Controlling for Entry Threat Effects in Merger Analysis*

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Abstract: Although peripheral markets (i.e. those in which *neither* of the merging firms currently competes) often comprise the control group in merger retrospectives, they can experience considerable merger-induced changes in the probability of new entry, which may in turn influence the accuracy and robustness of inference. Using data on four large mergers in the U.S. airline industry, we demonstrate the potential for incorrect and/or inconsistent inference when entry threat effects are ignored, and we provide a new approach to selecting the control group that yields more precise and more robust results.

Keywords: Mergers; Merger Retrospectives; Antitrust; Threat of Entry
JEL Codes: L41; L11; D43; L93

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1 Introduction

The antitrust literature is now replete with merger retrospectives examining the effects of mergers on various measures of market performance. Motivated by both oligopoly theory and antitrust policy, these studies typically focus on overlap markets (i.e. those where the merging companies competed with one another prior to the merger), frequently designating peripheral markets (i.e., those in which *neither* merging firm is an incumbent) to be a major part of the control group in a two-way fixed effects (i.e. difference-in-differences, or DiD) model.[1] While it seems natural to assume that such markets should be unaffected by the merger, Kim and Mazur (2022) suggest that this assumption may fail when the merger in question alters the merging firms' threat of entry into peripheral markets. Moreover, because the direction of this effect is found to be heterogeneous across both markets and mergers, understanding and controlling for it seems essential to the robustness of standard merger retrospective results. This paper offers an easy-to-use approach for both assessing and controlling for any merger-induced entry-threat effects. We demonstrate the effectiveness of our approach using U.S. airline data, finding that results are both more precise and more robust to alternative methodologies.

Beyond improving the precision of existing estimates, our approach naturally improves accuracy as well. Merger retrospectives using peripheral markets as controls may very well be under- or over-estimating the true effects of mergers in overlap markets.[2] Whereas Kim and Mazur (2022) find consistent evidence that peripheral markets are indeed affected by mergers, the present paper offers guidance on how to control for this effect when evaluating the direct effects of a merger (i.e. upon overlap markets).

Using data on four recent mergers in the U.S. airline industry, we examine whether the relationship between prices and merger-induced changes in entry probability identified by

[1] See, for example, Carlton et al. (2019) and Ashenfelter and Hosken (2010).

[2] Airline studies seem to be particularly sensitive to control group selection and methodology, as highlighted by Mehta and Miller (2012) and Orchinik and Remer (2020).

Kim and Mazur (2022) matters for inference in merger retrospective analysis.[3] As a first pass at answering this question, we simply isolate a set of peripheral markets that can reasonably serve as a more "pure" control group, namely, those with entry probability changes of less than 5% in absolute value, which we deem to be minimally affected by the merger. We then examine changes in log prices on the remaining markets before and after the merger, using these minimally affected markets as controls. Based on this simple analysis, we find that ignoring the effect of a given merger on entry probabilities in peripheral markets may, but does not always, lead to incorrect inference. A more thorough approach, which we are working on now, would be to conduct our own merger retrospective for each of the four mergers using three different sets of markets for the control group. The first set would include peripheral markets plus intermediate markets; the second would include only the set of peripheral markets; and the third would include only those peripheral markets which experience a minimal change in the threat of entry. Comparing these three analysis would reveal more systematically the effect that ignoring entry threats can have on merger retrospectives. Moreover, examining the robustness of each of these specifications to methodological variation is an important part of this analysis, as we expect that accounting for entry threat changes will result in estimates that are more robust to variation in methodology and control group. As of this writing, that work is still in progress.

Because peripheral markets are precisely those which typically comprise the control group in merger retrospective studies, our paper highlights the need to think very critically about that control group.[4] In many cases, the overall effect of the merger may be robust to controlling for changes in entry probability - for example, if the overall change in entry probabilities across control markets aggregates to a non-effect. However, we stress that if the entry threat effect differs significantly and systematically from the direct effect of

[3]Just because we have found a relationship between prices and entry probability changes does not automatically imply that prices in peripheral markets are affected by mergers in the aggregate. For instance, it may be that the variation in entry probability changes washes out, resulting in an overall non-effect on price.

[4]We are by no means the first to highlight the importance of control group selection. In airline studies specifically, Mehta and Miller (2012) and Orchinik and Remer (2020) drive this point home, and Carlton et al. (2019)'s discussion provides a helpful summary.

the merger[5], then inference will be quantitatively muddied, and potentially qualitatively misleading.[6] For example, suppose that, relative to a hypothetical, "pure" control group, a particular merger reduces prices by 2% in overlap markets (the "treatment" group) and reduces prices by 4% in peripheral markets (the "control" group). In this case, the researcher would observe what appears to be a merger-induced price increase of 2%, and the researcher would be exactly wrong. The key mistake in this hypothetical scenario is assuming the "control" markets' trend represents the counterfactual trend for the "treatment" markets, when in fact the "control" markets experience a *different* treatment, namely, the merger's entry threat effect. We demonstrate that controlling for changes in the likelihood of potential entry can, but does not always, yield quantitatively different results in analyses of a merger's effect on prices in overlap markets. We therefore expect our findings to lead to more accurate inference in future merger retrospective studies.

In addition to more accurate inference, we expect that accounting for entry threat effects will also yield more robust inference. We show that, because of the high degree of heterogeneity across markets in both the composition and quality effects, studies which use peripheral markets as the control group are likely to suffer from a lack of robustness in their findings, either across mergers of the same type or across methodologies in analyses of the same merger. Specifically in airline merger retrospectives, this pattern is illustrated in work by Mehta and Miller (2012) and Orchinik and Remer (2020), who find evidence of substantial variation in results due to changes in control group and/or methodology. We intend to show that some of these inconsistencies can be explained by variation in the entry threat effects of a merger, such that controlling for a merger's effects on the likelihood of entry can yield

[5]Specifically, if the two effects run counter to one another, or if the control group's effect is larger in magnitude than the treatment group's effect.

[6]Note that if a particular merger affects treatment and control markets in a similar manner, but affects control markets to a lesser degree, then the study's conclusions will appear, if anything, conservative, and certainly qualitatively accurate. This is the main thrust of Dix and Orzach (2021), for example, which suggests that the direct effect of a merger on non-stop markets understates the full direct effect of the merger. Absent a theoretical basis for a *different* mechanism of effect, the assumption that results are conservative seems a fair one to make, in which case concerns over a lack of absolute "purity" in the control group would be minor. We argue in this paper that such a mechanism does exist, and that it has solid theoretical foundations.

more robust results.[7]

2 Literature Review

In this section, we review the merger retrospectives literature, which concerns the empirical study of merger effects, especially upon overlap markets.

2.1 Price Effects in Traditional Retrospectives

Difference-in-differences analysis has become a common tool of retrospective merger analysis.[8] The most basic approach compares the change in prices for products/markets most directly affected by the merger (i.e. the treatment group) to the change in prices for similar products/markets believed to be unaffected by the merger (i.e. the control group). In this setup, the important assumption identifying the effect of the merger on prices is known as the parallel trends assumption, namely, that the treatment and control groups would share a common time trend were it not for the merger. A rather large empirical literature on the price effects of mergers has arisen along these lines (e.g. Ashenfelter and Hosken (2010), Ashenfelter et al., 2013). At the end of this article, we provide a tabular summary of relevant studies. While much of the work has validated the commonplace assertion that a reduction in the number of competitors tends to increase the market price, other studies have identified price reductions, quality enhancements, and other related effects. Of particular relevance to our study are Werden et al. (1991), Borenstein (1990), Kim and Singal (1993), Peters (2006), Mazur (2015), Das (2019), Carlton et al. (2019), and others, all of which document the price effects of airline mergers in various contexts and with varying controls. In fact, even the variation itself has gained substantial attention, at least within the context of airline merg-

[7]This aspect of the analysis is ongoing.

[8]The U.S. Federal Trade Commission has even undertaken to formalize a research program dedicated to such studies. For an overview of that long-running program and some helpful references to this stream of literature, visit the Commission's website here: `https://www.ftc.gov/policy/studies/merger-retrospective-program/overview`

ers. Mehta and Miller (2012) analyze the Delta/Northwest merger of 2008 using a variety of difference-in-differences-based approaches, demonstrating that the choice of control group can "affect substantially the implied inferences." Orchinik and Remer (2020) follow up with similar evidence indicating that variation in methodology can produce results which yield very different conclusions about a merger's effect on prices. We hope our work in this paper can help shed some light on the reasons why airline mergers experience such wide variation in results. Most of these studies assume that the effect of the merger upon peripheral markets is nil, which is why such markets are often used as the control group in studies like these. However, if one recognizes that there may be entry threat effects in peripheral markets, it becomes quite hard to imagine a suitable alternative control group. By quantifying the market-level change in potential entry probability due to the merger, our paper provides a way of isolating a clean(er) set of control markets for merger retrospective studies, or at the very least controlling for the entry threat effect of the merger. In future drafts, we hope to demonstrate improvements in the consistency of findings as a result of accounting for these effects.

2.2 Other Effects in Traditional Retrospectives

In addition to their effects on prices, mergers can have significant effects upon product quality and other attributes, product mix, entry, exit, and more. These are important effects that deserve rigorous study, and other authors have made great strides along these lines. See, for example, Fan (2013), Mazzeo et al. (2018), and Rossi (2020), and Li et al. (2022). While we do not treat the direct effects of mergers on non-price metrics in this paper, we do recognize that accounting for such changes (e.g. as in Carlton et al. (2019)) can significantly influence results, and we look forward to documenting other changes in future iterations of this paper.

3 Methodology

As laid out in Kim and Mazur (2022), our basic analytical approach proceeds in two stages for each merger. In the first stage, we estimate a flexible probit model of entry using pre-merger data for both merging carriers. Using those estimates, we predict the probability of entry for the merged carrier, which we then use to calculate the overall change in the probability of entry, as well as its component effects (i.e. the composition and quality effects). In the second stage, we regress logged mean prices on these changes in entry probability, allowing them to have effects that vary with the pre-merger probability of entry. Doing so allows us to directly connect the change in entry threat to the change in prices, which we can then aggregate up to an overall price effect for each merger. Our main approach in the second stage is similar to a two-way fixed effects (or difference-in-differences) model with a continuous treatment effect, namely, the change in entry threat due to the merger.[9]

Following Kim and Mazur (2022), we define a potential entrant probabilistically, rather than assigning a binary definition of potential entrant status. A carrier's status as a potential entrant is a continuous variable, defined as its likelihood of entry, on the interval [0,1]. Adapting the approach of Sweeting et al. (2020), Dafny (2005), and Ellison and Ellison (2011), we estimate these probabilities with a probit model.

3.1 First Stage: Probit Model of Entry

In the first stage of our analysis, estimate a probit model of entry on a pre-defined set of markets. Defining a market to be any non-directional pair of airports, we include in our sample every market between the largest 125 airports in the contiguous U.S., regardless of whether or not that market is actually served.[10] Next we define entry entry as a binary variable equal to one if, in any of the next four quarters, a given carrier flies more than 20

[9] In future drafts, we hope to address the potential bias discussed in Goodman-Bacon (2019) by employing Callaway and Sant'Anna (2020) and/or Callaway et al. (2021) in robustness checks.

[10] We tabulate traffic using data for quarter 2 of the year 2000, keeping all airports that account for at least 0.05% of total domestic U.S. traffic. After dropping airports outside the contiguous U.S., our set of markets continues to account for about 90% of all domestic passenger flight traffic in the U.S.

ticketed DB1B passengers (roughly equivalent to 200 actual passengers) between those two endpoints, regardless of initial origin or final destination.

Our set of explanatory variables includes the following: airport presence at both endpoints and the geometric average presence across those endpoints; non-stop flight distance between the two endpoints; an indicator for long distance routes (round-trip distance greater than 2000 miles); geometric average population across endpoints; an indicator for the presence of slot constraints on the market[11]; indicators for year and quarter; and various polynomials and interactions of the aforementioned variables. For details on how each of these elements is computed and the data cleaned, please refer to the next main section of the paper.

With variables so defined, we next estimate a separate probit for each merger, using only the entry decisions of the merging firms in the pre-merger period, and treating all coefficients as common to both firms. This decision enables us to re-compute entry probabilities for the merged firm using *pro forma* presence values at each endpoint airport that are equal to the sum of presence for each merging carrier in the pre-merger period.[12] Just as important, this decision allows us to refrain from speculating on how the merger might change the merged carrier's overall network structure.

Lastly, we compute the relevant probability figures to be used in the next stage. Each merging carrier's entry probability is computed as the probit estimation's predicted value. Per the previous subsection, the baseline (i.e. pre-merger) entry probability is defined to be the maximum of these values among the two merging carriers. The overall change in entry probability, the composition effect, and the quality effect, are computed as described in the previous subsection, using the probit estimation's predicted values, but replacing the endpoint presence values with *pro forma* endpoint presence values equal to the sum of the merging firms' values. To reduce noise in these figures due to seasonal variation in

[11] Slot-controlled airports include John F. Kennedy International Airport (JFK), Newark Liberty International Airport (EWR), LaGuardia Airport (LGA) and Washington Reagan National Airport (DCA). Slot restrictions at Chicago O'Hare (ORD) expired in 2000 and are accounted for in our sample.

[12] Note that our set of explanatory variables must therefore deliberately exclude things like carrier-market dummies or hub indicators. We believe much of the impact of such carrier-market variables is accounted for by presence alone.

overall entry activity, we average these effects over the four quarters prior to the merger's announcement.

3.2 Second Stage: Difference-in-Differences

In the second stage, we regress logged average prices upon the measures computed from the first-stage estimation to examine how incumbent airlines respond to potential changes in market structure. Our setup is similar to the difference-in-differences approach with a continuous treatment effect used by Sweeting et al. (2020).

In future iterations, we will perform a set of robustness checks applying Callaway and Sant'Anna (2021) and Callaway et al. (2021) to mitigate the concern on the two-way fixed effect bias discussed in Goodman-Bacon (2021).

We examine the impact of the probability of entry induced by a merger on incumbents who are not engaged in the merger in a route facing the entry threat. Hence, endogeneity is less of a concern. However, one might be concerned that the control variables of the treated group are imbalanced compared to those of the comparison group due to potential conditional selection that enters into the treatment. That is, the markets threatened by the merging airlines might be systematically different from the markets not threatened by them. The merging carriers might select and consider entry to a certain set of routes conditional on the covariates. To alleviate this concern, as in Abadie (2005), we intend to match our comparison group in terms of propensity score and test parallel trends conditional on some set of covariates. We will use the average of the covariate matrix before the treatment to perform this test.

One might be concerned that the treatment, entry threat due to a merger, may occur at different times for the units that are exposed to the treatment. This creates a concern that the average treatment effect on the treated varies over the time period and potentially some previously treated units by other mergers can enter the comparison group. On a related note, one might be concerned if there were a more pronounced long-run treatment effect as well.[13] To mitigate these concerns, future iterations of this paper will perform robustness checks employing Callaway et al. (2021) and/or Callaway and Sant'Anna (2021).

4 Data Description

4.1 Data Sources and Sample Construction

We study the following mergers announced between 2008 and 2013: Delta-Northwest ("DLNW") was announced 4/2008; United-Continental ("UACO") was announced 5/2010; Southwest-Air Tran Airways was announced 9/2010; and American Airlines-US Airways was announced 2/2013.[14] For the purposes of our study, we treat the merger announcement date as the point at which the entry probabilities of the merging carriers changed.

Fare data come from the U.S. Department of Transportation's (DOT) Origin and Destination Survey, Data Bank 1B, hereafter referred to as DB1B, which represents a 10% sample of domestic U.S. airline tickets, aggregated quarterly from 1993 to the present.[15] This data set has been frequently employed in previous studies on the U.S. airline industry.

In constructing the data set, we focus exclusively on direct, round-trip fares, representing about 40% of overall domestic passengers carried in a typical year.[16] We next drop itineraries with different ticketing carriers in each direction, and we also eliminate duplicates and fares not deemed "credible" by the DOT. We combine subsidiaries with their parent companies as well as any commonly owned carriers, such as result from the close of a merger.[17] For our primary specification, we ignore variation in fare class and operating carrier. Thus, all direct, round-trip fares on a given directional market, ticketed by a given carrier, during a given year and quarter, are treated as the same product.

[13]Refer to Goodman-Bacon (2021) for more detailed discussion.

[14]Our analysis can easily accommodate more mergers. In future drafts, we expect to include American Airlines-TWA; America West-US Airways; and Alaska-Virgin America.

[15]Available here: https://www.transtats.bts.gov/DataIndex.asp

[16]For example, in quarter 2 of 2019, out of 9.9 million passengers in the raw DB1B data, 3.9 million of them (or about 39%) flew on 2-coupon, round-trip flights.

[17]Data cleaning code is available from the authors upon request.

While we allow prices to depend on the direction of travel, for the purposes of quantifying entry, we define markets to be non-directional airport pairs, such that a flight from Chicago Midway (MDW) to Orlando International (MCI) is treated identically to a flight in the other direction, but would be distinct from a flight between Chicago O'Hare (ORD) and MCI. Our decision to analyze airport pairs instead of city pairs reflects our view of entry probability on a given market, which we estimate using airline presence at each endpoint airport as a key explanatory variable. Endpoint presence implies access to gates and other resources associated with providing service at a given airport, but has little bearing on an airline's cost of providing service at other geographically proximate airports.[18]. To construct our measure of airport presence, we sum all passengers enplaned and deplaned at each airport for each ticketing carrier in each year and quarter, using the raw DB1B data at the segment level. Defining presence in this way, we can easily compute the *pro forma* presence values for a merged firm by summing the values for each firm prior to the merger.

4.2 Descriptive Statistics

Table 4.2 presents the summary statistics of our sample based on the Delta-Northwest merger. Table 4.2 presents the summary statistics of our sample based on the United-Continental merger. Table 4.2 presents the summary statistics of our sample based on the American-US Airways merger. Table 4.2 presents the summary statistics of our sample based on the Southwest-AirTran merger.

[18] We recognize, of course, that heavy presence at one airport in a city reduces an airline's likelihood of entry into alternative airports in that city. That is, Southwest is unlikely to start flying out of ORD partly because it has an enormous presence at MDW. For now, we are excluding this information from our probit estimation.

Table 1: Summary Statistics: DL-NW

	(1)				
	Mean	Std. Dev.	Median	Min.	Max.
Market-level passengers (thousands)	15.35	17.93	8.27	0.20	117.95
HHI	0.25	0.19	0.20	0.00	1.00
N	1.57	0.77	1.00	1.00	4.00
Change in Entry Prob	0.04	0.06	0.03	-0.15	0.31
Composition Effect	-0.04	0.03	-0.03	-0.19	-0.00
Quality Effect	0.08	0.07	0.06	-0.04	0.41
Gini Coefficient of Fare	0.20	0.05	0.19	0.02	0.46
Std. dev. of Fare	170.01	82.59	154.98	16.10	619.39
Mean Fare	429.42	140.58	418.68	78.89	1408.39
Min. Fare	167.22	85.46	156.69	25.17	693.65
Max. Fare	1044.72	457.05	989.37	115.38	1999.96
25th Pctl. of Fare	310.02	103.69	297.96	45.86	1119.85
50th Pctl. of Fare	386.30	132.30	369.47	49.68	1740.18
75th Pctl. of Fare	513.02	187.51	484.90	49.68	1863.75
Origin pop. (millions)	4.43	4.60	2.41	0.12	18.82
Destination pop. (millions)	4.01	3.96	2.70	0.12	18.82
Geometric avg. pop. (millions)	3.19	2.26	2.53	0.30	15.61
Distance (thousands of miles)	1.61	1.05	1.35	0.17	5.38
Distance over 2000 miles	0.28	0.45	0.00	0.00	1.00
Slot-controlled airport	0.19	0.39	0.00	0.00	1.00
Observations	34487				

Table 2: Summary Statistics: UA-CO

	(1)				
	Mean	Std. Dev.	Median	Min.	Max.
Market-level passengers (thousands)	14.51	17.54	7.97	0.20	141.91
HHI	0.26	0.21	0.21	0.00	1.00
N	1.50	0.72	1.00	1.00	5.00
Change in Entry Prob	0.05	0.07	0.03	-0.33	0.40
Composition Effect	-0.03	0.03	-0.02	-0.16	-0.00
Quality Effect	0.07	0.08	0.05	-0.30	0.43
Gini Coefficient of Fare	0.20	0.05	0.19	-0.00	0.45
Std. dev. of Fare	170.48	79.48	154.40	0.00	663.85
Mean Fare	433.37	145.04	411.28	50.22	1461.36
Min. Fare	153.98	94.12	145.20	25.06	1112.30
Max. Fare	1062.02	433.83	1027.19	50.22	1999.83
25th Pctl. of Fare	316.90	107.12	302.76	25.80	1388.13
50th Pctl. of Fare	390.46	137.61	365.60	37.59	1747.47
75th Pctl. of Fare	513.20	190.66	474.81	37.59	1876.01
Origin pop. (millions)	4.78	4.91	3.18	0.12	18.82
Destination pop. (millions)	3.70	4.42	2.07	0.12	18.82
Geometric avg. pop. (millions)	3.16	2.31	2.36	0.31	15.61
Distance (thousands of miles)	1.74	1.06	1.52	0.19	5.44
Distance over 2000 miles	0.32	0.47	0.00	0.00	1.00
Slot-controlled airport	0.16	0.36	0.00	0.00	1.00
Observations	38797				

Table 3: Summary Statistics: AA-US

	(1)				
	Mean	Std. Dev.	Median	Min.	Max.
Market-level passengers (thousands)	10.88	13.89	5.49	0.20	94.12
HHI	0.27	0.21	0.22	0.00	1.00
N	1.32	0.57	1.00	1.00	4.00
Change in Entry Prob	0.05	0.09	0.05	-0.57	0.50
Composition Effect	-0.05	0.05	-0.04	-0.22	-0.00
Quality Effect	0.10	0.10	0.09	-0.36	0.56
Gini Coefficient of Fare	0.20	0.05	0.20	0.01	0.48
Std. dev. of Fare	189.52	83.12	171.58	3.29	643.72
Mean Fare	481.75	169.07	457.11	56.63	1327.45
Min. Fare	166.60	104.15	151.75	25.01	807.14
Max. Fare	1070.88	392.53	1054.90	104.01	1999.85
25th Pctl. of Fare	348.16	128.35	334.12	25.80	973.69
50th Pctl. of Fare	435.90	164.24	408.90	37.59	1498.85
75th Pctl. of Fare	583.46	228.71	533.19	37.59	1889.74
Origin pop. (millions)	4.58	4.71	2.94	0.21	18.82
Destination pop. (millions)	3.48	4.20	1.98	0.12	18.82
Geometric avg. pop. (millions)	2.95	2.19	2.23	0.31	15.61
Distance (thousands of miles)	1.52	0.93	1.29	0.19	5.19
Distance over 2000 miles	0.25	0.43	0.00	0.00	1.00
Slot-controlled airport	0.11	0.31	0.00	0.00	1.00
Observations	28662				

Table 4: Summary Statistics: WN-FL

	(1)				
	Mean	Std. Dev.	Median	Min.	Max.
Market-level passengers (thousands)	14.83	22.98	5.82	0.20	201.30
HHI	0.23	0.18	0.19	0.00	1.00
N	1.69	0.95	1.00	1.00	7.00
Change in Entry Prob	0.03	0.08	-0.00	-0.06	0.64
Composition Effect	-0.01	0.01	-0.01	-0.13	-0.00
Quality Effect	0.04	0.08	0.00	-0.02	0.65
Gini Coefficient of Fare	0.22	0.05	0.21	0.01	0.46
Std. dev. of Fare	224.01	93.82	215.29	11.39	702.70
Mean Fare	516.34	163.94	509.64	58.16	1408.39
Min. Fare	160.51	104.81	145.34	25.06	807.14
Max. Fare	1307.01	413.93	1311.97	102.79	1999.93
25th Pctl. of Fare	363.72	122.46	350.71	26.54	1277.00
50th Pctl. of Fare	458.89	163.72	436.47	38.60	1740.18
75th Pctl. of Fare	622.00	235.08	586.47	62.65	1889.74
Origin pop. (millions)	5.75	5.71	4.47	0.21	18.82
Destination pop. (millions)	4.90	5.14	3.18	0.12	18.82
Geometric avg. pop. (millions)	3.85	2.81	2.97	0.31	15.61
Distance (thousands of miles)	1.65	1.12	1.31	0.13	5.44
Distance over 2000 miles	0.29	0.45	0.00	0.00	1.00
Slot-controlled airport	0.40	0.49	0.00	0.00	1.00
Observations	36399				

5 Results

5.1 First Stage: Computing Changes in Entry Probabilities

Table 5.1 presents the results of our first-stage probit estimation of entry. Pseudo-R-squared values range from 0.24 to 0.36. Thus, they are slightly below but still roughly in line with the 0.37 value reported in Appendix D of Sweeting et al. (2020). As expected, presence at the origin and destination both have a positive impact on the likelihood of entry, while long distances reduce entry probability, all else equal. We expect our specification can be further improved to increase predictive accuracy. It is worth noting, however, that the present specifications all outperformed those that used binary indicators of potential entrant status in place of continuous presence measures. In other words, by at least one measure of predictive accuracy, our definition of potential entry status outperforms the traditional binary presence metric, in most cases by a considerable margin.

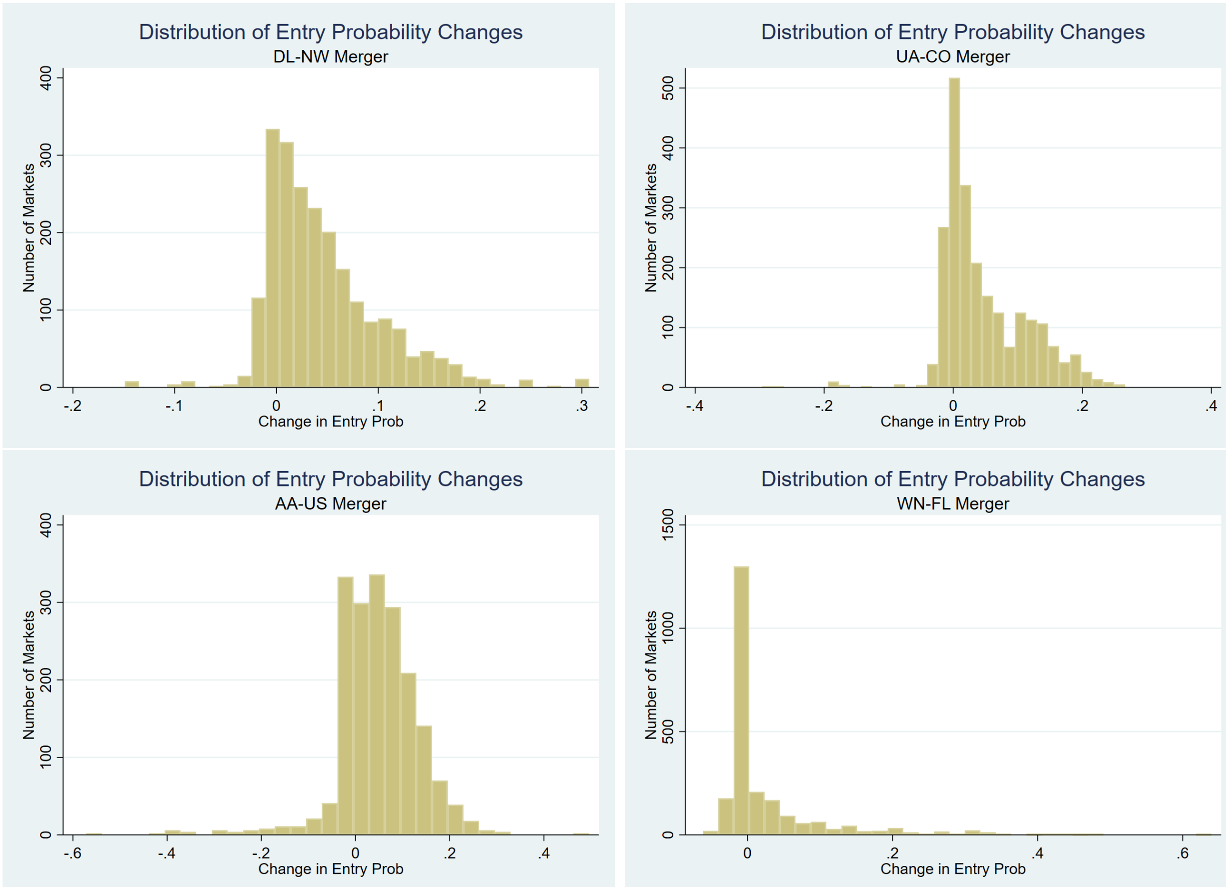
Using these estimates, we change endpoint presence values to account for the combination of the merging firms' activities at each endpoint, and compute the associated entry probability for the merged carrier. We then compute the overall change in entry probability due to the merger. Figure 1 presents the overall changes in market-level entry probabilities in peripheral markets for each merger. The main takeaway from this figure is that entry probability change is centered around zero, as one would expect, but has a great deal of variation, and includes non-trivial changes in likelihood of entry. To put these figures in context, consider that the inflection point in Sweeting et al. (2020)'s estimates of the fare effect of Southwest's likelihood of potential entry into monopoly markets occurs around 30% likelihood. Thus, modest changes on the order of 5% or 10% are likely to have a noticeable effect.

Table 5: Probit Estimation

	(1) DLNW	(2) UACO	(3) WNFL	(4) AAUS
Entry				
Distance	0.475***	-0.287***	1.339***	0.245***
	(0.030)	(0.027)	(0.039)	(0.030)
Distance^2	-0.274***	-0.033***	-0.568***	-0.159***
	(0.011)	(0.012)	(0.017)	(0.011)
Distance^3	0.037***	0.012***	0.064***	0.023***
	(0.001)	(0.002)	(0.002)	(0.001)
longdist	-0.016	-0.002	-0.041**	-0.008
	(0.012)	(0.014)	(0.018)	(0.011)
slot	0.168***	-0.003	-0.325***	0.175***
	(0.009)	(0.011)	(0.018)	(0.010)
Population (geometric average)	0.350***	0.389***	0.444***	0.298***
	(0.008)	(0.008)	(0.010)	(0.008)
Population^2	-0.036***	-0.041***	-0.050***	-0.014***
	(0.001)	(0.001)	(0.002)	(0.001)
Population^3	0.001***	0.001***	0.002***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Presence (geometric average)	0.074***	0.066***	0.072***	0.107***
	(0.003)	(0.002)	(0.002)	(0.002)
Presence^2	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Distance*Presence	0.035***	0.046***	0.007***	0.043***
	(0.002)	(0.002)	(0.002)	(0.002)
Distance*Presence^2	-0.000***	-0.000***	-0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Presence*Distance^2	-0.006***	-0.006***	-0.001***	-0.007***
	(0.000)	(0.000)	(0.000)	(0.000)
Presence$^2 * Distance^2$	0.000***	0.000***	0.000**	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Quarter FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	818727	948222	972193	1093032
Adjusted R^2				
Pseudo R^2	0.240	0.282	0.370	0.291

Population x presence interactions and separate origin and destination presence effects suppressed.

Figure 1: Distribution of Changes in Entry Probability



5.2 Second Stage: Price Effects of Merger-Induced Entry Threat

There is one glaring question we have not yet answered. Does any of this actually matter for the accuracy and robustness of inference in merger retrospectives? That is, given the wide range of baseline entry probabilities, the heterogeneity of merger-induced changes to them, and the non-monotonicity of their effect upon prices, might it all "wash out" in the end, leaving merger retrospective analysis intact? Tables 5.2 and 5.2 provide a first-pass at the answer in our context, and that answer is, "sometimes." Supposing that markets with a probability change of less than 5% in absolute value can be thought of as a "pure" control group, these tables estimate the effect of each merger on the remaining peripheral markets, which we might think of as being exposed to this secondary treatment effect (i.e. the entry threat effect). The choice to lump all such markets together into one treatment group is deliberately at odds with what we have already shown are substantial differences across markets in the entry threat effect on prices. Since our question is whether the control group we would have used - had we not read this paper - might lead to incorrect inferences of the effect of a merger on prices, Tables 5.2 and 5.2 simply tell us how far off our control group would have been. Our market-level specification yields three affected mergers out of four, whereas our carrier-market-level specification yields two affected, one of which was the unaffected merger in the market-level regressions. While we are still sorting out the reasons for the strange inconsistencies in significance, it is telling that in both specifications, all four mergers yield a negative coefficient on log price, usually in the range of 2-3%. Relative to the "pure" control group, the affected peripheral markets seem to experience a non-trivial decline in prices, suggesting that findings of anticompetitive effects for at least some of these airline mergers may be overstated.

We are quick to note, however, that this particular result is only a vague window into the actual effect. For one thing, because the control group used in other studies will include both our "pure" control markets and our affected markets, the actual impact on inference will be diluted relative to what is reported here. Just as importantly, the markets used here are not exactly the same as those that might be used in a true merger retrospective. Thus, to really answer this question, we need to conduct our own merger retrospectives, paying close attention to the control group along other dimensions (such as distance, number of competitors, etc.) and testing for methodological robustness, and then re-do that entire procedure using only "pure" control markets as candidates for the control group. That analysis is in the queue, and results are forthcoming!

Table 6: Market-Level Regressions

Merger Effect on Log(Price) in Peripheral Markets

	(1) DLNW	(2) UACO	(3) WNFL	(4) AAUS
Log of market-level passengers	-0.117***	-0.114***	-0.173***	-0.046***
	(0.014)	(0.013)	(0.020)	(0.018)
HHI	0.004	0.023***	0.002	0.009
	(0.009)	(0.006)	(0.010)	(0.010)
N	-0.054***	-0.043***	-0.026***	-0.090***
	(0.010)	(0.008)	(0.009)	(0.015)
affected	-0.025**	-0.026***	-0.020	-0.027**
	(0.011)	(0.008)	(0.014)	(0.011)
Constant	6.507***	6.422***	6.765***	6.768***
	(0.014)	(0.012)	(0.019)	(0.018)
Market FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	6700488	7228649	5135548	4376907
Adjusted R^2	0.914	0.894	0.874	0.887

Robust standard errors in parentheses, clustered at the market level, but not adjusted for first-stage estimation error.

Table 7: Market-Level Regressions

Merger Effect on Log(Price) in Peripheral Markets

	(1) DLNW	(2) UACO	(3) WNFL	(4) AAUS
Log of market-level passengers	-0.117***	-0.114***	-0.173***	-0.046**
	(0.015)	(0.012)	(0.017)	(0.019)
HHI	0.004	0.023***	0.002	0.009
	(0.008)	(0.008)	(0.009)	(0.010)
N	-0.054***	-0.043***	-0.026***	-0.090***
	(0.010)	(0.007)	(0.007)	(0.014)
affected	-0.025**	-0.026***	-0.020	-0.027**
	(0.011)	(0.007)	(0.014)	(0.012)
Constant	6.507***	6.422***	6.765***	6.768***
	(0.014)	(0.012)	(0.019)	(0.018)
Market FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	6700488	7228649	5135548	4376907
Adjusted R^2	0.914	0.894	0.874	0.887

Bootstrapped standard errors in parentheses. 100 bootstrap iterations were performed, with re-sampling clustered at origi

Table 8: Market-Carrier-Level Regressions

Merger Effect on Log(Price) in Peripheral Markets

	(1) DLNW	(2) UACO	(3) WNFL	(4) AAUS
Log of market-level passengers	-0.124***	-0.096***	-0.164***	-0.028*
	(0.014)	(0.013)	(0.029)	(0.017)
HHI	0.013	0.028***	0.014	0.029***
	(0.009)	(0.008)	(0.013)	(0.010)
N	-0.048***	-0.024***	-0.009	-0.054***
	(0.009)	(0.007)	(0.010)	(0.010)
affected	-0.013	-0.020**	-0.035**	-0.017
	(0.012)	(0.010)	(0.017)	(0.013)
Constant	6.501***	6.408***	6.751***	6.731***
	(0.012)	(0.012)	(0.021)	(0.015)
Carrier-Market FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	10868553	11518356	10694733	6109569
Adjusted R^2	0.911	0.904	0.881	0.923

Robust standard errors in parentheses, clustered at the market-carrier level, but not adjusted for first-stage estimation erro

Table 9: Market-Carrier-Level Regressions

Merger Effect on Log(Price) in Peripheral Markets

	(1) DLNW	(2) UACO	(3) WNFL	(4) AAUS
Log of market-level passengers	-0.124***	-0.096***	-0.164***	-0.028
	(0.014)	(0.012)	(0.024)	(0.018)
HHI	0.013*	0.028***	0.014	0.029**
	(0.008)	(0.008)	(0.013)	(0.012)
N	-0.048***	-0.024***	-0.009	-0.054***
	(0.010)	(0.006)	(0.009)	(0.012)
affected	-0.013	-0.020**	-0.035**	-0.017
	(0.014)	(0.009)	(0.016)	(0.011)
Constant	6.501***	6.408***	6.751***	6.731***
	(0.012)	(0.012)	(0.021)	(0.015)
Carrier-Market FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	10868553	11518356	10694733	6109569
Adjusted R^2	0.911	0.904	0.881	0.923

Bootstrapped standard errors in parentheses. 100 bootstrap iterations were performed, with re-sampling clustered at origi

6 Conclusions

To summarize, our paper examines the effects of four recent airline mergers on prices and the threat of entry in peripheral markets, where neither merging firm currently competes, finding a consistent and statistically significant pattern. We aggregate this "entry threat effect" across markets, finding that merger-induced changes the threat of new entry may, but do not always, lead to lower prices overall for what have heretofore been considered "control group" markets. The upshot of this alternative merger "treatment" is that some analyses of airline mergers may have overstated anticompetitive findings.

Our paper complements the existing empirical literature on the effects of mergers in overlap markets by quantifying the extent to which merger-induced changes in the likelihood of entry can result in lower prices in peripheral markets, the control group commonly used in merger retrospective studies. Moreover, our study may offer an explanation for and partial solution to the sensitivity of some difference-in-differences airline merger retrospective studies to changes in methodology or the set of markets chosen for the control group.

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Appendix

This section reserved for more detail on data construction, estimation, full regression results, etc. Full regression results contain far too many fixed effects to report here but are available upon request.

Year	Authors	Industry	Dependent Var.	Treatment	Treatment Group	Control Group
2019	Carlton et al.	Airlines	Price; output	UA/CO, DL/NW, AA/US mergers	Non-stop overlap markets with at most one other competitor, and at most 3 total competitors.	Non-stop comparable markets
2019	Carlton et al.	Airlines	Price; output	UA/CO, DL/NW, AA/US mergers	Connecting overlap markets with at most one other competitor, and at most 3 total competitors.	Connecting comparable markets
2022	Li et al.	Airlines	Price/Service Changes	DL/NW, UA/CO, AA/US, UN/US	Various post-merger service types	Service types fixed
2012	Mehta and Miller[a]	Airlines	Price	DL/NW	Matching estimators	All unaffected connect routes
2021	Dix and Orzach	Airlines	Price	DL/NW, UA/CO, WN/FL, US/HP, US/AA	Merged routes	Routes no longer offered
2020	Gil and Kim	Airlines	"on-time performance indicators"	DL/NW, UA/CO, AA/US	Merged routes	pre-merged incumbents
2008	Kwoka[b]	16 different, made up of petroleum, academic journals, airlines, hospitals, microfilm	Price	(Varied)	Mergers	All transactions

[a] This study was a bit unique as it focused less on actual price output, and more so on which method of control group was optimal
[b] Kwoka describes this study as a "meta-analysis" of available literature, so the results are less specific to particular mergers

Year	Authors	Industry	Dependent Var.	Treatment	Treatment Group	Control Group
2019	Le	Airlines	Price; Output	AA/US	Overlap Markets (Actual Competition)	Airlines neither AA or US services
2021	Ochinik and Remer[a]	Airlines	Price	DL/NW, UA/CO, WN/FL, AA/US	Overlap Routes	No Merging Firms
2021	Ochinik and Remer	Airlines	Price	DL/NW, UA/CO, WN/FL, AA/US	Overlap Routes	One Merging Firm
2010	Ashenfelter and Hosken	Retail brands	Price	Large range of products for each company	Other brands	”Private Label” brands
2010	Ashenfelter and Hosken	Retail brands	Price	Large range of products for each company	Other brands	Branded products that are close substitues to products owned by merging parties
2015	Mazur	Airlines	Price	DL/NW, UA/CO, WN/FL, AA/TW, US/HP	Concern Routes (those that show potential to raise market power)	No concern routes

[a] This paper was looking at methods of measuring effects of mergers, with one of three methods being the most common use of diff-in-diff techniques. These headings/descriptions reflect the section of the paper devoted to the first diff-in-diff technique

Year	Authors	Industry	Dependent Var.	Treatment	Treatment Group	Control Group
2022	Li et al.	Airlines	Price	DL/NW, UA/CO, AA/US, All UN/US, UA/US with AA connecting	Allow rival service changes	Service types fixed
2022	Li et al.	Airlines	Rivals	DL/NW, UA/CO, AA/US, All UN/US, UN/US with AA connecting	Allow rival service changes	Service types fixed
2005	Ivaldi and Verboven[a]	Automobile /Commercial Vehicles	Price; Consumer surplus	Volvo/Scania	5% cost efficiency	0% cost efficiency
2009	Brown and Gayle	Airlines	Price	DL/NW	Codeshared products between DL/NW	All products
2011	Weinberg	Consumer Products (Feminine Hygeine Products)	Price	Feminine products	simulated price changes	estimated price changes
1994	Werden and Froeb[b]	Long distance carriers	price; welfare	Simulated mergers between ATT, Sprint, MCI, and Minor	Logit estimated differences	[not completely clear]

[a] The main goal of this paper was to look at how merger analysis can be improved - the treatment/ control groups described were a small part of the overall analysis

[b] this paper focused on testing a method of estimation for price/welfare differences regarding mergers, but did not provide extremely clear test and results about real mergers, hence lack of clear control group

Year	Authors	Industry	Dependent Var.	Treatment	Treatment Group	Control Group
2021	Bontemps et al.	Airlines	Price; Consumer surplus	AA/US	Market when AA and US present	Neither present
2021	Bontemps et al.	Airlines	Price; Consumer surplus	AA/US	Market when AA or US present	Neither present
1992	Brueckner et al.	Airlines	Price	TWA/St. Louis, Ozark/St. Louis, Northwest/Minneapolis, Republic/Minneapolis	Post-Merger networks	original networks
2006	Peters	Airlines	Price	Northwest/Republic, TWA/Ozark, Continental/People Express, Delta/Western, US-Air/Piedmont	Simulated price changes	Observed price changes
2019	Das	AIrlines	Price; Quality	AA/US	Routes operated by either AA, US, or both	Routes not operated by either AA, US, or both
1990	Borentstein	Airlines	Price	Northwest/Republic, TWA/Ozark	Merging airlines	major airlines