control Group Validity</b>			
3	Are the treatment and control group too different?	Propensity score matching	1.D.3
4	Are baseball card prices affected by the NTS entry?	Before-after analysis of baseball card prices  DiD analysis following Eichenbaum et al. (2024) with baseball card data from 2021 and 2022	1.E  1.E
5	Are the results driven by an incompatible control group?	Employ trajectory balancing and look-ahead sampling only on basketball cards Use basketball cards from 2022 as the control group	2.C.1 1.D.2
<b>Focal Estimate - Potential Biases</b>			
6	Is the focal estimate driven by selection-based endogeneity?	Employ trajectory balancing and look-ahead sampling only on basketball cards	2.C.1
7	Is the original observation window too narrow to have reliable results?	Analyze PWCC data from June 19, 2020 to April 9, 2021	1.B
8	Is the focal estimate biased by serial correlation between card prices and unobservables?	Randomized treatment test following Bertrand et al. (2004)	1.F
9	Is the focal estimate affected by cards listed before and sold after the NTS entry?	Exclude these cards in our main model	1.G
10	Does the focal estimate diminish in the short-term and in the long-term?	Analyze the period after the shock and eBay 2022 data	1.H
11	Is the focal estimate affected by sellers exiting the market?	Analyze the listings of sellers who are present after the shock	1.I
12	Is the focal estimate affected by outliers?	Exclude outliers in our main model	1.J

**Table 2 Overview of Appendices (Continued).**

#	Concern	Check	Appendix
Listing Formats & Moderators			
13	Are the items listed under the two different formats inherently different?	Analyze the differences of the two samples	2.A
14	Are the results robust to other specifications of product scarcity, product quality, and seller reputation?	Analyze the subsample with quartiles analysis and three-way interaction terms	2.B
15	Are the results driven by selection-based endogeneity?	Test alternative identification strategies using (1) trajectory balancing and look-ahead sampling and (2) matching	2.C
NLP Model and PWCC Search Tool			
16	How does the NLP model construct product identifiers?	Discuss how the model works	3.A
17	What is the PWCC search tool?	Show and explain the PWCC search tool	3.B
Parallel Trends			
18	Does the eBay 2021 data satisfy the assumption?	Run the three tests	4.A
19	Do the PWCC and eBay 2022 data satisfy the assumption?	Run the three tests for these data sets	4.B, 4.C
Understanding NFTs			
20	How do NFTs differ from other digital goods and how can these differences shape price effects?	Present the differences related to our setting and discuss how they may shape price effects	5.A, 5.B
21	Why do collectors prefer one type of good over the other? What are the demographics of collectors? Is there evidence for market expansion?	Discuss the survey results	5.C

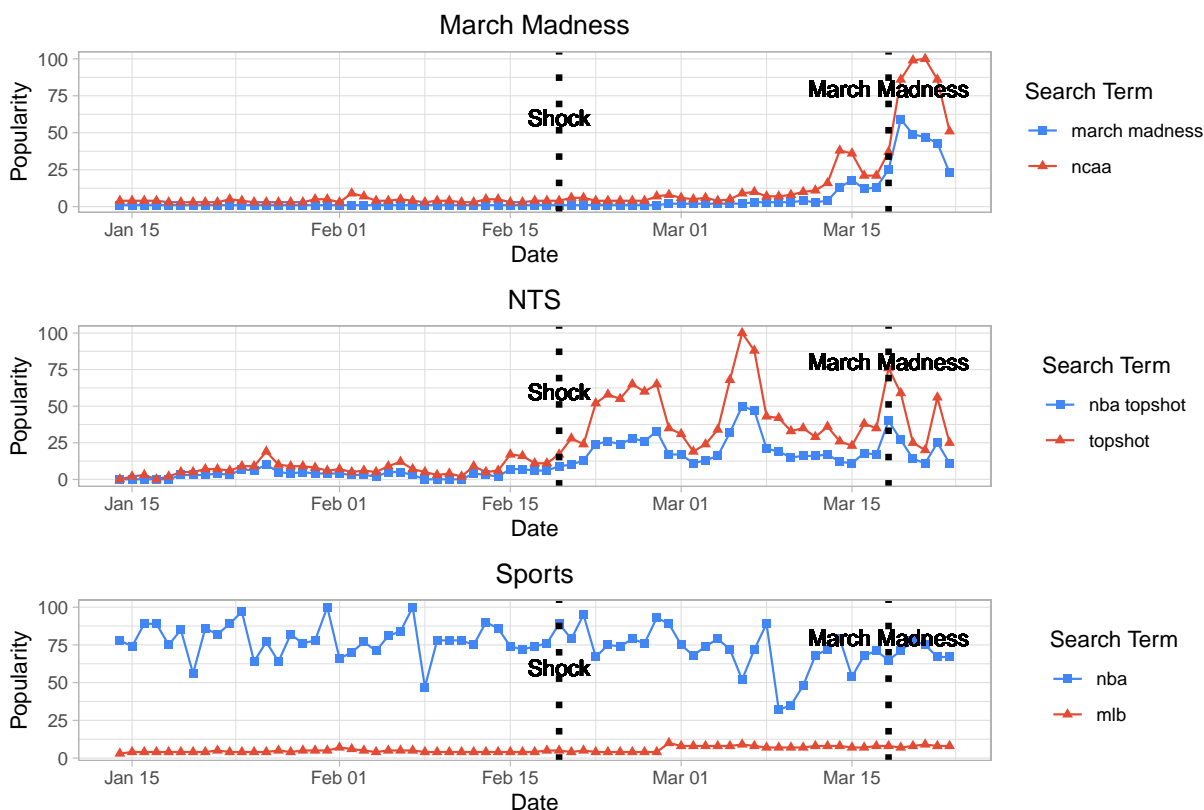
## Appendix. 1. Main Specification - Robustness Checks

### A. Major Sports Events

It is possible that the selected NTS shock can be affected by specific events in the sports calendar. To address such concerns, we have conducted a series of analyses that we present and discuss below. These analyses provide no evidence to support that our results are driven by said events.

One of these main events is NCAA March Madness. This could very well be the case, as March Madness is a USA tournament of the top men's college basketball teams. Naturally, the annual March Madness event draws the attention of fans of men's college basketball and as an extension, it could be a driving force behind our results. We start by checking whether this scenario is plausible. To do this, we plot the (Google) search term popularity of (1) March Madness, (2) NTS, and (3) the two sports. We present these charts in Figure 1 below. In each of these charts, we have added two vertical dotted lines indicating our shock date (February 19, 2021) and the beginning of March Madness (March 18, 2021). The charts provide interesting insight. From the March Madness chart (the top chart), we see that its search term popularity started rising very close to the beginning of that period and way after our shock date. This does not support the argument that March Madness could be driving our results. From the NTS chart (the middle chart), we see that its search term popularity began a few days after our chosen shock date and about a month before March Madness began. This provides further support that March Madness is unlikely to be driving our results. From the Sports chart (the bottom chart), we see that the popularity of the two sports remained fairly consistent across time. All in all, the evidence from the search term popularity charts does not support that our results are driven by the March Madness event.

We also study this using a Granger causality test. In a nutshell, this test is used to identify whether one time series affects the development of another time series. In this case, we aim to understand whether the popularity of NCAA March Madness affects the popularity of NTS, in which case the evidence would support that our results are driven by NCAA March Madness. For the popularity measures we use the ones that we presented in our previous analyses, namely the Google popularity of the search terms "march madness" and "ncaa" for NCAA March Madness and "nba topshot" and "topshot" for NTS. Each of the time series comprises a relatively generous time horizon around March Madness, spanning from January 14, 2021 up to March 23, 2021 (the same window as our eBay 2021 data), allowing us to capture interest in March Madness prior to March 2021. Since we have two search terms related to NCAA March Madness and two related to NTS, we need to implement four Granger causality tests. Prior to implementing a Granger causality test, we first need to set each model's "order" which is the number of past time lags that will be included in the model (e.g., an order of two means that the model includes the last two time data points). To identify each model's optimal order, we first use Kwiatkowski–Phillips–Schmidt–Shin (KPSS) tests to check whether each of these time series is stationary, and finally we use a Vector Autoregressive model to identify the optimal order. The Granger causality tests are insignificant for all cases: (1) "march madness" and "nba topshot" (F-statistic: 0.3632 and p-value 0.5488), (2) "march madness" and "topshot" (F-statistic: 0.4317 and p-value 0.5135), (3) "ncaa" and "nba topshot" (F-statistic: 0.2004 and p-value 0.6559), (4) "ncaa" and "topshot" (F-statistic: 0.1744 and p-value 0.6776). Hence, these tests provide no significant evidence to support that March Madness popularity Granger-causes NTS popularity, which means that March Madness popularity does not significantly help predict NTS popularity beyond its own past value.

**Figure 1** March Madness - Search Term Popularity.

Next, we proceed to statistically analyze and investigate whether March Madness could affect our results and we do this in two ways. One way is to rerun our main specification but exclude from our dataset the period relating to March Madness 2021. Given that the tournament started on March 18, 2021 and ended on March 29, 2021, for this analysis, we should exclude the dates from March 18, 2021 until March 29, 2021 (which is after the end of our observation window). We implement this and present the results in column (1) of Table 3. As we see, the coefficient estimate remains consistent supporting the validity of our shock date timing. In column (2) of the same Table, we restrict our observation window so that it ends 10 days prior to the beginning of March Madness - that is on March 7, 2021 - because interest for this event may start earlier than the event itself. As we see, the coefficient estimate is consistent with those of our main results in that it is negative and significant ( $p = 0.011$ ). Another way is to rerun our main specification but add controls for March Madness. We take this a step further and enrich our specification by adding controls for the sports seasonality of the two sports. In total, we add four controls. We add one control that identifies if a date is within: the 2021 March Madness period (March 18, 2021 up to March 29, 2021), the 2020-2021 NCAA season (November 25, 2020 up to March 14, 2021), the 2020-2021 NBA season (December 22, 2020 up to May 16, 2021), the 2021 MLB season (April 1, 2021 up to October 3, 2021). In summary, these variables not only control for the NCAA March Madness start, but also for more general seasonality of the relevant sports we investigate, but more on that in the next paragraph. We add the controls and rerun our main specification, whose results we present in Table 4

that follows<sup>1</sup>. As we see, the coefficient estimate remains very similar, indicating that differences in sports seasonality are unlikely to drive our estimated effect.

**Table 3 Main Specification Excluding March Madness Period.**

	Log Price	
	(1)	(2)
<i>After Shock</i> × <i>Basketball Card</i>	−0.049*** (0.018)	−0.059** (0.023)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Adjusted R <sup>2</sup>	0.640	0.642
Observations	861,093	701,779

*Notes.* In column (1), we analyze the period from January 14, 2021 up to March 17, 2021. In column (2), we analyze the period from January 14, 2021 up to March 7, 2021. In both specifications, we cluster the standard errors at the seller level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 4 Main Specification With Sports Seasonality Controls.**

	Log Price
<i>After Shock</i> × <i>Basketball Card</i>	−0.053*** (0.020)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
NBA Season	Yes
MLB Season	Yes
March Madness	Yes
NCAA Season	Yes
Adjusted R <sup>2</sup>	0.638
Observations	958,377

*Notes.* We cluster the standard errors at the seller level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Beyond NCAA March Madness, one can raise more general concerns about potential seasonality effects biasing our results. These are rooted in the difference in sports calendars between basketball and baseball. To econometrically address this, we pursued two strategies. First, we added several control variables (see Table 4). Second, we leveraged an entirely new identification strategy with untreated basketball players as the control group. We do this to deal with lingering concerns of seasonality.

In this strategy, we only use basketball cards to address the remaining concerns of seasonality differences between basketball and baseball seasons. Additionally, we construct the control group via a look-ahead sampling strategy

<sup>1</sup> In both Table 3 and Table 4, the coefficient estimates remain consistent if we include the additional controls presented in Table 2 (Table titled “Main Effects”) of the main paper.

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(Bapna et al. 2018, Jung et al. 2019) by using players who had no moments during our observation period but received moments after our observation window. This makes the treatment and control group more comparable. Finally, we estimate this sample using a novel counterfactual analysis method, namely trajectory balancing (TJBAL), a method that stems from the family of generalized synthetic control groups (Hazlett and Xu 2018). The results of this analysis remain consistent with our main model, suggesting that different sports seasonality is not biasing our results. This analysis is explained in detail in 2.C.1.

Apart from these analyses, we conduct three additional analyses:

First, in 1.D.2, we present the results of our main specification where we replace the eBay 2021 data with eBay data covering the same period but in 2022 (i.e., January 14, 2022 up to March 23, 2022). This period (2022) is ideal to test if seasonality impacts our estimated effect (of 2021). This is because the periods of March Madness, the NBA season, and the MLB season started and finished on similar dates between 2021 and 2022. This means that if our estimated effect in 2021 is driven by sports seasonality, then we expect that, *ceteris paribus*, a similar effect will be estimated in 2022. However, as we see from the results of Table 7, the coefficient estimate is no longer significant. This indicates that our estimated effect in 2021 is driven by a reason other than sports seasonality.

Second, in 1.D.2, we present the results of our main specification with an alternative control group. In short, for this analysis we keep basketball cards from the eBay 2021 data as the treated group but the control group now becomes the basketball cards from the eBay 2022 data. The results of this analysis are presented in Table 8, where the coefficient estimate remains negative and statistically significant. This result serves a dual role: It provides evidence that our results are consistent if we use basketball cards as the control group and it further mitigates concerns over sports seasonality (i.e., NCAA March Madness) biasing our estimates.

Third, in 1.H.1, we investigate whether the focal effect diminishes or reverses over time. For this analysis, we use March 7, 2021 as our placebo shock date and we split the period after the shock into two equal parts: February 19, 2021 up to March 6, 2021 and March 7, 2021 up to March 22, 2021. It is important to note that the second part includes several days of March Madness 2021 (March Madness 2021 began on March 18, 2021). As a result, if March Madness 2021 is the driver of our results, then we would expect that the coefficient estimate of the interaction term would be negative and significant. Nevertheless, in the results of this analysis (seen in Table 15), we find a positive and insignificant coefficient estimate which provides no evidence to support this argument.

Finally, we would like to discuss a concurrent event that took place during our observation window from February 15 - March 8, namely the 2021 Winter Goldin Auction. Although one might be concerned that this event affected the prices of basketball cards and thus acts as a confounder, this is relatively unlikely for several reasons. First, the Winter Golden Auction features both basketball and baseball cards, thus a systematic effect on basketball cards is less likely. Second, the Winter Goldin Auction is a periodical event that took place again in 2022. We analyze the time window in 2022 using the same econometric framework and find no effect (1.D.2), suggesting the Winter Goldin Auction does not drive our results. Third, if the Winter Goldin Auction was the driver behind our results, we would expect the effect on prices to revert after the auction event ended. However, we find that this is not the case (1.H.1). Fourth, the 2021 Winter Goldin Auction featured 2,184 items, whereas our dataset featured 958,377 items. This substantial difference further suggests that it is relatively unlikely that a small set of auction items affected the prices of a much larger set of items.

## B. Extended Time Window Replication

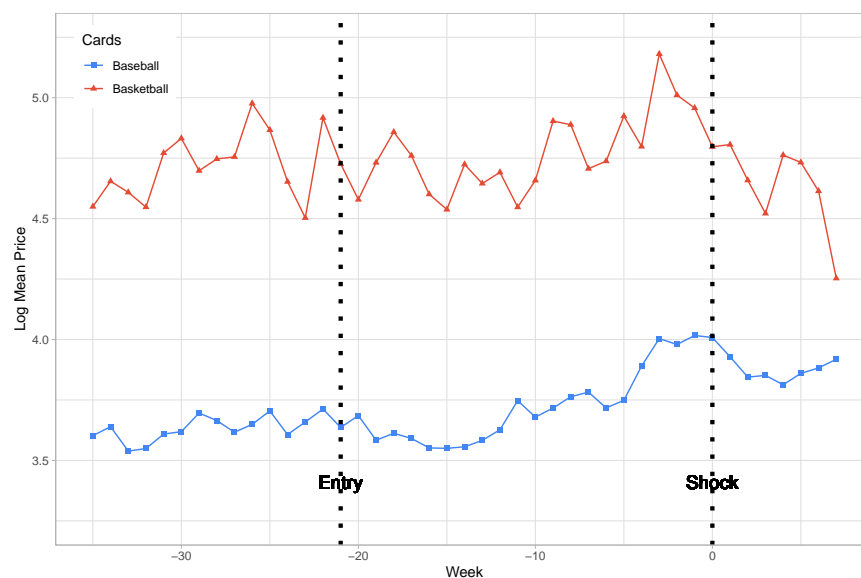
One might argue that our observation window of 69 days of eBay data is too narrow to reasonably interpret our estimates. First, it could be that differences in the NBA and MLB season calendar drive NBA card price decreases after the shock date. Second, it is also possible that the 36-day pre-treatment period is too short to reliably test the parallel trends assumption. Third, a longer time window would allow us to use the entry of NTS as the shock date instead of the shock date informed by public attention – as typically measured by Google Trends data (Ben-Rephael et al. 2017, Da et al. 2011, Liu et al. 2023) – that we use in our main specification. In summary, a longer time window of data would be of great help to substantiate the credibility of our estimates, by allowing us to deal both with suspicions over our choice of the shock date and with concerns over our estimates being biased.

To address this concern, we collected a data set from PWCC<sup>2</sup>, as it offered the advantage that we could collect a longer time window of data than what was possible through eBay. This platform collaborated with eBay to provide the prices of past sold listings on eBay dating back to 2004 through PWCC’s search tool<sup>3</sup>. To collect the new data set, we leveraged PWCC’s search tool using the search terms “nba”, “basketball”, “mlb”, and “baseball”. Although this search tool is no longer available, we present it in 3.B. This strategy allowed us to collect a data set spanning from June 19, 2020, to April 09, 2021. Figure 2 plots the log mean weekly prices of the PWCC data and the dashed vertical lines indicate the entry of NTS and the shock date. As we see there, the prices of the sold basketball cards begin to drop right at the shock date, whereas those of the baseball cards remain relatively unchanged. More importantly, the extended time window of 35 weeks before the shock date clearly shows that NBA and MLB card prices move in parallel before the entry of NTS. This provides further credibility to the parallel trends assumption and rules out effects due to differences in NBA and MLB season calendars. If there were calendar effects, we would expect them to be already visible in the 35 pre-treatment weeks. We further support this by testing the validity of the parallel trends assumption using three tests in 4.B.

We also re-estimated our main model specification from equation (1) with our new PWCC data. One minor restriction is that PWCC does not provide a seller ID, which prevents us from implementing seller-fixed effects and clustering the standard errors at the seller level. To circumvent this restriction, we use the listing titles to construct a product ID for the listings that have been flagged as “ID-Valid” (about 53.11% of the total PWCC observations), and we cluster the standard errors at the card level. The results of this analysis are found in Table 5. In column (1), we use the PWCC data for the same window of observation as our eBay data and in column (2) we use all the PWCC data. In both cases, we do not cluster the standard errors. In columns (3) and (4), we use the same specifications as in columns (1) and (2), respectively, but we analyze only the “ID-Valid” listings, we control for card fixed effects, and we cluster the standard errors at the card level. The results are consistent with the results of Tables 2 and 3 of the main paper, thus supporting our main findings on an extended observation window.

<sup>2</sup> <https://www.pwccmarketplace.com/>

<sup>3</sup> We did not use this source for our main analysis for several reasons. First, the PWCC data does not include seller information rendering it impossible to include seller-fixed effects in the specifications. Second, this data is not as rich in terms of card information which limits our ability to include card-fixed effects for many listings. Third, because the PWCC data is poorer in terms of available information (i.e., seller information, product quality), it prohibits us from running the analyses on product quality and seller reputation. Finally, the search tool returns only the sold listings whose titles contain the searched keyword. Thus, to retrieve all the sold listings, one would need to initiate tens of thousands of http requests (considering the number of players).

**Figure 2 Log Mean Weekly Price - Extended Observation Window.****Table 5 Extended Observation Window.**

	Narrow	Extended	Log Price ID-Valid - Narrow	ID-Valid - Extended
	(1)	(2)	(3)	(4)
<i>After Shock</i> × <i>Basketball Card</i>	-0.077*** (0.007)	-0.186*** (0.005)	-0.135*** (0.018)	-0.140*** (0.024)
Seller Fixed Effects	No	No	No	No
Day Fixed Effects	Yes	Yes	Yes	Yes
Card Fixed Effects	No	No	Yes	Yes
Adjusted R <sup>2</sup>	0.021	0.032	0.392	0.374
Observations	988,995	3,305,135	535,147	1,755,611

*Notes.* In all columns, we analyze the PWCC data. In columns (1) and (3), we analyze the same window of observation as our eBay data (cards sold between January 14, 2021, and March 23, 2021). In columns (2) and (4), we analyze the complete window of observation (cards sold between June 19, 2020, and April 09, 2021). In columns (3) and (4), we analyze only the “ID-Valid” listings, and we cluster the standard errors at the card level. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

The extended observation window of the PWCC data allows us to conduct another analysis, in which we use the entry date of NTS (October 1, 2020) as the shock date. Because NTS started receiving attention around February 19, 2021, we hypothesized that the prices of cards sold before that date would be unaffected by the entry of NTS<sup>4</sup>. As a result, if the observations before February 19, 2021 are included in the post-shock period (which will be the case if we use the entry of NTS as the shock date), then we expect the coefficient estimate of the interaction term to be diluted, meaning that it will be smaller in magnitude and/or weaker in terms of significance. To conduct this analysis, we use the DiD specification of column (4) of Table 5 but use the entry date of NTS as the shock date; we present the results in Table 6. Consistent with our expectation, we find that the interaction term is negative and significant only at the

<sup>4</sup> In fact, this is supported in all our data sets by all the robustness checks and placebo tests.

10% level. This indicates that the market players did not react (as reflected by changes in the card prices) in the first few months after the entry of NTS.

**Table 6 Entry Date (Only ID-Valid Listings).**

	Log Price
<i>After Shock</i> × <i>Basketball Card</i>	−0.034* (0.020)
Seller Fixed Effects	No
Day Fixed Effects	Yes
Card Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.374
Observations	1,755,611

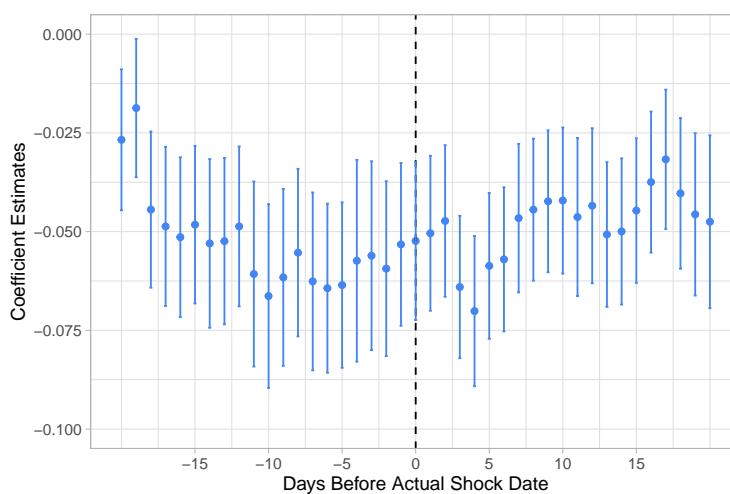
*Notes.* We analyze the PWCC “ID-Valid” listings for the complete window of observation, using the entry date of NTS (October 1, 2021) as the shock date, and we cluster the standard errors at the card level. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

### C. Shock Date Selection

One could argue that if we changed the shock date to before or after our chosen shock date (February 19, 2021), our main results might become insignificant. We test this by rerunning specification (3) of Table 2 of the main paper. We rerun it 40 times and in each iteration, we change the shock date to a date ranging from 20 days before to 20 days after February 19, 2021. After each iteration, we extract the coefficient estimate and standard error of the interaction term and produce the graph of Figure 3. As we see, the estimate of the interaction term remains relatively similar irrespective of the chosen shock date, thus alleviating this concern.

**Figure 3 Shock Date Selection - Moving The Shock Date.**



## D. Baseball Cards Valid Counterfactual to Basketball Cards

### D.1. Theoretical Validity

We use baseball cards as the control group because we believe they are the sports trading cards most comparable to basketball cards. First, baseball cards are similar in popularity and trade volume. Second, the results are unlikely to be affected by sports seasonalities, as the MLB regular season started in April 1, 2021<sup>5</sup>, and the NBA regular season was underway during our observation window<sup>6</sup>. A further potential concern is that the NBA differs from the MLB in that a larger percentage of its audience resides outside of the US, which could affect the card prices of the sports trading cards. To address this concern, we adapted our data extraction script<sup>7</sup> to retrieve only those sold sports trading cards that could have been shipped to the US.

Next, we focus on providing further evidence that these sport-related differences do not drive our main finding.

### D.2. Sports Seasonality

First, we implement a test related to seasonality effects, which refer to major sports calendar events – such as the start of a sports season – which are usually associated with changes in market prices. Although we argued that season-start effects are unlikely to drive our estimates, there may still be lingering concerns about other major sports calendar events filling that role. We alleviate these concerns with the following robustness check. We argue that if the estimated effect on basketball card prices is driven by differences in the sports seasonality of the two sports, then we expect that, *ceteris paribus*, a similar effect will be estimated for any year prior to 2021 – apart from 2020. Because we do not have data for prior years, we collected data for 2022 – described in 1.H.2 – to fill this role. We use this data<sup>8</sup> to run specification (3) of Table 2 of the main paper and we present the results in Table 7. As we see, the estimated coefficient is insignificant, which supports the argument that the observed effect in our main results (2021 data) is unlikely to be driven by seasonality effects.

**Table 7 Sports Seasonality - 2022 eBay Data.**

	Log Price
<i>After Shock</i> × <i>Basketball Card</i>	−0.026 (0.018)
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.475
Observations	651,972

*Notes.* We cluster the standard errors at the seller level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

We further add on this through another analysis where we use an alternative control group, namely basketball cards. This analysis serves two purposes as it mitigates concerns over biases introduced by using baseball cards as control group and it addresses lingering concerns on sports seasonality biasing our estimates.

<sup>5</sup> [https://www.espn.com/mlb/story/\\_/id/29435023/all-30-teams-open-april-1-2021-mlb-schedule](https://www.espn.com/mlb/story/_/id/29435023/all-30-teams-open-april-1-2021-mlb-schedule)

<sup>6</sup> <https://www.nba.com/news/nba-announces-structure-and-format-for-2020-21-season>

<sup>7</sup> We did so by passing a specific value to a parameter of the http requests. Since then, eBay's layout has changed and our web scraper is no longer operational.

<sup>8</sup> Before using this data we tested the validity of the parallel trends assumption using a placebo test and a fully flexible DiD model with treatment lags and leads (The results are available in 4.C).

For this analysis, we create a new sample that consists of the eBay 2021 basketball cards and the eBay 2022 basketball cards, with the 2021 part being the treated group and the 2022 part being the control group. By using eBay basketball cards for both groups and since the sports calendar is reasonably similar every year, we can cancel out sports seasonality effects and tackle concerns about the validity of the control group. Currently, this technique is being widely embraced in economics as well as IS literature (Eichenbaum et al. 2024, Geva et al. 2024, Quinn and Gutt 2025). We use February 19 as the shock date for both groups (February 19, 2021 for the basketball cards sold in 2021 and February 19, 2022 for the basketball cards sold in 2022). The results of this analysis are presented in Table 8 that follows and as we see the coefficient estimate remains consistent with our main results. The key takeaways are twofold: First, our results are unlikely to be an artifact of our baseball card control group, as we see consistent estimates when using basketball cards as the control group. Second, the co-occurrence of sports events (e.g., NCAA March Madness) around the shock date is unlikely to bias our estimates, as it cancels out in this framework – it happens to both the treatment and the control group.

**Table 8 Basketball Only.**

	Log Price
<i>After Shock</i> × <i>Year</i> 2021	−0.046** (0.023)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.585
Observations	502,807

*Notes.* We cluster the standard errors at the seller level. Observation window: January 29 up to March 23 (2021 and 2022).

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

We also test the robustness of the parallel trends assumption for this combined data using the three tests that we have applied in our other data sets. This is vitally important for this particular analysis because one could be concerned that the NTS effect on basketball cards in 2021 might dynamically carry over to the 2022 basketball card control group. If this was the case, we would expect the parallel trends assumption to be violated. To do so, we use the same three tests that we present in Appendix 4 and we find no evidence to support that the parallel trends assumption is violated. The results are available in Appendix 4.D.

### D.3. Matching

Second, our baseline results have established that sold prices decrease for the treatment group. Nevertheless, some might be concerned that the cards are so different that it is difficult to draw conclusions from our sample. To mitigate such concerns, we apply matching to obtain a sample of basketball and baseball cards that are as similar as possible, conditional on observables. We implement matching in two ways, and in both cases, we analyze the “ID-Valid” listings as we make use of the product IDs. First, we implement matching on all the unique product IDs that were sold before the shock. We aggregate our data per product ID; for each ID, we estimate the mean grade, the percentage of its listings that were sold through the “Buy It Now” format, whether the card contains a Hall-of-Famer, the mean feedback score of its sellers, the percentage of its listings sold during the weekend, and its total number of entries. To

implement the matching, we use logistic regression to calculate propensity scores on these covariates and then nearest-neighbor matching<sup>9</sup> to build our matched sample. We assess the matching balance in Table 9, where the standard mean differences of all the covariates are less than 0.1, indicating that we have a balanced sample. This is a substantial improvement over the unmatched sample shown in Table 10.

**Table 9 Matched Sample - Propensity Score Matching - Balance Assessment.**

Variable	Baseball	Basketball	S.M.D.
N	8938	8938	-
Mean Grade	8.54 (1.60)	8.50 (1.60)	0.026
Buy It Now (%)	0.73 (0.34)	0.73 (0.35)	0.012
HoFer	0.21 (0.41)	0.21 (0.41)	0.015
Mean Seller Feedback Score	156330.33 (146189.64)	164803.23 (142911.37)	0.059
Weekend Sale (%)	0.32 (0.37)	0.32 (0.36)	0.009
Number of Entries	12.30 (113.19)	21.71 (206.25)	0.057

*Notes.* The variables are aggregated at the product ID level prior to matching. S.M.D.: Standardized Mean Difference. HoFer: Hall-of-Famer.

**Table 10 Unmatched Sample - Balance Assessment.**

Variable	Baseball	Basketball	S.M.D.
N	30971	8938	-
Mean Grade	7.19 (2.46)	8.50 (1.60)	0.631
Buy It Now (%)	0.70 (0.36)	0.73 (0.35)	0.090
HoFer	0.16 (0.36)	0.21 (0.41)	0.146
Mean Seller Feedback Score	138349.43 (142916.68)	164803.23 (142911.37)	0.185
Weekend Sale (%)	0.35 (0.37)	0.32 (0.36)	0.070
Number of Entries	6.83 (62.73)	21.71 (206.25)	0.098

*Notes.* The variables are aggregated at the product ID level prior to matching. S.M.D.: Standardized Mean Difference. HoFer: Hall-of-Famer.

After our matched sample is created, we subset our original data to include all the observations of only the matched product IDs. Using this matching method, we drop about 25% of our observations. This can raise concerns that biases due to data stratification might influence our estimates. Thus, we implement another form of matching, in which we keep a great number of observations. The second way that we implement matching is by subsetting our pre-shock data to include only the product IDs that are present after the shock. We use these two matched samples to run specification (3) of Table 2 of the main paper inclusive card-fixed effects and present the results in Table 11. As we see, the coefficient estimates remain very similar to those of specification (3) of Table 2 of the main paper, thereby increasing our confidence in the validity of our identification strategy.

<sup>9</sup> We also attempted Coarsened Exact Matching but Propensity Score Matching resulted in a better covariate balance.

**Table 11 Matching (Only ID-Valid Listings).**

	Log Price	
	(1)	(2)
<i>After Shock</i> × <i>Basketball Card</i>	-0.058*** (0.021)	-0.066*** (0.025)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Card Fixed Effects	Yes	Yes
Adjusted R <sup>2</sup>	0.721	0.747
Observations	606,801	818,431

*Notes.* In both specifications, we analyze only the “ID-Valid” listings, and we cluster the standard errors at the seller level. In column (1), we analyze the “ID-Valid” listings matched using propensity score matching. In column (2), we first identify the product IDs sold after the shock date and we then analyze only the “ID-Valid” listings of those product IDs for our complete observation window. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## E. Baseball Card Prices unaffected by NFTs

There is also the possibility that baseball card prices are affected by the entry of NTS. This would constitute a violation of the Stable Unit Treatment Value Assumption (Angrist and Pischke 2009). Figure 2 shows that after the shock date, the prices of baseball cards experienced a slight decline, unlike the sharp decline of the basketball card prices. This indicates that baseball card prices were not affected by the entry of NTS, or that they were affected to a lesser degree.

To gather evidence on whether baseball cards were affected, we conduct two analyses. In the first, we use the subsample of baseball cards and apply a specification similar to specification (3) of Table 2 of the main paper; we regress baseball log prices on whether cards were sold after the shock date while controlling for seller-fixed effects and day-fixed effects, and we cluster the standard errors at the seller level. The estimate is shown in Table 12 and it indicates that the prices of sold baseball cards were not significantly affected by the entry of NTS.

**Table 12 Baseball Listings’ Analysis.**

	Log Price
After Shock	0.016 (0.023)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.652
Observations	538,878

*Notes.* We cluster the standard errors at the seller level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 13 Baseball Listings 2021 vs 2022.**

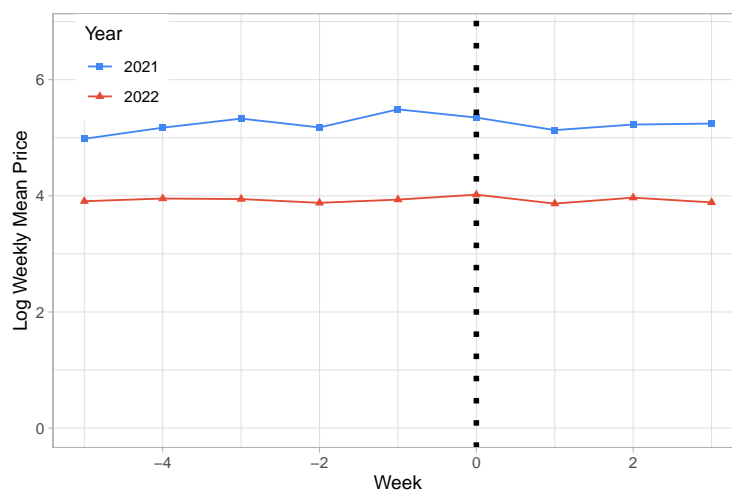
	Log Price
After Shock	-0.003 (0.011)
Sold 2021	0.554*** (0.073)
<i>After Shock</i> × <i>Sold 2021</i>	0.034 (0.024)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.594
Observations	983,614

*Notes.* We use baseball cards sold in 2021 as the treatment group and baseball cards sold in 2022 as the control group, while we cluster the standard errors at the seller level. After Shock: Listings sold after February 18.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

In the second analysis, we aim to corroborate these results in a more rigorous manner using the 2021 and 2022 eBay data of the sold baseball cards. We plot the prices of these two year groups in Figure 4, where we see both lines remain relatively stable around the shock date and throughout the whole observation window. The only notable difference is that the 2022 line is lower than that of 2021. This could be driven by several factors such as individual users moving their funds away from physical collectible investments. To conduct the second analysis, we follow a novel DiD strategy proposed by Eichenbaum et al. (2024). This approach is based on analyzing only the baseball cards using both the 2021 and the 2022 eBay data, with 2021 being the treatment group and 2022 being the control group. We apply a specification similar to specification (3) of Table 2 of the main paper and use *Sold 2021* as the treatment indicator, *After Shock* as the shock date indicator, and the interaction of the two as the DiD estimator. We posit that if the entry of NTS affected the prices of baseball cards in 2021, then, ceteris paribus, this would be reflected by a significant coefficient estimate of the interaction term. We present the results in Table 13, where we see that the estimated coefficient of the interaction term is insignificant. This provides further evidence that baseball card prices in 2021 have not been significantly affected by the entry of NTS.

**Figure 4** Log Weekly Mean Price of Sold Baseball Auctions (The dotted line indicates the shock week).



One might wonder what influence baseball NFTs have on baseball card prices. Up to 2021, no baseball NFTs had received as much attention as NTS. The first attempt to create baseball NFTs was in 2018, with “MLB Crypto Baseball”, but this attempt did not receive much support<sup>10</sup> and became defunct by 2020<sup>11,12</sup>. Further attempts to offer baseball NFTs include those of Topps<sup>13</sup>. Topps announced the release of its first set (Series 1) of baseball NFTs on April 12, 2021. Because these were announced after our observation window, their release should have no impact on the prices in our sample.

<sup>10</sup> For the platform daily sales number, we refer the reader to <https://nonfungible.com/market-tracker/mlbchampions>.

<sup>11</sup> Short report available in <https://morningconsult.com/2021/03/19/mlb-champions-nft-crypto/>.

<sup>12</sup> <https://www.engadget.com/topps-mlb-baseball-card-nft-collection-130630367.html>

<sup>13</sup> <https://www.topps.com/blog/topps-debuts-its-first-mlb-baseball-card-nft-collection-with-topps-series-1-baseball-launch-.html>

Topps NFTs are static cards and not moments like the NFTs of NTS. This attempt was met with more success than “MLB Crypto Baseball”, and Topps released its second MLB NFT product on October 4, 2021<sup>14</sup>. Nonetheless, compared to NTS, Topps’ NFTs have not had as much success in terms of sales<sup>15</sup> or social media followers<sup>16</sup>. This may be related to key user experience differences between the two platforms. One of these differences is that NTS allows its users to transact with US\$, whereas the users of Topps must transact with cryptocurrencies, thereby creating a barrier to entry for potential users who are inexperienced – or uncomfortable – with cryptocurrencies. Another key difference that may discourage less tech-savvy collectors from using the Topps platform is that users of NTS can trade NFTs directly with other users on the platform, whereas users of Topps must use third-party websites<sup>17</sup> to do so.

## **F. Randomized Treatment Test**

One might be concerned that our results are driven by serial correlation between the logarithmic prices and unobserved factors. To alleviate such concerns, we run a randomized treatment test (Alyakoob and Rahman 2022, Bertrand et al. 2004). For this test, we randomly allocate observations to either a treated (i.e., basketball) or untreated (i.e., baseball) condition, while keeping the percentage of treated observations equal to that of our data (about 43.77%). We then use the updated treatment allocation and run specification (3) of Table 2 of the main paper; we repeat this process for 1000 iterations. With each iteration, we extract the coefficient estimate of the interaction term, thereby producing the distribution of Figure 5. As we can see, this distribution is centered around 0 (mean = -0.0001, standard deviation = 0.005), indicating that if the observations were randomly allocated to a treatment condition, the impact of NFT entry on the prices of the “treated” observations would be very close to 0. We further support this statistically by testing whether the mean of the distribution is statistically different from zero ( $p = 0.53$ ). Lastly, using this distribution, we estimate that the probability of obtaining a coefficient equal to that of specification (3) (-5.2%) is less than 0.0001<sup>18</sup>. Thus, we find no evidence that our results are driven by serial correlation between the logarithmic prices and unobserved factors.

## **G. Excluding Cards Listed Before and Sold after the Shock Date**

Listings on eBay have a fixed duration, which is set by the seller. In addition, unsold listings are automatically relisted by default, unless the seller chooses otherwise<sup>19</sup>. Thus, our sample may contain items that were listed before the shock date but sold after the shock date. Although we assumed that this was not the case, we relax this assumption and alleviate related concerns in this robustness check by running specification (3) of Table 2 of the main paper, while excluding observations close to the shock date. Instead, we include only listings sold within either the first two weeks (January 14, 2021 up to January 27, 2021) or the last two weeks of our observation window (March 10, 2021 up to March 23, 2021). We present the result of this analysis in Table 14. As we can see, the estimate of the interaction term is similar to that of our main analysis, thus strengthening the robustness of our results.

<sup>14</sup> <https://www.topps.com/blog/topps-unveils-2021-topps-series-2-baseball-nft-collection.html>

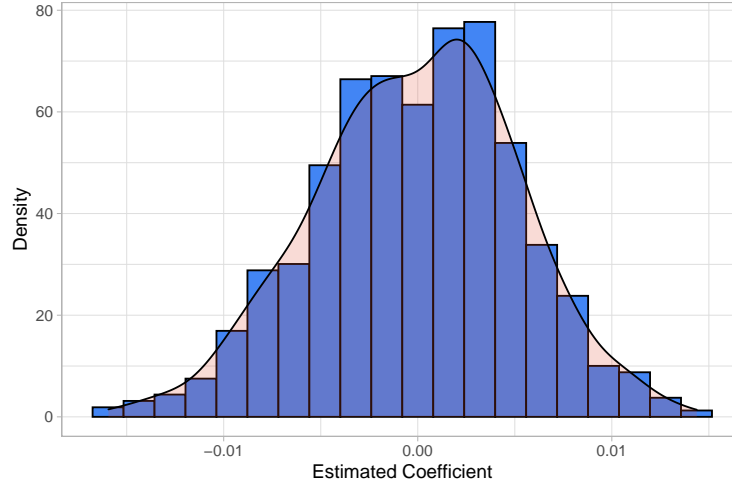
<sup>15</sup> In April 2022, the NFT sales volume of NTS was about US\$ 25 million, whereas that of Topps was about US\$ 86 thousand. See <https://cryptoslam.io/topps-mlb/sales/summary> and <https://cryptoslam.io/nba-top-shot/sales/summary> for the respective data.

<sup>16</sup> As of May 21, 2022, NTS has 141 thousand followers on Instagram and 290 thousand on Twitter, whereas Topps has 5.5 thousand and 14.3 thousand, respectively.

<sup>17</sup> <https://www.onlinegambling.com/news/sports-nfts-comparing-nba-top-shot-to-topps-mlb-collectibles/>

<sup>18</sup> To estimate this probability, we assume that Figure 5 follows a normal distribution. We support this assumption using the Kolmogorov–Smirnov test ( $p = 0.21$ ).

<sup>19</sup> <https://www.ebay.com/help/selling/listings/creating-managing-listings/relisting-items?id=4147>

**Figure 5 Randomized Treatment Test – Distribution of Coefficient Estimates.****Table 14 First Two and Last Two Weeks.**

	Log Price
<i>After Shock</i> × <i>Basketball Card</i>	-0.071** (0.031)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.648
Observations	387,102

*Notes.* We cluster the standard errors at the seller level. Only observations sold within either the first two (January 14, 2021 up to January 27, 2021) or the last two weeks of our observation window (March 10, 2021 up to March 23, 2021) are included.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## H. Effect Diminishment or Reversal

### H.1. Short Term

We further explore whether the focal effect diminishes or reverses over time. Although our identification strategy estimates a significantly negative effect of the entry of NTS on the log prices of cards, one could argue that this estimate is driven by the reaction of the players (sellers and buyers) during the first few weeks after the shock date. Following this line of thought, it is possible that this (negative) effect might become insignificant over time. Because our eBay data does not span several months before and after the shock date, we cannot test for this. However, we can test whether there is any evidence to indicate that such an effect begins to occur during our observational window.

To test for the presence of any such evidence, we run a placebo test using specification (3) of Table 2 of the main paper, where we analyze only post-shock-date observations. We use March 7, 2021 as our placebo shock date because it allows us to split the analyzed time window into two equal parts: February 19, 2021 to March 6, 2021 and March 7, 2021 to March 22, 2021 (16 days each). If the estimate of our main effect is indeed driven by the first few weeks of activity and reverses over time, then in this test we would expect the interaction term to have a significant and positive

coefficient estimate; this would mean that the estimated main (negative) effect diminishes over time. However, because the estimate of Table 15 does not rise to the level of statistical significance, it provides no evidence to indicate that the estimated main effect diminishes during our observational window (i.e., in the short term).

## **H.2. Long Term**

It is also important to investigate whether the observed effect diminishes or reverses in the long term. To do so, we collected another eBay data set of sold basketball and baseball card listings. This data set covers the period between January 14, 2022, and March 23, 2022, which is the same time frame as we have for our baseline analysis, albeit one year later. This data was collected in a different way<sup>20</sup>, in which we identified all the sellers of our original eBay data, tracked their complete sales history, and extracted the product page information for every listing that they sold within a period of three months<sup>21</sup>. By collecting the sales history of the same set of sellers, we were able to create a homogeneous eBay data set for 2022. This new data includes 211,048 basketball and 450,382 baseball sports trading cards.

To estimate whether the main effect diminishes or reverts in the long term, we use specification (3) of Table 2 of the main paper. We apply this DiD specification on the combined cards sold between February 19, 2022 and March 23, 2022 and those sold between January 14, 2021 and February 18, 2021. This analysis allows us to compare the difference in the prices of basketball cards between the two periods against the difference in the prices of baseball cards between the two periods. By choosing the same time frame for both years, we seek to obviate seasonal effects that might affect the treatment group. If the main effect has reverted in the long term, then we would expect our estimated interaction term coefficient to be either insignificant or significant and positive. The results of this analysis are presented in column (1) of Table 16, where we see that the estimated coefficient is significant and negative.

One might argue that numerous sellers who were present in 2021 may not have been present in 2022, and that this could impact our estimates. To alleviate such concerns, we replicate this analysis; but, we analyze only the listings of sellers who were present in 2022. This means that from the listings sold before February 19, 2021, we discard those listings whose sellers were not present in 2022. We define a seller as being present in 2022 if they sold at least one card between February 19, 2022, and March 23, 2022. The estimated coefficient can be seen in column (2) of Table 16 and, once again, it is significant and negative. These findings do not support the argument that the main effect diminishes or reverts in the long term. The collective evidence does not support that the price decrease reverts after one month or one year.

## **I. Sellers Leaving The Market**

One might be concerned that our focal effect is driven by reputable sellers leaving the market. This concern is tested in column (2) of Table 16, where we kept only those sellers who were present about a year after the entry of NTS. We further test this by analyzing only the 2021 eBay data and focusing on the listings sold by sellers who were present after the entry of NTS (between February 19, 2021 and March 23, 2021). We present the results in Table 17. The estimated coefficient remains consistent, thus alleviating concerns that our focal effect is driven by reputable sellers leaving the market.

<sup>20</sup> Since collecting our 2021 eBay data, the layout of the eBay website was changed and we were no longer able to directly collect the URLs from the product pages of all the sold listings in the same way. Therefore, to collect new data, we had to resort to different methods.

<sup>21</sup> It was infeasible to retrieve the product page information for listings that were sold more than three months prior because these product pages were no longer available.

**Table 15 Main Effect Diminishment Analysis - Short Term.**

	Log Price
<i>After Placebo</i> × <i>Basketball Card</i>	0.026 (0.029)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.646
Observations	489,869

*Notes.* We cluster the standard errors at the seller level. Placebo shock date: March 07, 2021. Only observations after February 18, 2021 were used.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 16 Main Effect Diminishment Analysis - Long Term.**

	Log Price	
	All (1)	After Placebo (2)
<i>After Placebo</i> × <i>Basketball Card</i>	-0.498*** (0.060)	-0.569*** (0.074)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Adjusted R <sup>2</sup>	0.612	0.569
Observations	743,625	477,094

*Notes.* “After Placebo” refers to the observations after February 18, 2022, while the rest are the observations before February 19, 2021. In column (1), we analyze all of the listings. In column (2), we analyze all of the listings of sellers who were present in the “After Placebo” period. In both specifications, we cluster the standard errors at the seller level..

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 17 Only 2021 After Shock Sellers.**

	Log Price
<i>After Placebo</i> × <i>Basketball Card</i>	-0.068*** (0.025)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.642
Observations	681,249

*Notes.* We analyze all the listings of the sellers who were present after February 18, 2021, and we cluster the standard errors at the seller level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## J. Outlier Analysis

One might also be concerned that our focal effect is driven by outliers. To gather evidence on this, we remove the top 1%, 2.5%, and 5% of the sold price distributions per sport. In our case, these outliers would be the most expensive cards which are the high-quality cards of popular players.

From the market expansion analysis that was presented in Table 11 of our main paper, we found that while the prices of some card groups were statistically significantly impacted by the NTS shock, the prices of high-quality cards of popular players were not. Hence, according to our theory and prior findings, we would expect that removing outliers would, if anything, increase the absolute magnitude of our estimates. To identify the outliers we aggregate observations on the card player level. Then, we conduct three outlier analyses where we tag as outliers the top 1%, 2.5%, and 5% of the sold price distributions per sport. We rerun our main specification (specification (2) of Table 2 of the main paper) and we present the results in Table 18 that follows. In each of the columns (1), (2), and (3), we exclude the top 1%, 2.5%, and 5% of the outliers respectively. As we see, corroborating our intuition, the coefficient estimates remain significant but become larger in magnitude compared to our main results in Table 2 of our manuscript. In Table 19 below, we rerun this analysis but identify outliers on the card level and we arrive at similar findings. These results alleviate concerns over outliers driving our focal effect.

**Table 18 Main Effect - Excluding Outliers - Players.**

	Log Price		
	Excl. Top 1%	Excl. Top 2.5%	Excl. Top 5%
	(1)	(2)	(3)
<i>After Shock</i> × <i>Basketball Card</i>	-0.110*** (0.025)	-0.144*** (0.028)	-0.130*** (0.030)
Seller Fixed Effects	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes
Card Fixed Effects	Yes	Yes	Yes
Adjusted R <sup>2</sup>	0.759	0.759	0.763
Observations	752,192	598,538	479,325

*Notes.* In all specifications, we analyze only the “ID-Valid” listings and we cluster the standard errors at the seller level. We identify outliers on the player level. In column (1) we remove the top 1% of the outliers, in column (2) we remove the top 2.5% of the outliers, and in column (3) we remove the top 5% of the outliers. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 19 Main Effect - Excluding Outliers - Cards.**

	Log Price		
	Excl. Top 1%	Excl. Top 2.5%	Excl. Top 5%
	(1)	(2)	(3)
<i>After Shock</i> × <i>Basketball Card</i>	-0.093*** (0.026)	-0.093*** (0.026)	-0.098*** (0.029)
Seller Fixed Effects	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes
Card Fixed Effects	Yes	Yes	Yes
Adjusted R <sup>2</sup>	0.737	0.736	0.741
Observations	846,693	751,040	604,337

*Notes.* In all specifications, we analyze only the “ID-Valid” listings and we cluster the standard errors at the seller level. We identify outliers on the card level. In column (1) we remove the top 1% of the outliers, in column (2) we remove the top 2.5% of the outliers, and in column (3) we remove the top 5% of the outliers. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## Appendix. 2. Listing Format & Moderators

### A. Listing Format Sample Differences

The estimated coefficients of Table 4 of the main paper indicate that buyers' WTP drops by about 10%, while sellers' RPs drop by about 5%. These differences could be estimated if the two samples represent different kinds of cards. For example, sellers might be more inclined to choose the "Auction" format for listings that are usually sold at higher prices (e.g., cards featuring HoFers, or higher-quality cards)<sup>22</sup>. To identify whether this is the case, we regress the binary indicator of whether a card refers to a HoFer on a dummy indicating whether a card was sold after the shock date, a dummy indicating whether a card was a basketball card, an interaction between the two, and a dummy indicating the listing format. We further control for seller-fixed effects, day-fixed effects, and card-fixed effects, while we cluster the standard errors at the seller level. We repeat this analysis while using the binary indicator of whether a card is of high quality as dependent variable. The results of the two analyses are presented in columns (1) and (2) of Table 20, respectively. The insignificant coefficient estimates of "Buy It Now" indicate that the cards sold under the two listing formats do not represent inherently different samples, thus mitigating this concern.

**Table 20 Listing Formats - Sample Difference Analysis (Only ID-Valid Listings).**

	Probability of Card Being	
	HoF	Grade $\geq 9$
	(1)	(2)
Buy It Now	0.0001 (0.0002)	0.017 (0.015)
<i>After Shock</i> $\times$ <i>Basketball Card</i>	0.0003 (0.0002)	-0.008 (0.006)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Card Fixed Effects	Yes	Yes
Adjusted R <sup>2</sup>	0.996	0.589
Observations	834,874	834,874

*Notes.* In both specifications, we analyze only the "ID-Valid" listings, and we cluster the standard errors at the seller level. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

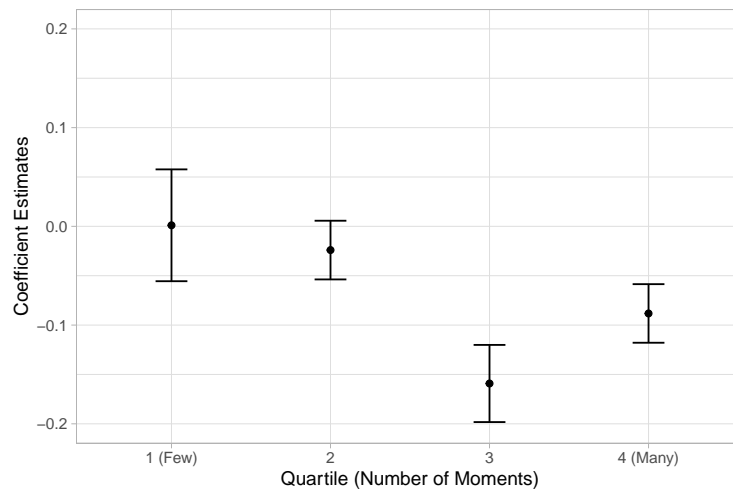
### B. Alternative Moderator Definition

In our analysis of digital scarcity (Table 7 of the main paper), we have categorized basketball players into those with many NFTs and those with few NFTs using a median split. We would be interested to use a continuous metric of player moments and study the same effect. This could be achieved by enriching our analysis with a three-way interaction

<sup>22</sup> To provide an indication of how these prices differ, we made the following estimates of the mean raw sold prices: For all cards referring to active players, the mean raw sold price is about US\$ 370.2; for all cards referring to inactive HoFers, it is about US\$ 539.2; and for all cards referring to inactive non-HoFers, it is about US\$ 75.47. For these estimates, we consider only the "ID-Valid" listings that were sold before the shock date. Similarly, the estimate for all high-quality cards is about US\$ 363.23, while for the rest of the cards it is about US\$ 313.74. For these estimates, we consider only the listings that were sold before the shock date.

term between *After Shock*, *Basketball Card*, and the total number of moments available for a player. However, none of the baseball cards (control group) have any moments available, thus prohibiting this analysis. To categorize basketball players into more groups, we use their number of moments to split them into quartiles. We run our specification inclusive card-fixed effects for the “ID-Valid” listings four times. Each time, we use the listings belonging to the active basketball players of a different quartile (1–4) as the treatment group and the baseball listings as the control group. We present the results graphically in Figure 6. As we see, the estimated coefficients are not different from 0 only for the cards featuring players with fewer NFTs (i.e., quartiles 1 and 2). This further supports our finding that cards featuring players with many NFTs are more negatively affected than those featuring players with few NFTs.

**Figure 6** Product Scarcity - Number of Moment Quartiles.



In column (2) of Table 8 of the main paper, we presented how cards that have received an official grade of less than 9 are affected by the entry of NFTs. Nevertheless, for that analysis we had replaced missing grade values with the value number 0. We replicate that analysis by excluding the observations with missing grade values and present the results in Table 21. The coefficient estimate and its significance remain consistent.

**Table 21** Product Quality - Discarded Observations with Missing Grades.

	Log Price Grade < 9
<i>After Shock</i> × <i>Basketball Card</i>	0.053 (0.061)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.582
Observations	246,391

*Notes.* We cluster the standard errors at the seller level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 22 Product Quality - Three-Way Interaction.**

	Log Price	
	All	ID-Valid
	(1)	(2)
<i>After Shock</i> × <i>Basketball Card</i> × <i>HighQuality</i>	-0.109** (0.053)	-0.124*** (0.026)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Card Fixed Effects	No	Yes
Adjusted R <sup>2</sup>	0.640	0.766
Observations	958,377	834,874

*Notes.* In column (1), we analyze all the listings. In columns (2), we analyze only the “ID-Valid” listings. In both specifications, we cluster the standard errors at the seller level. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

In our analyses of physical quality, we split our sample into cards with a grade equal to or higher than 9 and cards with a grade lower than 9. In the robustness check that follows, we discard this binary split. Instead, we enrich our specification by adding a three-way interaction term between *After Shock*, *Basketball Card*, and the card grade. In Table 22, we present the results of this analysis for (1) all listings and (2) “ID-Valid” listings in columns (1) and (2), respectively. For the last specification, we add card-fixed effects. As we see, the results support our finding that cards of higher quality face greater price declines compared to lower-quality cards.

In our analyses of seller reputation, we have categorized sellers as either reputable or non-reputable using a median split. In the following robustness check, we analyze seller reputation by adding a three-way interaction term between *After Shock*, *Basketball Card*, and the seller reputation score to our main specification. The results of this analysis are presented in Table 23. Its columns refer to (1) all listings and (2) “ID-Valid” listings, while in the last specification we include card-fixed effects. As we see, the three-way interaction term coefficient estimate is negative across both specifications, lending support to our finding that the listings of reputable sellers are affected more than those of non-reputable sellers.

### C. Endogeneity and Selection Issues - Alternative Identification Strategies

In this section of the Online Appendix, we present alternative identification strategies aimed at dealing with endogeneity and selection issues. In what follows, we present alternative identification strategies for analyzing product choice, product scarcity, product quality, seller reputation, and the market expansion effect.

#### C.1. Product Choice - Alternative Identification Strategy

For this identification strategy, we use trajectory balancing (TJBAL) (Hazlett and Xu 2018) because it mitigates biases due to time-variant unobservables, such as time-varying player popularity. To achieve this, TJBAL adjusts the control units’ weights so that the averages of the pre-treatment outcomes and covariates are nearly identical between the treatment group and the reweighted control group. This approach is similar to the synthetic control (SC) method (Abadie et al. 2010) in that it aims to balance the treatment and control groups on the pre-treatment outcomes and covariates. Similar to the SC method, TJBAL treats treatment observations after the treatment as missing and predicts their outcomes using untreated observations. The difference between the predicted counterfactuals and the actual

**Table 23 Seller Reputation - Three-Way Interaction.**

	Log Price	
	All (1)	ID-Valid (2)
<i>After Shock</i> × <i>Basketball Card</i> × <i>Seller Feedback Score</i>	−0.0002*** (0.0001)	−0.0004*** (0.0001)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Card Fixed Effects	No	Yes
Adjusted R <sup>2</sup>	0.639	0.754
Observations	958,377	834,874

*Notes.* In column (1), we analyze all the listings. In columns (2), we analyze only the “ID-Valid” listings. In both specifications, we cluster the standard errors at the seller level. The seller feedback score is divided by 1000 to provide clearer coefficient estimates. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

treatment observations yields the average treatment effect on the treated. However TJBAL differs in that it allows for “approximate balance” by aiming to balance on higher-order features of the covariates (the principal components), instead of the covariates, when predicting the counterfactuals. Besides this, another advantage of TJBAL is that it operates without a large number of pre-treatment periods (which is the case with SC), making it suitable for our data set (Hazlett and Xu 2018). In addition, considering that matching methods such as propensity score matching can substantially reduce selection bias (Smith and Telang 2009) but only account for time-constant unobservables, TJBAL lends itself as a more appropriate alternative identification strategy, as it mimics matching but accounts for time-varying unobservables, as explained above.

To further safeguard against control group concerns and selection effects, we complement the TJBAL approach with two additional techniques. First, we only keep the cards related to basketball players. We do this because one can raise concerns over the validity of baseball cards as the control group due to differences in the seasonality between the two sports. Second, we construct the control group via a look-ahead sampling approach (Bapna et al. 2018, Jung et al. 2019). This approach exploits the fact that not all players received their first moment at the same time, as the rollout of moments was staggered across time. Exploiting this setup, in the control group we use only the cards of those players who received their first moment after the shock date (we also test another setup where in the control group we use only the cards of those players who received their first moment after our observation window - more on this in the paragraphs that follow). Doing so allows us to compare the cards of players who have a similar propensity to have moments - albeit some of the players receive their moments later - thus mitigating selection concerns in our treatment effect estimation (as the product and timing selection decisions can make the estimated price coefficient biased). Third, we only keep the cards of active players and discard those of inactive players to alleviate concerns that cards of active players are systematically different to cards of inactive players. All in all, our combination of TJBAL with a look-ahead sampling strategy of basketball cards, allows us to alleviate concerns over (1) the focal estimate being a seasonal effect and (2) our focal estimate being driven by an incompatible control group, while (3) it allows us to test the robustness of our estimate through an alternative identification strategy.

Using this sample, we proceed with TJBAL. To use TJBAL, we need to transform our data and construct a balanced panel data set of every sold card. We should note that if a copy of a sold card was not sold on a specific date, then

we have to remove all the observations of this card from our panel data set (i.e., remove the time series of this card). It follows logically, that the more granular our time dimension, the more cards we will need to (completely) remove from our panel data set. Therefore, to avoid data sparsity, we opted to create a balanced panel dataset of every sold card that includes their sales information aggregated on a biweekly basis for the period ranging from January 14, 2021 up to March 31, 2021. As this method does not allow us to keep our complete data set, we do not use it for our main estimates, but instead as a robustness check.

After constructing the panel data set, we assign the treatment and control observations. For both groups, we find each card's average sold price for each two-week period and we discard the cards with (any) missing observations. For the treatment group, we chose players who received their first moments during our pre-shock window that is adjusted for the biweekly panel data (i.e., January 14, 2021 up to February 14, 2021). When implementing TJBAL, one needs to define the period (and as an extension the observations) during which each treated unit was under control (i.e., not yet treated) and under treatment. Following the strategy of TJBAL, for players in the treatment group, we assigned all their observations that belong to the biweekly adjusted pre-shock window (i.e., January 14, 2021 up to February 14, 2021) as under control and all their observations in the biweekly adjusted after-shock window (i.e., February 15, 2021 up to March 23, 2021) as under treatment.<sup>23</sup> We use February 15, 2021 for the sole reason of splitting the observations of the *treatment* group into observations *under control* and observations *under treatment*. We use this specific date for the split, as it is the *biweekly* date that is closest to and earlier than the shock date that we use in our paper (February 19, 2021). For players in the control group, following a look-ahead sampling strategy (Bapna et al. 2018, Jung et al. 2019), we need the players who received their first moments later than the treatment group players. Therefore, we chose players who received their first moments within the period from February 15, 2021 (this is the two-week period closest to the shock date) up to May 10, 2021 (about a month after the end of our observation window). All the observations of these players are assigned as under control. Our sampling strategy is depicted in Figure 7 that follows.<sup>24</sup> We apply TJBAL using the mean balancing approach - that is used in the original paper (Hazlett and Xu 2018).

Before we look at the estimated average treatment effect of TJBAL, we assess the balance of the pre-treatment (logarithmically transformed) prices and the chosen covariates between the treatment and control groups. The results are presented in Figure 8 that follows. The results indicate that we have a better covariate balance (as assessed by the standardized difference in means) when the sample has been weighted using TJBAL compared to when it has not been weighted. This adds to the appropriateness of TJBAL, as it improves the balance of the pre-shock covariates.

The results of TJBAL are presented in the first row of Table 24 that follows. TJBAL yields an average treatment effect on the treated of about -0.2051 with a p-value of about 0.0366. This means that cards of treatment group

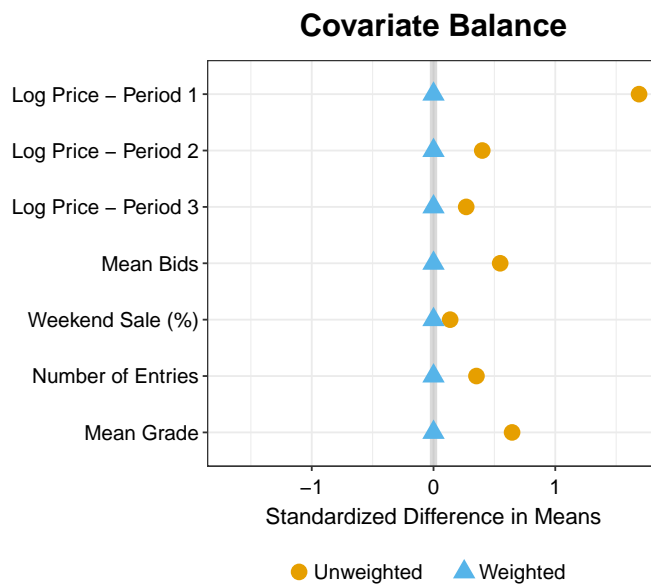
<sup>23</sup> This is similar to the interaction term coefficient between the treatment group and the after-treatment period in a classical DiD setting.

<sup>24</sup> An alternative sampling process would be to create a treated group that consists of players who received their first moment during the post-shock period (i.e., from February 15 to March 23, 2021), while the control group would include those players who received their first moment after the post-shock period (i.e., after March 23, 2021). Nevertheless, in this alternative process, it is likely that no treatment effects are observed, even though the treated group received moments (i.e., was affected) during the post-shock period. Reasons for this can include that (1) the players received their first moment very close to the end of the observation window, leaving us with few (if any) observations that reveal the treatment effect and/or that (2) the treatment effect takes a few days to materialize and because of the length of our post-shock period it does not materialize during our observation window. Under this scenario, this alternative approach would not allow us to estimate the treatment effect. For this reason, we did not follow this sampling process.

Figure 7 Product Choice - Trajectory Balancing - Sample Split.

				Card Listing Observation Window			
				Before Shock		After Shock	
				From	To	From	To
				January 14	February 14	February 15	March 23
When Player Received Their First Moment	Duration	Dates	Groups				
	From	January 7	Treatment	Under Control		Under Treatment	
	To	February 14					
	From	February 15	Control	Under Control		Under Control	
To	May 10						

Figure 8 Product Choice - Trajectory Balancing - Balance Assessment



The biweekly periods from January 14, 2021 up to February 15, 2021 are coded from 1 to 3.

players decrease in price by approximately -20.51% compared to basketball players who have not yet received their first moment (but will receive on later). Next, in Figure 9, we present the average treatment effect on the treated on every biweekly period. We want to draw attention to the form of the function in the pre-treatment period. We can see that by applying `tjbal` (with the chosen covariates), the pre-treatment outcomes are equal to 0 indicating that they are seamlessly balanced between the treatment and the control groups. This showcases that – as it should – TJBAL uses the provided covariates and weights observations in such a way that for the pre-treatment period, the control group presents an ideal counterfactual group to the treatment group. Hence, this also supports the appropriateness of TJBAL.

Finally, we have implemented an analysis as a falsification test for TJBAL. For this test, we randomize the treatment allocation of cards, while ensuring that the distribution of treated and control observations remains the same. The intuition behind this test is that if we randomly allocate the treatment and implement TJBAL, then the resulting estimates should not be statistically significant. The results of this implementation are presented in the second row of Table 24 and as we see the coefficient estimate is no longer statistically significant. This shows that TJBAL does not falsely provide statistically significant estimates, thereby lending to the validity of this method in our setting.

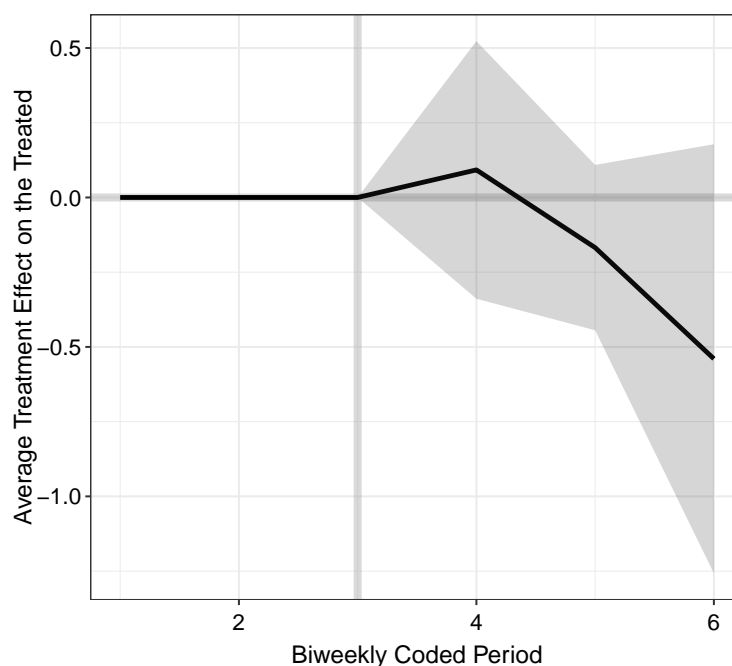
**Table 24 Product Choice - Trajectory Balancing - Estimates.**

Purpose	ATT	S.E.	Z-Score	CI. Lower	CI. Upper	P-Value
Main Result	-0.2051	0.0981	-2.09	-0.3974	-0.0128	0.0366
Falsification Test	0.2258	0.2076	1.087	-0.1812	0.6327	0.2769

*Notes.* The control group includes players that received their first moment in the period from February 15, 2021 up to May 10, 2021. In the first row, we present the results of the TJBAL implementation. In the second row, we present the results of the falsification test of TJBAL.

\* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

**Figure 9 Product Choice - Trajectory Balancing - Average Treatment Effect on the Treated per Biweekly Period.**



The biweekly periods from January 14, 2021 up to February 15, 2021 are coded from 1 to 3.

All in all, this alternative identification strategy - which is more robust against self-selection and sports-related seasonality - supports our previous results, as it estimates a negative and significant coefficient.

One might raise concerns that our control group (players who received their first moments starting February 15, 2021) violates the key difference-in-differences assumption that the control group is unaffected by the treatment. This is because the way our control group is defined, all the observations of players who received their first moments within the (biweekly adjusted) after-shock window are considered as control. In a classical difference-in-differences estimation, this could bias our estimates. Owing to the fact that some players in the control group became treated (i.e., received their first moment) during the after-shock window, the prices of these observations could have been negatively impacted (because of the treatment). But since these observations are considered control, the average prices of our defined control group are negatively impacted, thus leading to lower bound estimates for our estimated effect. However, we note that in counterfactual frameworks such as TJBAL - or for that matter the SC method or interactive fixed effects - the post-treatment control observations are represented by the predicted values for the treatment group, had this

group not received the treatment, and they are not represented by the actual post-treatment control observations (Xu 2017) (which is the case in a classical difference-in-differences estimation). Thus, using TJBAL, while employing a look-ahead sampling strategy to form the control group (that received their first moment after the shock date but during our observation period) is feasible. We also note that such a control group arguably allows having a more comparable set of players - as they received their first moments with little difference in time. Therefore, combining TJBAL with a look-ahead sampling strategy, makes our identification strategy even more solid against the above concerns.

Nevertheless, these arguments notwithstanding, we understand that this might raise some disquiet. Therefore, to alleviate concerns over the composition of our control group and for additional robustness, we run another iteration where the control group includes only players who received their first moments after our observation period, i.e., within the period from April 22, 2021 up to May 10, 2021. All the observations of these players are assigned as under control. The results of this TJBAL strategy are presented in Table 25 that follows. The results remain consistent with our first approach, yielding an average treatment effect on the treated of about -0.2483 with a p-value of about 0.0438.

**Table 25 Product Choice - Trajectory Balancing - Robustness Check - Estimate.**

ATT	S.E.	Z-Score	CI. Lower	CI. Upper	P-Value
-0.2483	0.1232	-2.016	-0.4897	-0.0069	0.0438

*Notes.* The control group includes players that received their first moment in the period from April 22, 2021 up to May 10, 2021.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

To summarize, TJBAL coupled with the look-ahead sampling strategy and only basketball cards presents itself as an alternative identification strategy that is more robust against selection. As the results are in line with our previous estimates for product choice (i.e., analysis of the cards of active players), we believe that they can provide support to alleviate the concern of selection on product choice.

## C.2. Product Scarcity - Alternative Identification Strategy

To tackle this, we utilize an alternative identification strategy to alleviate this concern. The identification strategy that we use is once again TJBAL.

However, to investigate product scarcity, we essentially need to analyze two cases: the case of players with many NFTs and the case of players with few NFTs. In both cases, we use the same sample that we used for product choice (i.e., the previous analysis), which is a look-ahead sample of basketball cards of active players that are aggregated to create a balanced biweekly panel data set, while we discard cards with missing observations. Although in each of the two cases we need to assign treated and control observations – similar to the product choice that we discussed above – the strategy now becomes slightly more complex.

Case 1 - Many NFTs (Non-Scarce): We start with the case of players with many NFTs. For this case, we need a group of players who had many NFTs for the duration of the observation window and a group of players who had few NFTs before the shock but afterwards they had many NFTs. We follow our implementation of TJBAL for product choice and we use February 14, 2021 as our point in time before the shock and May 10, 2021 as our later point in time. We then construct the two groups as follows: The control group includes the observations of players whose number of moments was larger than the median number of moments on both points in time<sup>25</sup>. All the observations of these players

<sup>25</sup> The two medians are different as they are updated accordingly.

are assigned as under control. For the treatment group, we chose players whose number of moments was positive and lower than (or equal to) the median before the shock, while it was larger than the median afterwards<sup>26</sup>. Once again, we need to define the period (and as an extension the observations) during which each treated unit was under control (i.e., not yet treated) and under treatment. Following the strategy of TJBAL, for these players, we assigned all their observations that belong to the two-week adjusted pre-shock window (i.e., January 14, 2021 up to February 14, 2021) as under control (i.e., not yet treated) and all their observations in the two-week adjusted after-shock window (i.e., February 15, 2021 up to March 23, 2021) as under treatment. Our sample split strategy is depicted in Figure 10 that follows.

**Figure 10 Product Scarcity - Many (Non-Scarce) - Trajectory Balancing - Sample Split.**

			Card Listing Observation Window			
			Before Shock		After Shock	
			From	To	From	To
Player # Moments as of February 14	Player # Moments as of May 10	Groups	January 14	February 14	February 15	March 23
$0 < \# \leq \text{Median}$	$\# > \text{Median}$	Treatment	Under Control		Under Treatment	
$\# > \text{Median}$	$\# > \text{Median}$	Control	Under Control		Under Control	

Case 2 - Few (Scarce): We then proceed similarly for the case of players with few NFTs. Following our previous implementations of TJBAL, we use the same points in time for before the shock (February 14, 2021) and afterwards (May 10, 2021). We then construct the two groups as follows: The control group includes the observations of players whose number of moments was positive and lower than (or equal to) the median number of moments in both points in time. All the observations of these players are assigned as under control. For the treatment group, we chose players who had no moments before the shock, while their number of moments was positive and lower than (or equal to) median afterwards. Following the strategy of TJBAL, for these players, we assigned all their observations that belong to the two-week adjusted pre-shock window (i.e., January 14, 2021 up to February 14, 2021) as under control (i.e., not yet treated) and all their observations in the two-week adjusted after-shock window (i.e., February 15, 2021 up to March 23, 2021) as under treatment. Our sample split strategy is depicted in Figure 11 that follows.

Before we present the results of the TJBAL implementations (using the mean balancing approach), we first inspect the balance of the pre-treatment (logarithmically transformed) prices and the chosen covariates between the treatment and control groups. The results are available in Figure 12 below. As we see, we have a better covariate balance when the sample has been weighted using TJBAL which adds to the appropriateness of TJBAL.

The results of this TJBAL implementation (using the mean balancing approach) are presented in the first two rows of Table 26. For the players with many moments (non-scarce), TJBAL estimates an average treatment effect on the treated of about -0.1544 with a p-value of about 0.047. For the players with few moments (scarce), TJBAL yields

<sup>26</sup> The two medians are different as they are updated accordingly.

**Figure 11 Product Scarcity - Few (Scarce) - Trajectory Balancing - Sample Split.**

			Card Listing Observation Window			
			Before Shock		After Shock	
			From	To	From	To
Player # Moments as of February 14	Player # Moments as of May 10	Groups	January 14	February 14	February 15	March 23
0	0 < # <= Median	Treatment	Under Control		Under Treatment	
0 < # <= Median	0 < # <= Median	Control	Under Control		Under Control	

an average treatment effect on the treated of about 0.0401 with a p-value of about 0.6645. Next, in Figures 13a and 13b that follow, we present the average treatment effect on the treated on every biweekly period. These graphs show that pre-treatment outcomes are well balanced between the treatment and control groups, supporting the validity of TJBAL.

**Table 26 Product Scarcity - Trajectory Balancing - Estimates.**

Purpose	Moment Scarcity	ATT	S.E.	Z-Score	CI. Lower	CI. Upper	P-Value
Main Results	Many (Non-Scarce)	-0.1544	0.0778	-1.986	-0.3068	-0.002	0.047
Main Results	Few (Scarce)	0.0401	0.0924	0.4337	-0.141	0.2212	0.6645
Falsification Test	Many (Non-Scarce)	-0.0856	0.0669	-1.28	-0.2167	0.0455	0.2006
Falsification Test	Few (Scarce)	0.104	0.101	1.03	-0.0939	0.3018	0.303

Finally, in the third and fourth rows of Table 26 we show the results of two falsification tests similar to that presented for the product choice analysis. As we see, both coefficient estimates are no longer statistically significant. This indicates that TJBAL does not yield statistically significant estimates incorrectly, thereby supporting the validity of this method in our context.

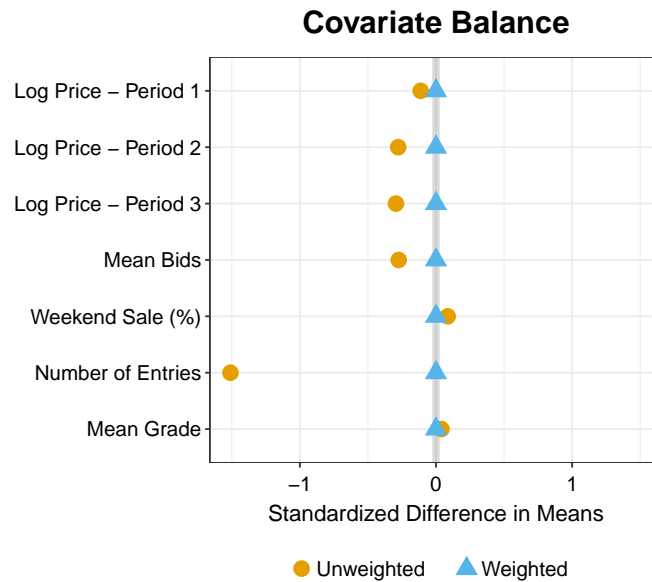
All in all, the analyses with TJBAL support the results from our analyses of the moderating impact of product scarcity, as we find that cards of players with many NFTs are significantly affected, while cards of players with few NFTs are not significantly affected. Given that TJBAL presents itself as an alternative identification strategy that is more robust against selection, we believe that its results can help alleviate the concern of selection on product scarcity.

### C.3. Product Quality and Seller Reputation - Alternative Identification Strategy

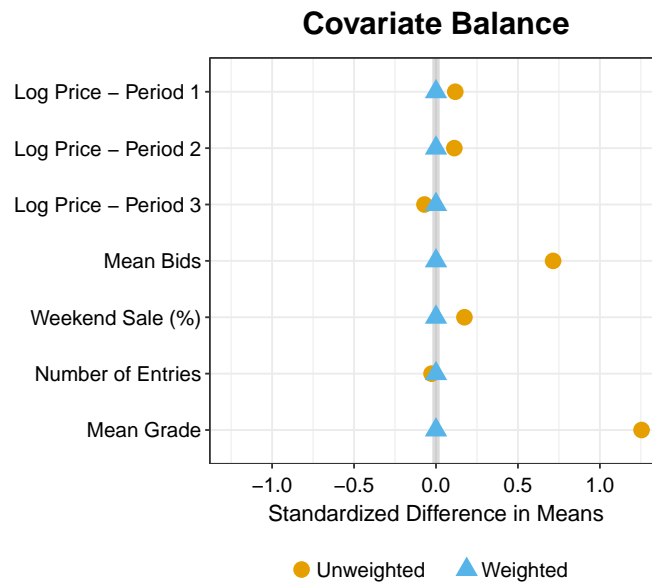
To investigate the validity of this concern, we utilize an alternative identification strategy.

This time we do not use TJBAL. If we use TJBAL for product quality, then we would need to aggregate the sales information of each card for both high and low-quality cards, leading to a sparser data set as more cards would need to be discarded due to missing observations. Similarly, if we use TJBAL for seller reputation, we would need to aggregate the sales information of each card for every seller which would once again lead to a sparser data set. Instead of TJBAL, we proceed with the more traditional option of propensity score matching to reduce the dissimilarity between high and low-quality cards as well as high and low-reputation sellers (Smith and Telang 2009). We proceed as follows.

Figure 12 Product Scarcity - Trajectory Balancing - Balance Assessment



(a) Many (Non-Scarce).



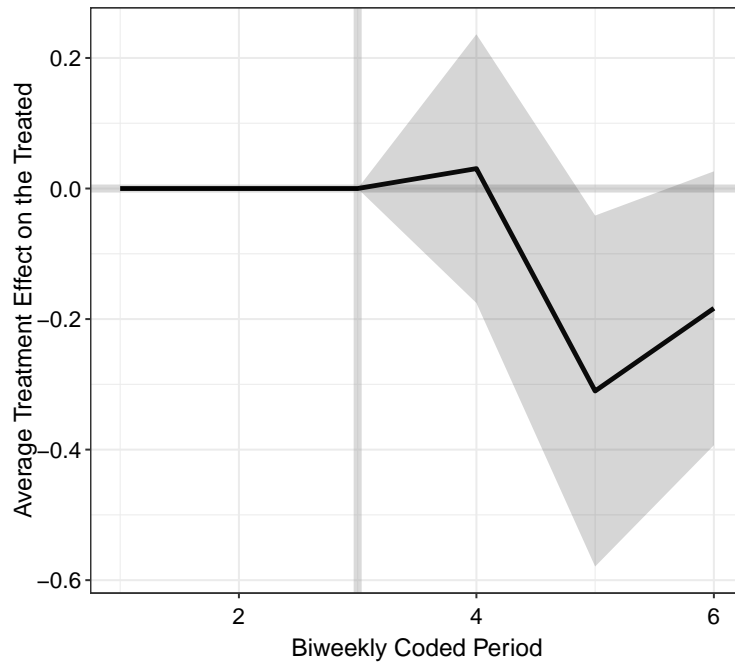
(b) Few (Scarce).

The biweekly periods from January 14, 2021 up to February 15, 2021 are coded from 1 to 3.

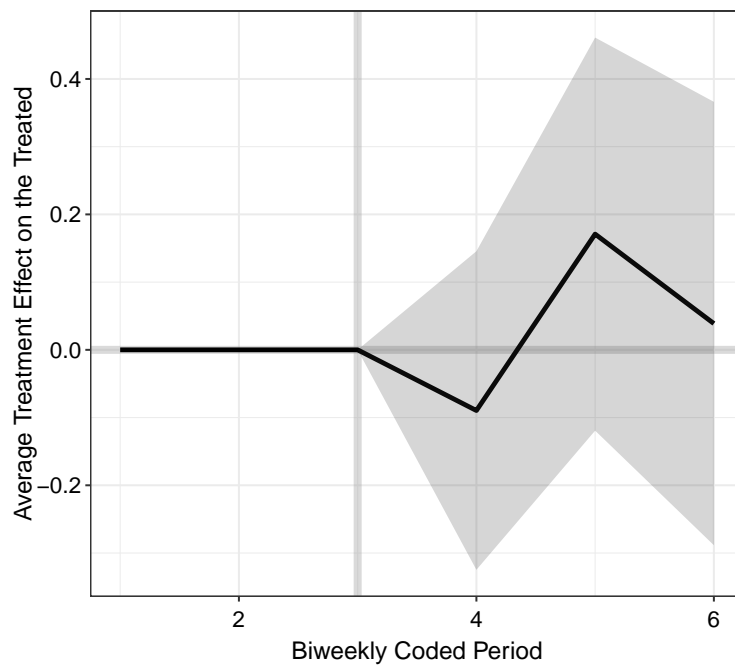
For product quality, we aggregate a number of variables<sup>27</sup> on the card level, for all the “ID-Valid” listings using their observations before the shock. We use these variables as the independent variables for matching. To implement the matching, we use logistic regression to calculate propensity scores on these covariates and then nearest-neighbor

<sup>27</sup> The specific variables can be found in Table 27 that follows.

**Figure 13** Product Scarcity - Trajectory Balancing - Average Treatment Effect on the Treated per Biweekly Period.



(a) Many (Non-Scarce).



(b) Few (Scarce).

The biweekly periods from January 14, 2021 up to March 23, 2021 are coded from 1 to 6.

matching to build our matched sample. As the dependent variable in this logistic regression, we use a binary variable which is 1 if the card is of high quality (grade  $\geq 9$  out of 10) and 0 if it is low quality (grade lower than 9). This strategy addresses product selection concerns raised (e.g., that cards of higher quality are systematically different to those of lower quality - this can be because cards of newer cards such as cards of active players were trade fewer times and are thus in better shape) by creating a sample wherein treatment and control groups contain very similar cards (e.g., in terms of active players, hall of famers, or price before the shock).

We assess the balance of the matched sample in Table 27 below. As we can see, the standard mean differences of all covariates are less than 0.1, indicating that we have a balanced sample, according to the thresholds typically applied (Imbens and Wooldridge 2009, Zhang et al. 2019). This is an improvement over the balance of the unmatched sample shown in Table 28. We note, however, that in the unmatched sample, high-quality cards were, among other dissimilarities, primarily active players and fewer HoFers. After matching, however, low- and high-quality cards are strikingly similar across all covariates.

We use the matched sample to rerun the analysis of the main paper and the results for higher quality cards and lower quality cards are presented in columns (1) and (2) of Table 29 respectively. As we see, the results remain similar in nature in that the coefficient estimate of the interaction term is negative and significant for cards of higher quality, while it is not significant for cards of lower quality.

**Table 27 Product Quality - Matched Sample - Balance Assessment.**

Variable	Lower Quality	Higher Quality	S.M.D.
N	10052	10052	-
Basketball	0.10 (0.31)	0.11 (0.32)	0.029
Active	0.02 (0.15)	0.04 (0.19)	0.084
HoFer	0.14 (0.34)	0.17 (0.37)	0.088
Mean Price	39.13 (153.92)	32.24 (206.00)	0.038
Buy It Now (%)	0.74 (0.37)	0.71 (0.41)	0.070
Mean Bids	1.52 (3.25)	1.73 (3.76)	0.058
Weekend Sale (%)	0.34 (0.39)	0.38 (0.44)	0.099
Number of Entries	2.24 (2.38)	2.29 (4.05)	0.016

*Notes.* The variables are aggregated at the product ID level prior to matching. S.M.D.: Standardized Mean Difference. HoFer: Hall-of-Famer

For seller reputation, we proceed in a similar fashion. We aggregate a number of variables<sup>28</sup> on the seller and sports level, for all the “ID-Valid” listings using their observations before the shock. We use these variables as the independent variables for matching and as the independent variable we use whether a seller is reputable or not<sup>29</sup>. To implement the matching, we use logistic regression to calculate propensity scores on these covariates and then nearest-neighbor matching to build our matched sample. This strategy addresses concerns that reputable sellers sell systematically different cards (e.g., in terms of card quality). As we see, after the matching these covariates are well-balanced between high and low reputation sellers.

<sup>28</sup> The specific variables can be found in Table 30 that follows.

<sup>29</sup> Reputable sellers are those whose seller feedback score is larger than the median - 325.

**Table 28 Product Quality - Unmatched Sample - Balance Assessment.**

Variable	Lower Quality	Higher Quality	S.M.D.
N	12286	18027	-
Basketball	0.09 (0.28)	0.31 (0.46)	0.590
Active	0.02 (0.13)	0.20 (0.40)	0.610
HoFer	0.15 (0.36)	0.13 (0.34)	0.067
Mean Price	82.40 (849.97)	26.97 (159.38)	0.091
Buy It Now (%)	0.69 (0.38)	0.75 (0.38)	0.178
Mean Bids	2.40 (4.61)	1.46 (3.38)	0.232
Weekend Sale (%)	0.35 (0.38)	0.33 (0.42)	0.046
Number of Entries	2.67 (3.46)	2.35 (3.69)	0.090

*Notes.* The variables are aggregated at the product ID level prior to matching. S.M.D.: Standardized Mean Difference. HoFer: Hall-of-Famer.

**Table 29 Product Quality - Matched Sample.**

	Log Price	
	Grade $\geq$ 9	Grade < 9
	(1)	(2)
<i>After Shock</i> $\times$ <i>Basketball Card</i>	-0.103*** (0.035)	0.075 (0.081)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Card Fixed Effects	Yes	Yes
Adjusted R <sup>2</sup>	0.857	0.832
Observations	46,047	45,985

*Notes.* We analyze the listings matched using propensity score matching and we cluster the standard errors at the seller level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

We assess the balance of the matched sample Table 30 that follows. As we see, the standard mean differences of all the covariates are less than 0.1, indicating that we have a balanced sample. This is an improvement over the balance of the unmatched sample shown in Table 31. Again, comparing the unmatched with the matched sample, we find that before matching, reputable sellers sold, for instance, more HoFer cards and fewer cards of active players. After matching, however, the sold cards of reputable and non-reputable sellers are very similar.

Finally, we use the matched sample to rerun the analysis of the main paper. The results are presented in Table 32. As we see, the results remain similar in nature in that the coefficient estimate of the interaction term is negative and significant for reputable sellers, while it is not significant for non-reputable sellers.

To summarize, we use matching to create balanced samples that are more robust against selection concerns and rerun our main analyses. The estimates from these new analyses are in line with those of our main analyses, therefore supporting the validity of our results as robustness checks, and alleviating the concerns over product quality and seller selections biasing our estimates.

**Table 30 Seller Reputation - Matched Sample - Balance Assessment.**

Variable	Non-Reputable	Reputable	S.M.D.
N	16662	16662	-
Mean Basketball	0.57 (0.46)	0.59 (0.44)	0.034
Mean Active	0.52 (0.46)	0.50 (0.45)	0.033
Mean Price	658.16 (3300.33)	711.25 (2741.79)	0.017
Mean Near Mint	0.76 (0.39)	0.74 (0.38)	0.042
Mean Grade	8.61 (2.16)	8.55 (1.99)	0.026
Buy It Now (%)	0.62 (0.46)	0.60 (0.46)	0.041
Mean Bids	4.22 (8.55)	4.40 (8.57)	0.022
HoFer	0.37 (0.44)	0.38 (0.42)	0.024
Weekend Sale (%)	0.32 (0.40)	0.32 (0.37)	0.003
Number of Entries	3.26 (5.64)	10.02 (295.72)	0.032

*Notes.* The variables are aggregated at the seller level prior to matching. S.M.D.: Standardized Mean Difference. HoFer: Hall-of-Famer.

**Table 31 Seller Reputation - Unmatched Sample - Balance Assessment.**

Variable	Non-Reputable	Reputable	S.M.D.
N	20043	24941	-
Mean Basketball	0.63 (0.45)	0.48 (0.45)	0.336
Mean Active	0.58 (0.46)	0.43 (0.44)	0.327
Mean Price	598.77 (3037.92)	622.68 (2724.31)	0.008
Mean Near Mint	0.79 (0.37)	0.70 (0.39)	0.220
Mean Grade	8.74 (2.07)	8.38 (2.15)	0.173
Buy It Now (%)	0.56 (0.48)	0.66 (0.44)	0.229
Mean Bids	4.44 (8.43)	3.78 (8.06)	0.080
HoFer	0.33 (0.43)	0.40 (0.42)	0.165
Weekend Sale (%)	0.31 (0.40)	0.32 (0.36)	0.045
Number of Entries	2.97 (5.20)	13.47 (247.33)	0.060

*Notes.* The variables are aggregated at the seller level prior to matching. S.M.D.: Standardized Mean Difference. HoFer: Hall-of-Famer.

**Table 32 Seller Reputation - Matched Sample.**

	Log Price	
	Reputable (1)	Non-Reputable (2)
<i>After Shock</i> × <i>Basketball Card</i>	-0.118*** (0.041)	-0.032 (0.021)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Card Fixed Effects	Yes	Yes
Adjusted R <sup>2</sup>	0.774	0.718
Observations	356,846	88,499

*Notes.* We analyze the "ID-Valid" listings matched using propensity score matching. In column (1), we analyze the listings of sellers with a seller feedback score greater than the median (325) and in column (2), we analyze the listings of sellers with a seller feedback score lower than or equal to the median. In both specifications, we cluster the standard errors at the seller level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

#### C.4. Market Expansion Effect - Alternative Identification Strategy

Similarly to the two analyses above, we do not use TJBAL, because TJBAL will lead to a sparser data set as more cards would need to be discarded due to missing observations. Instead of TJBAL, we proceed with the more traditional option of propensity score matching to reduce the dissimilarity between high and low-quality cards for each group of players (Smith and Telang 2009). For this analysis, we follow the process described below separately for cards of high and low quality.

We aggregate a number of variables<sup>30</sup> on the card level, for all the “ID-Valid” listings using their observations before the shock. We use these variables as the independent variables for matching. To implement the matching, we use logistic regression to calculate propensity scores on these covariates and then nearest-neighbor matching to build our matched sample. As the dependent variable in this logistic regression, we use a binary variable which is 1 if the card depicts a popular player and 0 if it depicts an unpopular player. This strategy addresses the selection concerns by creating a sample wherein treatment and control groups contain very similar cards (e.g., in terms of or price before the shock).

We assess the balance of the matched samples in Tables 33 and 35 below. As we see, the standard mean differences of all the covariates are less than 0.15, indicating that we have balanced samples (Imbens and Wooldridge 2009). This is an improvement over the balance of the unmatched samples shown in Tables 34 and 36.

We use the matched samples to rerun the analysis of Table 11 (of the main paper) and present the results in Table 37 below. The results for higher quality cards of popular and unpopular players are presented in columns (1) and (2) respectively, while those of lower quality cards of popular and unpopular players in columns (3) and (4) respectively. As we see, the coefficient estimates of the interaction term remain qualitatively similar to those of the unmatched sample (Table 11 of the main paper) providing no evidence to support that these results are impacted by endogeneity or selection issues.

**Table 33 Player Popularity - Matched Sample of High Quality Cards - Balance Assessment.**

Variable	Unpopular	Popular	S.M.D.
N	172	172	-
Basketball	0.44 (0.50)	0.41 (0.49)	0.047
Mean Price	59.14 (87.15)	69.90 (86.36)	0.124
Buy It Now (%)	0.63 (0.30)	0.67 (0.23)	0.149
Mean Bids	3.37 (3.43)	3.15 (2.92)	0.071
Weekend Sale (%)	0.28 (0.25)	0.27 (0.20)	0.066
Number of Entries	38.34 (165.45)	46.33 (141.27)	0.052

*Notes.* The variables are aggregated at the product ID level prior to matching. S.M.D.: Standardized Mean Difference.

<sup>30</sup> The specific variables can be found in Table 33 that follows.

**Table 34 Player Popularity - Unmatched Sample of High Quality Cards - Balance Assessment.**

Variable	Unpopular	Popular	S.M.D.
N	4695	260	-
Basketball	0.37 (0.48)	0.58 (0.49)	0.445
Mean Price	48.58 (182.83)	359.75 (1485.28)	0.294
Buy It Now (%)	0.67 (0.36)	0.62 (0.24)	0.185
Mean Bids	2.44 (3.67)	5.26 (5.77)	0.583
Weekend Sale (%)	0.29 (0.34)	0.27 (0.18)	0.086
Number of Entries	27.23 (225.58)	120.76 (393.69)	0.292

*Notes.* The variables are aggregated at the product ID level prior to matching. S.M.D.: Standardized Mean Difference.

**Table 35 Player Popularity - Matched Sample of Low Quality Cards - Balance Assessment.**

Variable	Unpopular	Popular	S.M.D.
N	132	132	-
Basketball	0.54 (0.50)	0.54 (0.50)	<0.001
Mean Price	60.26 (83.45)	68.84 (83.73)	0.103
Buy It Now (%)	0.52 (0.40)	0.48 (0.37)	0.101
Mean Bids	4.38 (5.82)	4.39 (4.73)	0.001
Weekend Sale (%)	0.27 (0.33)	0.30 (0.32)	0.078
Number of Entries	6.15 (13.70)	7.38 (11.99)	0.095

*Notes.* The variables are aggregated at the product ID level prior to matching. S.M.D.: Standardized Mean Difference.

**Table 36 Player Popularity - Unmatched Sample of Low Quality Cards - Balance Assessment.**

Variable	Unpopular	Popular	S.M.D.
N	1401	176	-
Basketball	0.30 (0.46)	0.61 (0.49)	0.657
Mean Price	50.26 (143.33)	259.58 (700.15)	0.414
Buy It Now (%)	0.46 (0.42)	0.50 (0.34)	0.105
Mean Bids	3.87 (5.47)	5.15 (5.22)	0.240
Weekend Sale (%)	0.35 (0.40)	0.29 (0.30)	0.189
Number of Entries	4.92 (20.02)	20.21 (73.37)	0.284

*Notes.* The variables are aggregated at the product ID level prior to matching. S.M.D.: Standardized Mean Difference.

**Table 37 Matched Samples - Active Players - Popularity and Quality Analysis.**

	Log Price			
	Grade $\geq$ 9 Popular (1)	Grade $\geq$ 9 Unpopular (2)	Grade $<$ 9 Popular (3)	Grade $<$ 9 Unpopular (4)
<i>After Shock</i> $\times$ <i>Basketball Card</i>	0.077 (0.048)	-0.188*** (0.060)	0.634*** (0.132)	-0.113 (0.182)
Seller Fixed Effects	Yes	Yes	Yes	Yes
Day Fixed Effects	Yes	Yes	Yes	Yes
Card Fixed Effects	Yes	Yes	Yes	Yes
Adjusted R <sup>2</sup>	0.733	0.657	0.592	0.432
Observations	17,632	14,711	2,179	1,677

*Notes.* In all specifications, we analyze only the “ID-Valid” listings and we cluster the standard errors at the seller level. ID-Valid: Listings with exactly one identified player.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## Appendix. 3. Card-Fixed Effects Model And PWCC Search Tool

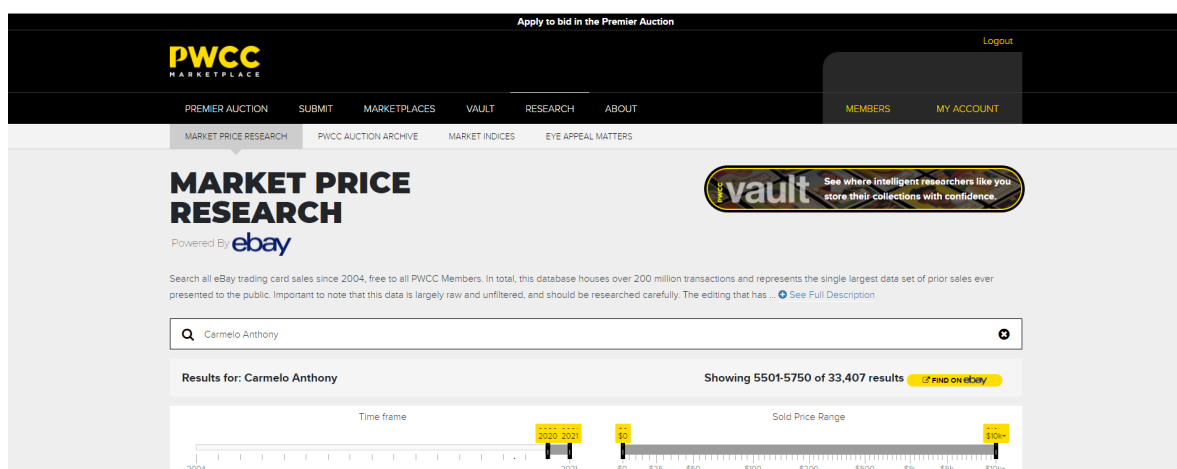
### A. Card-Fixed Effects

Our model identifies both the year and the player featured in a sold card, using the card's title and other card details provided by the seller. The model works as follows: First, it identifies the player(s) featured in a card. To do so, we web scraped two data sources that contain the full names of all the basketball and baseball players who have ever played in the NBA<sup>31</sup> and the MLB<sup>32</sup>. These sources also contain information such as the players' active years and whether a player has been inducted into the Hall of Fame. After getting this data, every card title is compared to the respective sport player dictionary to identify the player(s) who are contained in its title. Our approach identifies at least one player for about 94% of the basketball cards and about 85% of the baseball cards. Second, it uses natural language processing to identify the year that a card refers to by analyzing the card's title and any other card details provided by the seller. Both player name dictionaries contain instances of duplicate player names. For example, in the baseball player names' dictionary there are 6 entries of "Bob Smith", each referring to a different player. To disambiguate which specific player a card refers to, we utilize the players' active years and the year that a card refers to. Our approach is unable to break the ties if (1) multiple players with the same names were active during the same years and (2) the year that a card refers to falls within that time overlap. However, this happens in very few cases. We flag all observations referring to exactly one identified player as "ID-Valid" and we construct a product ID (i.e., card ID) by combining the player and the year that each listing refers to.

### B. PWCC Search Tool

In Figure 14 below, we present an example of the PWCC search tool. This tool provided a window where the user can enter their desired keyword and retrieve all the results that include that keyword in their title. In this example, the used keyword was "Carmelo Anthony".

Figure 14 PWCC Search Tool.



<sup>31</sup> <https://www.basketball-reference.com/players/>

<sup>32</sup> <https://www.baseball-reference.com/players/>

## Appendix. 4. Parallel Trends Assumption

### A. eBay 2021 Data

We validate the robustness of the parallel trends assumption with three tests. The first is a placebo test where we analyze only the observations before the shock date. We use February 1, 2021, as our placebo shock date, because it allows us to split the analyzed time window into two equal parts: January 14, 2021, up to January 31, 2021, and February 1, 2021 up to February 18, 2021 (18 days each). The results of this test (using specification (3) of Table 2 of the main paper) can be found in Table 38, where the insignificant estimate shows no evidence of the parallel trends assumption being violated.

**Table 38 Placebo Test - eBay 2021 Data.**

	Log Price
<i>After Placebo</i> × <i>Basketball Card</i>	−0.025 (0.021)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.652
Observations	453,135

*Notes.* We cluster the standard errors at the seller level. Placebo shock date: February 1, 2021. Only observations before February 19, 2021 were considered.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

The second test uses a fully flexible DiD model with treatment lags and leads. In the model’s traditional implementation, pre-treatment and post-treatment periods are split into multiple equally-sized intervals (usually two to three annual ones) (Autor 2003, Angrist and Pischke 2009). We follow this approach by creating 12-day<sup>33</sup> intervals before the shock date but combine all the post-treatment observations into one interval. We do so because the alternative option (i.e., splitting the post-treatment period into intervals of 12 days) would reduce the post-treatment period by 9 (out of its total 33) days. Our specification is as follows:

$$\begin{aligned} \log(\text{Sold Price})_i = & \beta_0 + \beta_1 \Sigma \text{Period}_t + \beta_2 \text{Basketball Card}_i \\ & + \Sigma \beta_3 \text{Period}_t \times \text{Basketball Card}_i + \text{Seller}_i + \text{Day}_i + \epsilon_{it} \end{aligned} \quad (1)$$

where  $\text{Seller}_i$  and  $\text{Day}_i$  indicate seller and day-fixed effects, respectively, while  $\text{Period}_i$  indicates the period during which card  $i$  was sold.  $\text{Period}_{-1}$  to  $\text{Period}_{-3}$  indicates one of three 12-day periods between January 14, 2021, and February 18, 2021, while the higher the  $t$ , the closer the 12-day period is to the shock date. Additionally,  $\text{Period}_0$  indicates cards sold at or after the shock date. Lastly, we cluster the standard errors at the seller level. The coefficient estimates are displayed in Table 39, where  $\text{Period}_{-1}$  is used as the baseline. The insignificant estimates of the interaction terms between  $\text{Period}_{-3}$  and *Basketball Card* and between  $\text{Period}_{-2}$  and *Basketball Card* provide no evidence of the parallel trends assumption being violated.

In the third test, following the suggestion of Angrist and Pischke (2009), we restricted the data to the pre-treatment time and regressed the outcome variable on the treatment indicator, a linear time trend, and their interaction. If the

<sup>33</sup> Using 12-day intervals allows us to keep all our pre-treatment observations (36 days in total).

parallel trends assumption holds, the estimated interaction term coefficient is insignificant. The results of this specification can be seen in column (1) of Table 40. The interaction term coefficient is very small and significant only at the 10% level, suggesting only minor pre-trend differences. We look deeper into this by removing the observations on the 17<sup>th</sup> and 18<sup>th</sup> of February from our observation window and rerunning this analysis. We do this because the effect may have started a few days before our shock date. The results, which are presented in column (2), show that the estimated coefficient is insignificant. Thus, the three tests grant us confidence that the parallel trends assumption is not violated.

**Table 39 Fully Flexible DiD Model with Treatment Lags and Leads - eBay 2021 Data.**

	Log Price
$Period_{-3} \times Basketball\ Card$	0.011 (0.022)
$Period_{-2} \times Basketball\ Card$	0.029 (0.021)
$Period_0 \times Basketball\ Card$	-0.040** (0.017)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.638
Observations	958,377

*Notes.* We cluster the standard errors at the seller level.  $Period_{-1}$  (the 12-day period from February 7, 2021 up to February 18, 2021) is used as the baseline.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 40 Pre-Trend Analysis with Time Trend - eBay 2021 Data.**

	Log Price	
	(1)	(2)
$Time\ Trend \times Basketball\ Card$	-0.001* (0.001)	-0.001 (0.001)
Seller Fixed Effects	Yes	Yes
Day Fixed Effects	Yes	Yes
Adjusted R <sup>2</sup>	0.652	0.654
Observations	453,135	424,874

*Notes.* In both specifications, we cluster the standard errors at the seller level. In column (1), only observations before February 19, 2021 were considered. In column (2), only observations before February 17, 2021 were considered.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## B. PWCC Data

In Tables 38, 39, and 40, we test the robustness of the parallel trends assumption for the eBay data using three tests. We run similar tests here, albeit for the PWCC data. The first is a placebo test where we analyze only the observations before the shock date (February 19, 2021). In addition, we use October 19, 2020, as our placebo shock date, since it allows us to split the analyzed time window into two equal parts: June 19, 2020 up to October 18, 2020, and October 19, 2020 up to February 18, 2021 (about 122 days each). For this test, we use a specification (4) of Table 5 and the results are available in Table 41. The insignificant estimate shows no evidence of the parallel trends assumption being violated.

For the second test, we implement a fully flexible DiD model with treatment lags and leads. However, in this implementation, we use 80-day intervals. Thus,  $Period_{-3}$  refers to the 80-day period between June 21, 2020 and September 9, 2020,  $Period_{-2}$  refers to the 80-day period between September 10, 2020 and November 29, 2020,  $Period_{-1}$  refers to the 80-day period between November 30, 2020 and February 18, 2021, while  $Period_0$  refers to all the listings sold at or after February 19, 2021. In this specification, we cluster the standard errors at the card level. The results can be found in Table 42, where  $Period_{-1}$  is used as the baseline. The insignificant estimates of the interaction terms between  $Period_{-3}$  and  $Basketball\_Card$  and between  $Period_{-2}$  and  $Basketball\_Card$  provide no evidence of the parallel trends assumption being violated.

**Table 41 Placebo Test - PWCC Data.**

	Log Price
<i>After Placebo</i> × <i>Basketball Card</i>	0.015 (0.018)
Seller Fixed Effects	No
Day Fixed Effects	Yes
Card Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.378
Observations	1,364,194

*Notes.* We use specification (4) of Table 5 and we cluster the standard errors at the card level. Placebo shock date: October 19, 2020. Only observations before February 19, 2021 were considered.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 42 Fully Flexible DiD Model with Treatment Lags and Leads - PWCC Data.**

	Log Price
<i>Period</i> <sub>-3</sub> × <i>Basketball Card</i>	0.006 (0.023)
<i>Period</i> <sub>-2</sub> × <i>Basketball Card</i>	-0.018 (0.021)
<i>Period</i> <sub>0</sub> × <i>Basketball Card</i>	-0.126*** (0.020)
Seller Fixed Effects	No
Day Fixed Effects	Yes
Card Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.375
Observations	1,755,611

*Notes.* We use specification (4) of Table 5 and we cluster the standard errors at the card level.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 43 Pre-Trend Analysis with Time Trend - PWCC Data.**

	Log Price
<i>Time Trend</i> × <i>Basketball Card</i>	0.00002 (0.0002)
Seller Fixed Effects	No
Card Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.378
Observations	1,364,194

*Notes.* We use specification (4) of Table 5 and we cluster the standard errors at the card level. Only observations before February 19, 2021 were considered.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

For the third test, we analyze only the observations before the shock date. Again, we also change the specification and replace the interaction term with an interaction between whether a listing is a basketball card and a linear time trend. The results are presented in Table 43, where the insignificant coefficient estimate supports the validity of the parallel trends assumption.

### C. eBay 2022 Data

We also test the robustness of the parallel trends assumption for the 2022 eBay data. These tests are very similar to those that were used for the 2021 eBay data (Tables 38, 39, and 40). However, in this case we use the 2022 data. The results of the placebo test are presented in Table 44 and, once again, the estimated coefficient is insignificant, indicating no evidence of the parallel trends assumption being violated.

The results of the fully flexible DiD model with treatment lags and leads can be found in Table 45. As we see, the coefficient estimates between *Period*<sub>-2</sub> and *Basketball\_Card* and *Period*<sub>-3</sub> and *Basketball\_Card* are insignificant, thus providing no evidence of the parallel trends assumption being violated. The results of the third test can be

**Table 44 Placebo Test - eBay 2022 Data.**

	Log Price
<i>After Placebo</i> × <i>Basketball Card</i>	0.030 (0.019)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.488
Observations	361,482

*Notes.* We cluster the standard errors at the seller level. Placebo shock date: February 1, 2022. Only observations before February 19, 2022 were considered.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

seen in Table 46, where the insignificant coefficient estimate does not support that the parallel trends assumption is violated.

**Table 45 Fully Flexible DiD Model with Treatment Lags and Leads - eBay 2022 Data.**

	Log Price
<i>Period</i> <sub>-3</sub> × <i>Basketball Card</i>	-0.024 (0.023)
<i>Period</i> <sub>-2</sub> × <i>Basketball Card</i>	-0.015 (0.028)
<i>Period</i> <sub>0</sub> × <i>Basketball Card</i>	-0.039** (0.019)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.475
Observations	651,972

*Notes.* We cluster the standard errors at the seller level. *Period*<sub>1</sub> (the 12-day period from February 7, 2022 up to February 18, 2022) is used as the baseline.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 46 Pre-Trend Analysis with Time Trend - eBay 2022 Data.**

	Log Price
<i>Time Trend</i> × <i>Basketball Card</i>	0.001 (0.001)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.488
Observations	361,482

*Notes.* We cluster the standard errors at the seller level. Only observations before February 19, 2022 were considered.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

#### D. eBay Basketball 2021 vs 2022 Data

Finally, we tested the validity of the parallel trends assumption for the specification where we used eBay basketball data from 2022 as the control group and eBay basketball data from 2021 as the treated group. To do this, we used the same three tests that we have applied so far. The results of these tests can be found in Tables 47, 48, and 49 that follow. As we see, they provide no evidence to support that the parallel trends assumption is violated.

**Table 47 Placebo Test - eBay Basketball 2021 vs 2022.**

	Log Price
<i>After Placebo</i> × <i>Year</i> 2021	−0.057 (0.036)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.606
Observations	207,015

*Notes.* We cluster the standard errors at the seller level. Placebo shock date: February 8 (2021 and 2022). Only observations before February 19 (2021 and 2022) were considered.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 48 Fully Flexible DiD Model with Treatment Lags and Leads - eBay Basketball 2021 vs 2022.**

	Log Price
<i>Period</i> <sub>−3</sub> × <i>Year</i> 2021	0.051 (0.033)
<i>Period</i> <sub>−2</sub> × <i>Year</i> 2021	−0.019 (0.038)
<i>Period</i> <sub>0</sub> × <i>Year</i> 2021	−0.039 (0.025)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.585
Observations	502,807

*Notes.* We cluster the standard errors at the seller level. *Period*<sub>1</sub> (the 6-day period from February 13 (2021 and 2022) up to February 18, (2021 and 2022)) is used as the baseline.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

**Table 49 Pre-Trend Analysis with Time Trend - eBay Basketball 2021 vs 2022.**

	Log Price
<i>Time Trend</i> × <i>Year</i> 2021	−0.005 (0.003)
Seller Fixed Effects	Yes
Day Fixed Effects	Yes
Adjusted R <sup>2</sup>	0.608
Observations	188,291

*Notes.* We cluster the standard errors at the seller level. Only observations before February 19 (2021 and 2022) were considered.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## Appendix. 5. Understanding NFTs

### A. NFTs vs Other Digital Goods

One might wonder what (if any) are the differences between NFTs and other digital goods. In short, their differences arise from the blockchain technology that powers and differentiates NFTs from other digital goods. NFTs are records on the blockchain associated with specific copies of digital goods (i.e., files), while these records are publicly available and immutable. Furthermore, each record on the blockchain can have unique properties associated with them. The differences that are relevant in our setting are ownership, uniqueness, and scarcity. We discuss each of them below.

First, NFTs allow for ownership. Ownership is a requirement for collectibles to exist, as without this attribute the goods cannot be collected. NFTs possess this attribute as ownership information is publicly available and verifiable through the blockchain. Specifically, ownership refers to claims over specific copies of digital goods.<sup>34</sup> Because NFTs possess this attribute, it became possible for digital collectibles to exist (in the form of NFTs). On the other hand, digital goods other than NFTs lack this attribute. Due to their lack of this attribute, they can be copied and (some times illegally) used without the owner's or creator's consent<sup>35</sup>.

Second, each NFT is unique and distinguishable from others. In the setting of collectibles, uniqueness can add collectible value to the goods. NFTs possess this attribute as each NFT represents a singular record on the blockchain, ensuring that no two are identical. On the other hand, uniqueness is absent in digital goods other than NFTs, as each copy is the same as any other copy. Therefore, these digital goods do not have collectible value.

Third, NFTs can be scarce. Scarcity can add unique dynamics to the valuation of digital collectibles (NFTs), similar to how the prices of physical collectibles can change depending on the number of available copies. NFTs possess this attribute as they can be programmed to have a limited number of copies (i.e., scarcity). On the other hand, scarcity is not available in other digital goods – other than NFTs – as these goods can be copied and reproduced an unlimited number of times.

These three differences between NFTs and other digital goods make it possible for digital collectibles to exist in the form of NFTs. We summarize these unique attributes of NFTs compared to other digital goods in Table 50 that follows.

**Table 50 Unique Attributes of NFTs vs. Digital Goods.**

Attribute	Added Value	Why NFTs possess them
Ownership	Good can be collectible	Verifiable through the blockchain
Uniqueness	Collectible value	Each NFT is a unique record on the blockchain
Scarcity	Pricing dynamics	Limited number of copies

<sup>34</sup> By contrast, we note that purchasing an NFT does not typically grant ownership of underlying creative content, such as intellectual property rights (Weiss and Ozer 2022)

<sup>35</sup> In such cases, attempts to ensure that ownership is verified included actions such as watermarks but there are ways to circumvent them.

## **B. How Can These Attributes Shape Effects?**

These attributes may lead to different substitution or expansion effects of NFTs (compared to other digital goods) on physical goods.

First, we focus on ownership. Ownership is made possible due to the mechanism of transferability which allows NFTs to serve functions analogous to physical collectibles, such as being resold, gifted, or displayed, which other digital goods typically cannot support. As such, on one hand, ownership enables a substitution channel, since consumers who value possession and tradeability may shift demand from physical collectibles to NFTs. On the other hand, verifiable ownership may also generate market expansion, by drawing in consumers who are comfortable with digital platforms but previously disengaged from physical collectibles. This nicely complements the prior literature on the interaction between digital and physical goods. Previous research indicates that freely available digital copies can boost demand for physical editions of the same title (Nagaraj and Reimers 2023). Similarly, we find that the introduction of moments can boost demand for certain cards (of the same players) (column (3) in Table 11 of the main paper). In this case, ownership of a digital good spurs market expansion of a physical good.

Second, we focus on uniqueness. Uniqueness is made possible due to the mechanism of identity signaling through rarity, that allows NFTs to be verifiably unique rather than infinitely replicable. The uniqueness of NFTs enables them to mimic limited-edition physical goods and generate scarcity-based value, thereby supporting a substitution effect: consumers who are drawn to singular or rare items may find NFTs to be a viable alternative to physical collectibles. For this property, we believe that the potential for market expansion exists but is less certain and it likely depends on whether exposure to unique digital goods fosters interest in collecting more broadly. In contrast to NFTs, other digital goods lack the capacity to deliver uniqueness and expand markets through accessibility and convenience, instead of engaging users in identity-driven or rarity-based consumption.

Third, we focus on scarcity. This is made possible since blockchain allows for finite replicability. Scarcity reinforces the collectible logic of NFTs and supports substitution effects, particularly in markets where exclusivity and limited availability drive value. While a substitution effect is straightforward, whether scarcity also facilitates market expansion is less clear. In theory, limited-supply NFTs could create a sense of urgency, thereby increasing general interest in collectibles, including physical ones. However, this effect is likely to be more indirect and context-dependent. By contrast, the expansionary effects of other digital goods have typically relied on infinite scalability and widespread access, rather than scarcity-driven demand dynamics. In this case, our paper can offer some insight, as we find that cards of players with many NFTs (non-scarce) are more negatively affected than the cards of players with few NFTs (scarce) which suggests that the substitution effect dominates the market expansion effect.

Taken together, these three properties (summarized in Table 51 that follows) suggest that NFTs can affect the market for physical goods in ways that diverge meaningfully from the dynamics observed with other digital goods. While all three properties support potential substitution channels - by replicating features central to physical collectibles - only ownership appears to offer a relatively clear pathway toward market expansion, especially by introducing new consumers to collectible behaviors. In contrast, the expansionary effects of uniqueness and scarcity are more indirect and context-dependent, relying on factors such as consumer interest in rarity or urgency.

**Table 51** How the Unique Attributes of NFTs (vs. Digital Goods) Shape Substitution and Market Expansion.

Attribute	Physical Goods Effect – Substitution	Physical Goods Effect – Expansion
Ownership	Allows NFTs to substitute physical collectibles for consumers valuing possession	May attract digital-native consumers previously disengaged from physical collectibles
Uniqueness	Allows NFTs to appeal as substitutes to collectors who value rarity and singular goods	May indirectly expand interest in collectibles (context-dependent)
Scarcity	Allows NFTs to become substitutes for scarce physical items	May create urgency, indirectly expanding collectibles interest

## C. Survey Results

In this Online Appendix, we present our investigation into the utility that buyers/collectors derive from NTS moments and from basketball cards. To this end, we followed a two-step approach. First, we distributed an exploratory survey with open-ended questions on Reddit. Second, we used the Reddit results to inform the design of a second survey, which we distributed on Amazon Mechanical Turk (AMT). In both surveys, we asked respondents why they buy an NTS moment or a card; the reasons they provided served as proxies for the utility of each collectible type. In addition, we asked the respondents for their demographics so that we can better understand the two consumer groups.

### C.1. Reddit Surveys

In the first step, we designed two surveys with open-ended questions: one for NTS moments and one for basketball trading cards. In the NTS moments survey, we asked respondents if they have ever purchased a moment. If the answer was positive, we asked them for the reason why they bought a moment and for the reasons they prefer moments over sports trading cards. In the basketball trading cards survey, we asked the same questions but swapped “moments” and “sports trading cards”. We distributed the surveys to dedicated Reddit forums (called “subreddits”) because collectors of both goods frequently use this website to discuss collectible-related issues. We posted the NTS moments survey to the subreddit “/r/nbatopshot” and we posted the basketball trading cards survey to the subreddit “/r/basketballcards”. After gathering the responses, the open-ended answers of both surveys were manually coded independently by two of the authors to arrive at higher-order constructs that render responses across the two surveys comparable.

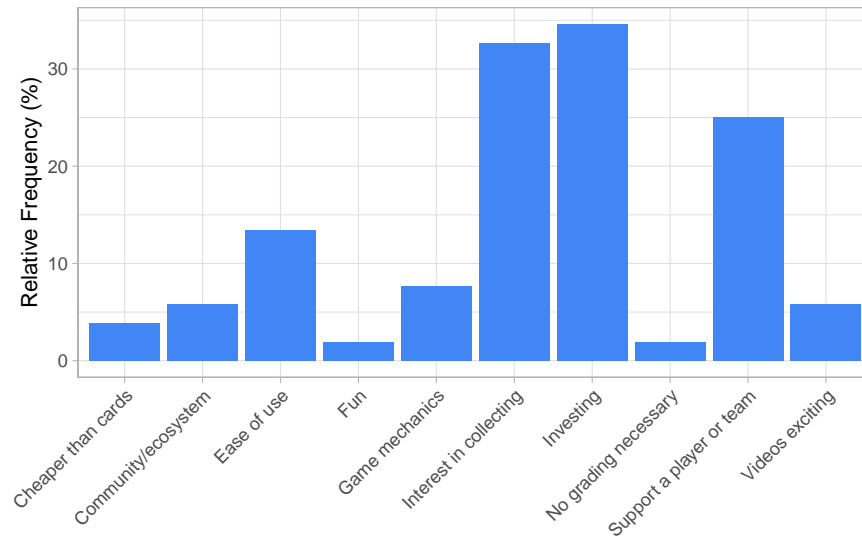
#### C.1.1. NTS Moment Collectors

We received valid responses from 56 participants. For NTS moments, Redditors indicated *Investing* (34.62%) as the most frequent purchase reason, followed by *Interest in collecting* (32.69%), and *Support a player or team* (25%) as we see in Figure 15a below. For example, the first two constructs are reflected in the following answer of a participant [sic throughout]: “As a collectable, I’ve always been a collector; cards, comics, CD, Records. and almost as a stock of that NBA player; betting that the price will rise and I can sell for more money.”. Redditors also indicated *Ease of use* (13.46%), followed by *Game mechanics* (7.69%), and *Community/ecosystem* (5.77%) as reasons for purchasing moments. Here, participants stated, for instance, that “*Potential prospects for utility in the NBA ecosystem (live events, merchandise, team benefits) or use in a video game.*” or “*it’s easy to use and widely accessible.*” as purchase reasons.

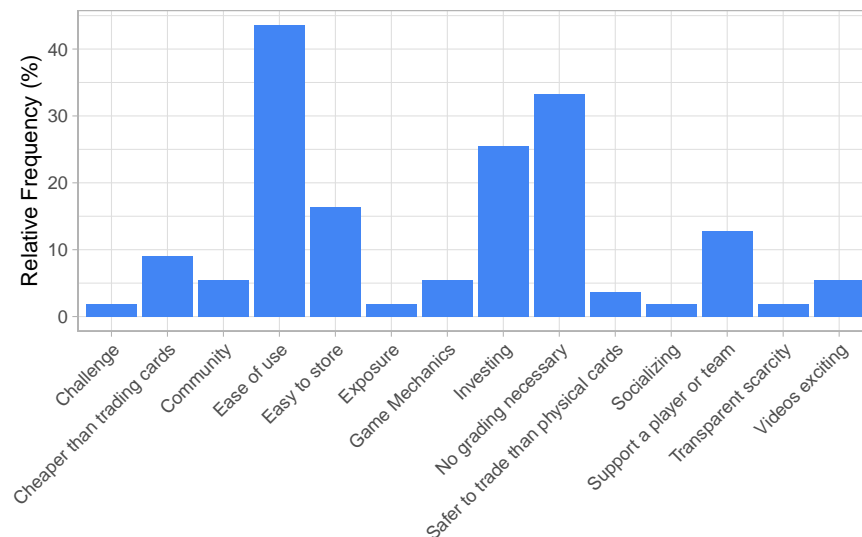
Regarding reasons why collectors prefer NTS moments over trading cards, the Redditors’ answers were also very informative. As we see in Figure 15b, they prefer moments over cards because the system makes it easier to buy and sell moments (compared to cards) (*Ease of use* - 43.64%), that grading is not necessary (*No grading necessary* - 33.3%), that moments are much easier to store (*Easy to store* - 16.36%), and also that they are *Safer to trade than physical*

cards (3.64%). The following two statements provide a good summary of these reasons: (1) “Easier to buy/sell. The blockchain technology helps buyer’s [sic] know they are getting the real item and sellers dont [sic] need to go through grading and shipping. Storage for physical cards is also an issue.” and (2) “They don’t take up the physical space that a card collection does. Don’t have to worry about grading/fake cards/shady sellers. I prefer a moment (video) to a still image.”. Finally, collectors also referred to *Investing* (25.54%) as another reason to prefer moments over cards.

**Figure 15** Reddit - NTS Moment Collectors.



(a) Purchase Reasons Moments.



(b) Reasons to Prefer Moments Over Cards.

### C.1.2. Basketball Trading Card Collectors

Next, we turn to the survey geared towards basketball trading card collectors, which was posted to the subreddit “r/basketballcards”. We received valid responses from 18 participants. Card collectors indicated that their main reasons for collecting cards are *Support a player or team* (33.3%), *Fun* (27.8%), *Interest in trading cards* (16.7%), and *Investing* (11.1%). These results are available in Figure 16a that follows. Moreover, when asked why they prefer cards over moments (Figure 16b), card collectors mentioned that they are *Skeptical about digital* collectibles (50%), that they prefer the tangible element (*Tangibility* - 42.85%) of cards, and that they can brag better with cards (*Bragging* - 7.14%).

In summary, when comparing the reasons for buying NTS moments (Figure 15a) with the reasons for buying physical cards (Figure 16a), we can see that *Investing* is a more important reason for NTS buyers than for card buyers. Moreover, when NTS buyers were asked what they like better about NTS compared to cards, they mentioned the *Ease of use* of the trading system, that there is *No grading necessary* as well as the fact that they are *Easy to store* and *Safer to trade than physical cards*. This aligns well with the characteristics that we analyze in section 6.3 and that current key traditional players are attempting to integrate into their ecosystem, as we mention in section 9.3.

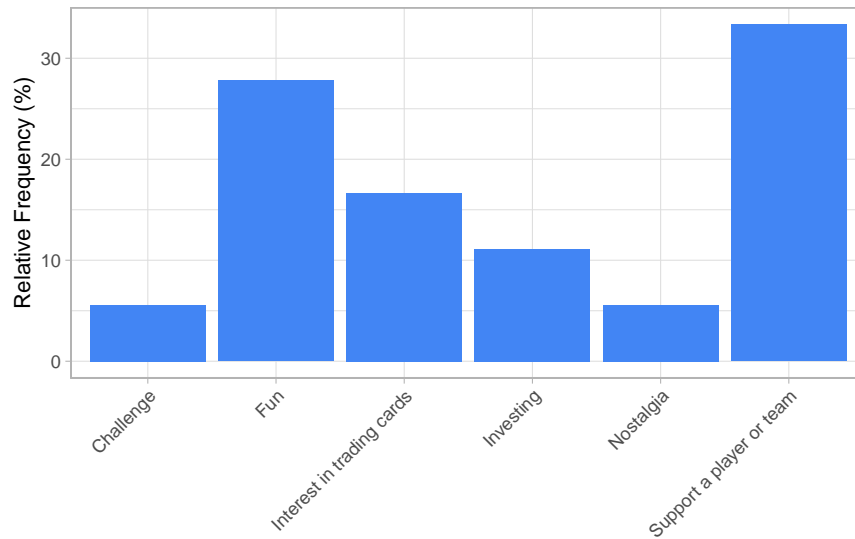
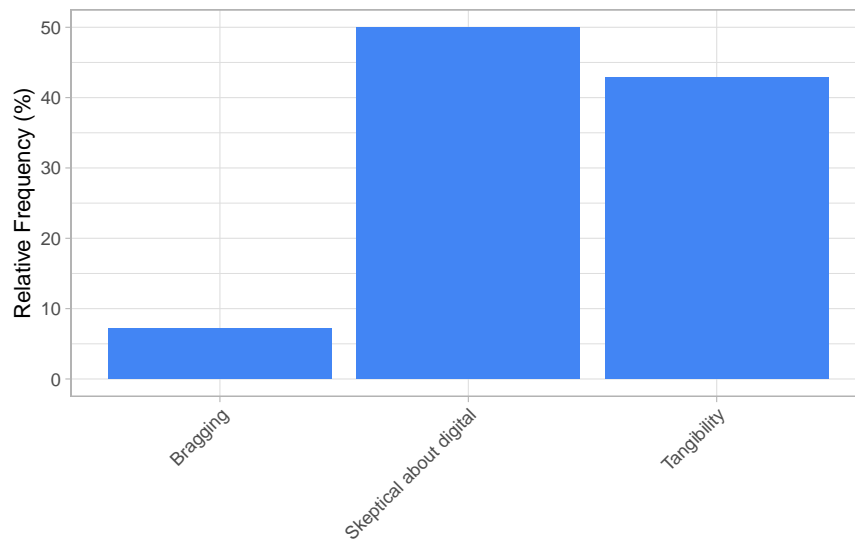
Moreover, across the two Reddit surveys, the participants were very comparable with regards to their age (Moments: 30.05 years, Cards: 29.69 years). Nonetheless, they differed somewhat with regards to which gender they identified as cards collectors were more skewed towards the male side (Moments: male - 84%, female - 16%, other - 0%; Cards: male - 95%, female - 5%, other - 0%).

We triangulate the demographics data of the two consumer groups with additional evidence from “www.similarweb.com”. This is a web analytics company that offers insight for the web traffic and performance of websites. For the consumer group of NTS collectors, we analyze the demographics data of NTS (“nbatopshot.com”), while for that of sports trading card collectors, we analyze the demographics of PWCC (“pwccmarketplace.com”). In both cases, we analyze the visitors for the three-month period from August 2022 to October 2022. A comparison of the two groups’ demographics suggests that the visitors to both websites are similar across age and gender. If anything, NFT collectors are slightly younger than card collectors.

## C.2. Amazon Mechanical Turk Survey

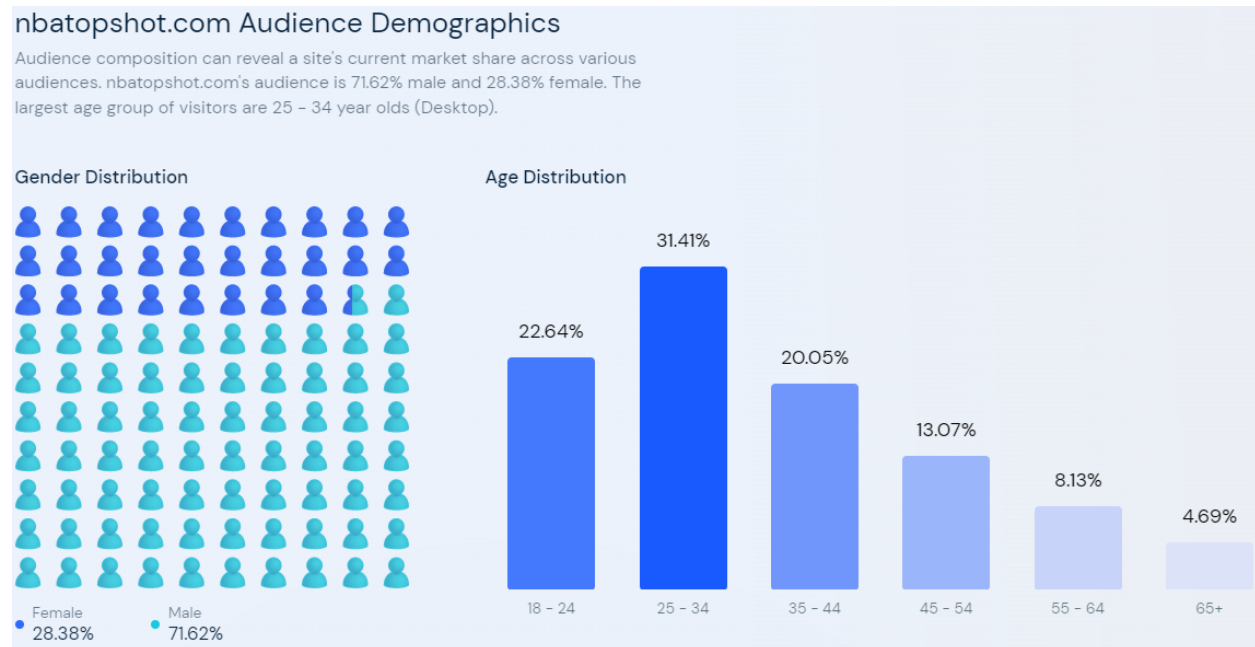
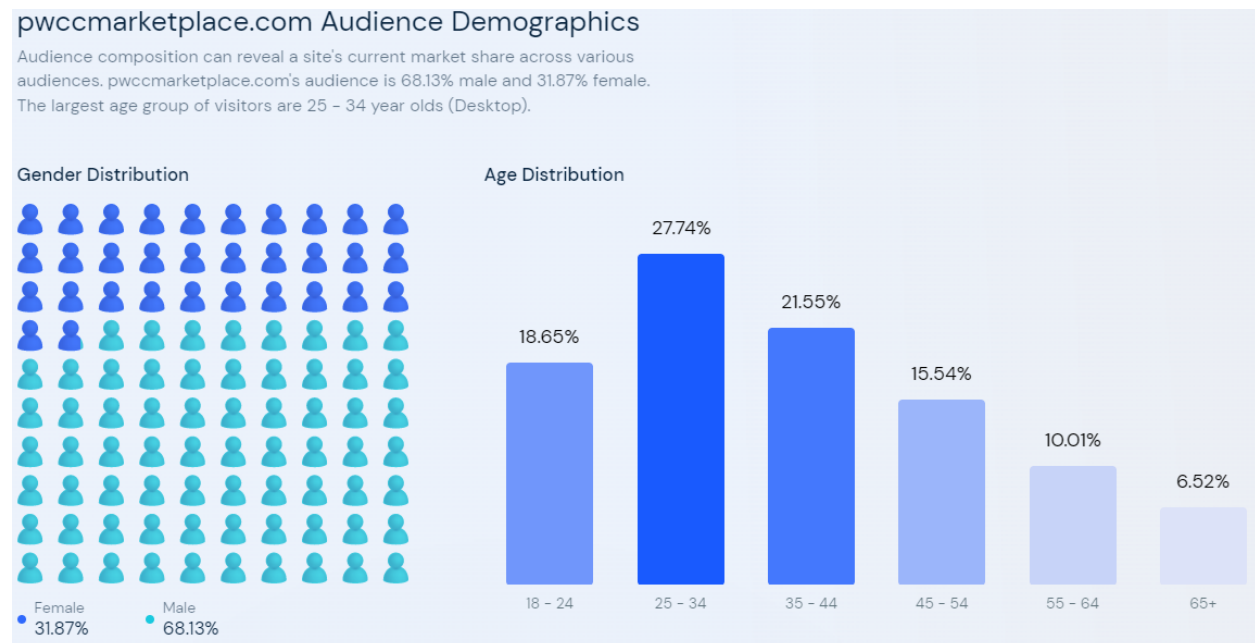
In the second step, we used the results of the Reddit survey to inform the design of a closed-ended survey, which we distributed on Amazon Mechanical Turk. We did so to ask the participants in this survey more focused questions about specific purchase reasons that emerged during the Reddit survey and to increase the sample size. We used the survey questions displayed in Figure 18. Additionally, we asked the participants whether they had ever purchased an NTS moment or a basketball trading card in the past.

Notably, each participant was asked both sets of questions so we could compare differences in purchase reasons within the same participant (rather than only between participants, as in the Reddit survey). We named our AMT HIT (Human Intelligence Task) “*Help us understand buyers of collectible items*” to attract workers with experience in the domain of collectibles. Furthermore, we set requirements so that our survey is presented only to AMT workers who fulfill three conditions: (1) they have completed at least one HIT, (2) their percentage of approved HITs is greater than 95%, and (3) they are located in the US. The first two conditions were designed to ensure that our respondents were less likely to be bots and more likely to be long-tenured AMTurkers who provide valid answers. The purpose of the

**Figure 16** Reddit - Basketball Trading Card Collectors**(a) Purchase Reasons Cards.****(b) Reasons to Prefer Cards Over Moments.**

third condition was to recruit respondents who are more likely to be familiar with sports trading cards and moments. Additionally, the last condition allows us to gather responses that more closely align with our eBay 2021 data (which contains only listings that could have been shipped to the US). After testing the survey duration through a pre-test, we decided to give each respondent 2 minutes to complete it. The average response duration was about 59 seconds, and no respondent failed to complete the survey on time.

For the NTS moment (sports trading card) analysis, we reduced the sample to those participants who indicated having bought an NTS moment (123 participants) (sports trading card (182 participants)) in the past. The results are

**Figure 17 Audience Demographics Data (Extracted from Similarweb).****(a) Moments (NTS).****(b) Cards (PWCC).**

displayed in Figure 19. We can see that slightly more participants indicated that they would buy moments (25.2%) than cards (23.62%) for *Investing* reasons. Thus, similar to the Reddit survey, monetary utility appears to be more prevalent among moment collectors than among card collectors. Moreover, participants also reconfirmed that they prefer moments because they are cheaper than cards, safer to trade, and do not require grading. Finally, we can glean another interesting observation from the data. The participants who indicated having bought an NTS moment in the

**Figure 18** AMT Survey.**Physical Collectibles**

1. On a scale of 1-7, how interested are you in buying sports trading cards?

\_\_\_\_\_ 1

2. Have you ever bought a sports trading card?

- Yes  
 No

3. What reason(s) did you have to buy a sports trading card?  
 If you have never purchased a sports trading card, move on to the next question.

- Easy to collect  
 Tangibility  
 Uniqueness  
 Nostalgia  
 Fun  
 Investing  
 Socializing  
 Gifting  
 Decoration

Here you can enter any other reasons you can think of. \_\_\_\_\_

**Digital Collectibles**

4. On a scale of 1-7, how interested are you in buying NBA TopShot moments?

\_\_\_\_\_ 1

5. Have you ever bought an NBA TopShot moment?

- Yes  
 No

6. What reason(s) did you have to buy an NBA TopShot moment?  
 If you have never purchased an NBA TopShot moment, move on to the next question.

- Easy to use  
 No grading is required  
 Easy to store  
 Investing  
 Support my favourite team/player  
 Community  
 Cheaper than sports trading cards  
 Fun  
 Safer to trade than sports trading cards

Here you can enter any other reasons you can think of. \_\_\_\_\_

**Both Collectibles**

Please answer the following question; if you have bought both a sports trading card and an NBA TopShot moment, hot moment. If this does not apply to you, move on to question 8.

7. Which of the two did you buy first, the sports trading card or the NBA TopShot moment?

- The sports trading card  
 The NBA TopShot moment

**Demographics**

8. How old are you?

Please enter your age as a number here. \_\_\_\_\_

9. What is your gender??

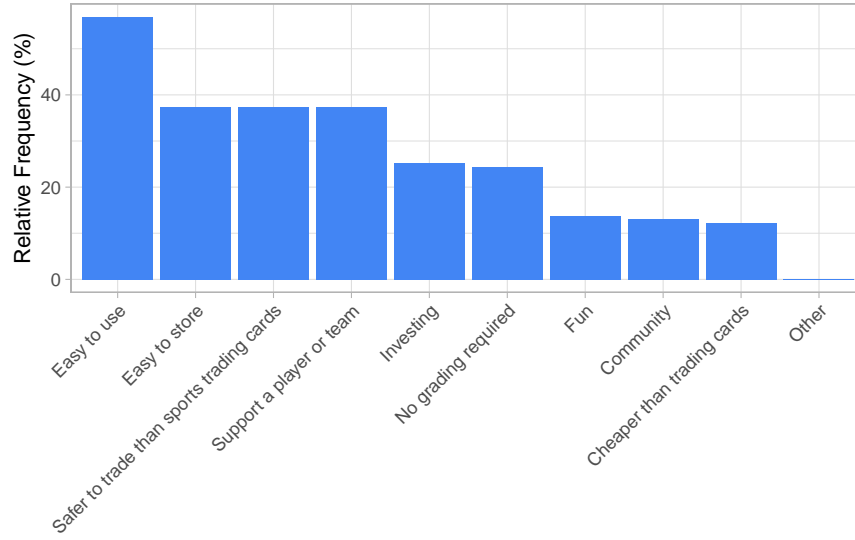
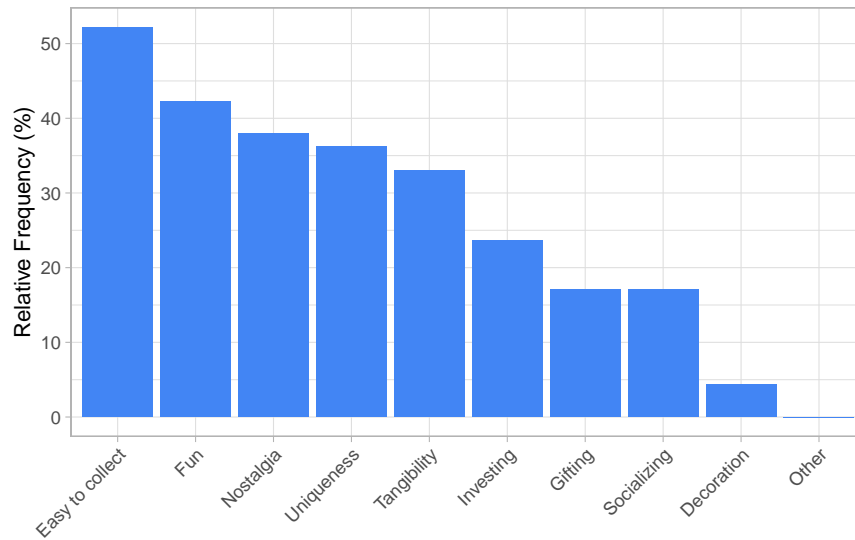
- Male  
 Female  
 Other

**Submit**

past are almost a perfect subset of those participants who indicated having bought a sports trading card. Only three participants indicated having bought a moment but did not report having bought a sports trading card. This substantiates the assumption that collectors of moments are interested in cards, which provides some evidence to support the presence of a market expansion effect.

To gather further evidence on this, we analyzed the responses to question 7. This question was only answered by the self-reported collectors of both cards and moments - this is 120 respondents. The question was “Which of the two did you buy first, the sports trading card or the NBA TopShot moment?”. We found that 34.17% of the respondents who have collected both cards and moments reported that they bought a moment first and a card afterwards. This indicates that some collectors of moments move towards cards which highlights that digital collectibles can introduce *new* customers into the world of collectibles and subsequently physical collectibles. Therefore, it provides further anecdotal evidence that supports the presence of a market expansion effect.

We also compare the demographics of participants and find those who indicated having bought moments were very comparable with those who indicated having bought cards with regards to age (Moments: 35.16 years, Cards: 37.36 years) as well as self-identified gender (Moments: male - 54%, female - 46%, other - 0%; Cards: male - 53%, female - 46%, other - 1%). Finally, we note that the AMT survey results should be interpreted with caution. Even though the results are quite consistent with the Reddit results, we could not verify whether participants had in fact purchased an NTS moment or a sports trading card in the past.

**Figure 19 AMT - Purchase Reasons.****(a) Moments.****(b) Cards.**

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