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FIRMS' ANTI-COMPETITIVE EFFECTS AND SPILLOVERS ACROSS MARKETS

A DISSERTATION APPROVED FOR THE
DEPARTMENT OF ECONOMICS

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Abstract

The first chapter is a joint paper with my committee member as well as my mentor, Dr. Myongjin Kim and Dr. Kerry Tan Loyola from University Maryland. We study the impact of tacit collusion on price dispersion in the U.S. airline industry. We find that tacit collusion driven by multimarket contact has a positive effect on prices, but a negative effect on price dispersion. Our empirical results suggest that airfares throughout the price distribution increases, yet the price distribution becomes more compressed since 10th percentile airfares increase by a larger amount than 90th percentile airfares. Moreover, we also find that this pricing phenomenon does not exist if Southwest Airlines is present on the route. Thus, route-level price competition is softened when the same airlines directly compete more frequently, except when Southwest Airlines services that route. As such, our empirical analysis provides evidence that the presence of Southwest Airlines exhibits an anti-collusive effect.

Chapter 2 is a joint work with my mentor, Dr. Myongjin Kim and Dr. Qi Ge from Vassar College. This paper examines US airline mergers between 1993 and 2018 and studies their impact on the labor market. Our difference-in-differences estimates indicate a significant reduction in the merging airlines' long-term wage and fringe benefits following the mergers. The effect is particularly salient among large-scale mergers involving major airlines and low cost carriers. The results also suggest a negative short-term employment impact of mergers that varies by occupation types. Our find-

ings are consistent with the impact of merger-induced monopsony power discussed in recent literature and offer important policy implications regarding how to account for employer monopsony power during mergers and acquisitions.

In the third chapter which is a joint work with my best friends, Brent Norwood and Sean O'Connor, we identify the cross and inter-state effects of marijuana legalization on house prices using a national housing data set from the online real estate listing database Zillow.com. We find positive effects upwards of ten percent in the top half of the price distribution following successful legalization ballot initiatives, and between five and fifteen percent across the distribution after the state enacts the ballot initiative and the first legal sales take place. A spatial difference-in-differences model reveals that within Colorado and Washington, prices in neighborhoods with new dispensary openings nearby experience a seven percent price appreciation. To summarize, our results suggest that there are second order benefits to marijuana legalization.

Chapter 1

Tacit Collusion and Price Dispersion in the Presence of Southwest Airlines

1.1 Introduction

The airline industry has been the focus of empirical studies on price discrimination because two important prerequisites for firms to price discriminate are present in this market. First, customers have different demand elasticities since demand from business travelers is less price elastic than that of leisure travelers. Second, airlines are able to distinguish between these two types with certain ticket restrictions, including advance-purchase requirements, nonrefundable tickets, and Saturday night stay-overs.

The existing literature contains contrasting research on the effect of competition on price dispersion. On the one hand, Borenstein and Rose (1994) use cross-sectional data to find a positive effect of airline competition on price dispersion, whereas Gerardi and Shapiro (2009) use panel data to find that price dispersion decreases with competition.

More recently, Kim and Shen (2018) reconcile these results by showing that the outcome hinges on product differentiation and market definition. Using panel data from 1993 to 2013, they find that an increase in competition has a positive effect on price dispersion for one-way tickets, but a negative effect for round-trip tickets.

Typically, competition is proxied using the Herfindahl-Hirschman Index (HHI) or the number of firms in a market. However, a decrease in market concentration or an increase in the number of firms might not necessarily result in stronger price competition. One of the market conditions that could facilitate collusion is multimarket contact, in which rival firms compete head-to-head in a multitude of markets. Indeed, Ciliberto and Williams (2014) find evidence of tacit collusion in the airline industry since an increase in average multimarket contact is associated with higher average airfares.

The first main result of this paper is that multimarket contact has a negative effect on price dispersion. Consistent with Ciliberto and Williams (2014), we find that airlines with more multimarket contact are more likely to tacitly collude by raising average prices. We expand on their analysis by showing that price dispersion decreases since airlines raise their 10th percentile airfares (likely paid by leisure travelers) by a relatively higher amount than their 90th percentile airfares (likely paid by business travelers). Our second main result is that the presence of Southwest Airlines mitigates the effect of multimarket contact such that the evidence for tacit collusion occurs in markets that are not serviced by Southwest but disappears in markets operated by Southwest.

This paper contributes to the existing literature in two ways. First, we combine the empirical research on the relationship between price dispersion, competition, and multimarket contact. We achieve this by constructing new instrumental variables for average multimarket contact based on outsourcing agreements between major carriers and regional airlines. Second, we provide evidence that Southwest creates not only a pro-competitive effect on airfares but more interestingly the presence of Southwest also exhibits an anti-collusive effect in the airline industry. To the best of our knowledge, we are the first to document the chilling effect that Southwest has on tacit collusion.

1.2 Literature Review

Feinberg (1984) and Bernheim and Whinston (1990) serve as seminal papers on the theoretical work on the effect of multimarket contact and price competition. In particular, Feinberg (1984) discusses the mutual forbearance behavior, in which conglomerate firms take each other's actions into consideration when they compete in multiple markets together. Firms choose output independently, yet fear indirect retaliation in another market. In other words, multimarket contact is more likely to induce collusion. Moreover, Bernheim and Whinston (1990) posits that multimarket contact facilitates collusion under certain conditions of repeated competition. Although the mutual forbearance story is typically associated with conglomerates, the theory can be applied to multi-product firms, including companies that produce a single product in multiple geographic markets. They show that a number of factors (e.g. costs, market characteristics, and discount factor) determine whether prices can rise or fall due to multimarket contact.

Several empirical papers have applied the theory from Feinberg (1984) and Bernheim and Whinston (1990) to the airline industry. Evans and Kessides (1994) finds that average one-way airfares are higher in city-pair markets served by carriers with extensive multimarket contact, whereas Zou, Yu, and Dresner (2012) find that airline alliances mitigate the positive relationship between multimarket contact and airfares for transpacific routes. Instead, they find that higher airfares exist when airlines have greater multimarket contact on open-skies routes. Although most applied work examine the effect of multimarket contact on price competition, Prince and Simon (2009) and Bilotkach (2011) find that multimarket contact can also adversely affect flight delays and flight frequency, respectively. Thus, multimarket contact has been shown to facilitate softer price competition and lower product quality in the U.S. airline industry.

There have certainly been considerable empirical work on the effect of multimarket contact on prices in industries other than airlines. For example, Fernandez and Marin (1998) confirm the theory in Bernheim and Whinston (1990) that multimarket contact facilitates collusion using data from the Spanish hotel industry. Interestingly, they find that the omission of variables measuring multimarket contact creates a downward bias on the effect of concentration on prices. Indeed, prices are higher when there is more multimarket contact among firms in the U.S. cement industry (Jans and Rosenbaum, 1996), movie theaters (Feinberg, 2014), and hospitals (Schmitt, 2018). Finally, Pilloff (1999) finds that multimarket contact is positively related to profitability in the banking industry.

1.3 Data

1.3.1 Data Sources

We obtain data from two main sources. Our first main data set is the Airline Origin and Destination Survey (DB1B) database, which is a 10% random sample of all domestic air travel and provides information on prices, origin, destination, the number of passengers per ticket, the number of coupons for an itinerary, distance, and a round-trip indicator. Following Gerardi and Shapiro (2009), we focus on domestic, coach-class, and nonstop airline tickets, but we expand the sample time period to 1993:Q1 and 2017:Q4. The second main data set is from the T-100, which provides data on capacity (the number of flights and seats), as well as total enplaned passengers. Both of these data sets are made publicly available by the Bureau of Transportation Statistics.

We identify outsourcing in the DB1B data set when the ticketing carrier is a major airline, while the operating carrier is a regional airline. Major airlines like American Airlines or Delta Air Lines outsource the operation of certain routes to various regional airlines like Air Wisconsin, Chautauqua, Mesa, Republic Airlines, SkyWest Airlines, and Trans State Airlines. Under these agreements, major airlines are responsible for ticket sales and airport operations, whereas regional airlines operate the route with their own aircraft and flight crew.[1]

1.3.2 Variable Construction

Our variable construction closely follows Gerardi and Shapiro (2009) and Ciliberto and Williams (2014). To calculate average fares for ticketing airline i on route j in year-quarter t ($Fare_{ijt}$), we first treat round-trip tickets as two one-way tickets by dividing the fare by two and deflating fares using the consumer price index to 2017 dollars.[2] As with Ciliberto and Williams (2014), routes are defined as a uni-directional airport-pair.[3] We also drop exceedingly low and high fares (less than \$25 and greater than \$2,500). We also calculate the 10th percentile airfare ($Fare10_{ijt}$) and the 90th percentile airfare ($Fare90_{ijt}$). The Gini coefficient ($Gini_{ijt}$) measures price dispersion and is defined as twice the expected absolute difference between two ticket prices drawn randomly from the population. As such, a Gini coefficient equal to 0 implies that every

[1]Forbes and Lederman (2009) and Tan (2018) provide detailed information on the relationship between major carriers and regional airlines.

[2]We use data on the Consumer Price Index from the Bureau of Labor Statistics in order to deflate prices.

[3]Following Gerardi and Shapiro (2009), we avoid "double counting" round-trip tickets by dropping one of the directions consistently. For example, suppose a passenger flies Southwest Airlines nonstop between Baltimore-Washington International Airport (BWI) and Boston Logan International Airport (BOS). We drop the return leg (BOS to BWI) in order to avoid double counting the round trip ticket. Moreover, our results are qualitatively similar if we instead define a route using city-pair groupings in Brueckner, Lee, and Singer (2014) and are available upon request.

passenger pays the same price, whereas an increase in the Gini coefficient suggests an increase in price dispersion. The log-odds ratio of the Gini coefficient ($Gini\_lodd_{ijt}$) is defined as $ln\left[\frac{Gini}{(1-Gini)}\right]$.

Our key variable of interest is average multimarket contact. We follow Evans and Kessides (1994) and Ciliberto and Williams (2014) to construct multimarket contact for a pair of airlines A and B on a route (MMC_{AB}^{t}) and average multimarket contact on a route (Avg_MMC_{jt}). First let MMC_{AB}^{t} denote the number of routes that two distinct carriers, A and B, simultaneously serve at time t. For example, American and Delta directly competed on 855 routes in the first quarter of 2017 so both MMC_{AADL}^{2017Q1} $= MMC_{DLAA}^{2017Q1} = 855$. For each quarter, we construct a matrix of these pair-specific variables. We then use the MMC^{t} matrix to calculate the route-specific average of multimarket contact for each year-quarter:

$$Avg\_MMC_{jt} = \frac{1}{F_{jt}(F_{jt}-1)} \sum_{A=1}^{F} \sum_{B=1, A \neq B}^{F} I[A \text{ } and \text{ } B \text{ } active]_{jt} * MMC_{AB}^{t},$$

where the indicator function, $I[A \text{ } and \text{ } B \text{ } active]_{jt}$ is equal to 1 if carriers A and B are both on route j at time t, F_{jt} is the number of incumbent firms on route j at time t, and F is the total number of airlines so that $F_{jt}(F_{jt}-1)$ drops diagonal elements in the matrix since those indicate multimarket contacts of themselves. Thus, Avg_MMC_{jt} is equal to the average of MMC_{AB}^{t} across the firms actively serving route j at time t. As such, variation in average multimarket contact across markets comes from differences in the set of firms operating in the market because the multimarket contact of two carriers, MMC_{AB}^{t}, is fixed for a specific time period. In other words, the numerical value for Avg_MMC_{jt} varies based on changes in the set of airlines operating in the market, as well as potential changes in the degree of overlap between a given pair of carriers.

We use outsourcing agreements between major carriers and independent regional airlines to construct two instruments for Avg_MMC_{jt}. We first separate routes into 10 markets based on deciles of route-level passenger traffic (i.e. 0-10, 10-20, 20-30,…, 90-100 percentiles of route-level enplanements). Next, we calculate two average outsourcing ratios: 1) an outsourcing ticket ratio for a particular airline ($own\_outsourcing_{ijt}$) and 2) an outsourcing ticket ratio for competing firms ($competitor\_outsourcing_{ijt}$) for the relevant market size of each route. To be sure, route j is not included in the construction of the two outsourcing variables in order to avoid the direct correlation between an airline's outsourcing decision made for a given market and our dependent variables (airfares and the Gini coefficient) in that market. For example, suppose American Airlines flies three airport-pair routes (A-B, C-D, and E-F) in one of the market groups. We define $own\_outsourcing_{ijt}$ for American Airlines on the A-B route by taking the average of $own\_outsourcing_{ijt}$ for C-D and E-F routes. Similarly, the average of $own\_outsourcing_{ijt}$ for A-B and E-F routes are used to calculate the value of $own\_outsourcing_{ijt}$ for American Airlines servicing the C-D route.

Regional airlines can be either a wholly owned subsidiary of a major airline or independent from major airlines. Following Tan (2018), we focus our attention on the partnerships between major airlines and independent regional airlines since including wholly owned regionals in our analysis can lead to endogeneity issues if a demand shock can lead to a major both changing its pricing and reallocating flights serviced by its wholly owned regional airlines.[4] Major airlines are responsible for the pricing of

[4]Since each major airline owns multiple regional airlines, we do not include these wholly owned subsidiaries in our outsourcing variables. For example, Envoy Air (formerly, American Eagle) and Executive Airlines have been American Airlines's wholly owned subsidiaries. PSA and Piedmont Airlines were wholly owned subsidiaries of US Airways before the American - US Airways merger in 2015, and subsequently became a wholly owned subsidiary of American. ExpressJet (formerly, Atlantic Southeast Airlines) was a wholly owned subsidiary of Delta Air Lines from 1999 to 2005 before being purchased by SkyWest, while Comair was a wholly owned subsidiary of Delta before Delta shut it down in 2012. Mesaba and Compass Airlines were wholly owned subsidiaries of Northwest Airlines and then became wholly owned subsidiaries of Delta following the Delta - Northwest merger in 2010 before being sold to Pinnacle and Trans States Airlines, respectively. Endeavor Air (formerly,

flights operated by regional airlines, especially in the case of wholly own subsidiaries, so major airlines can change their prices for flights operated by their wholly owned regionals promptly in response to market specific shocks. Thus, we do not include these wholly owned subsidiaries in defining our outsourcing variables and instead only use independent regional airline partners.

Although we do not have access to actual outsourcing contracts between major airlines and their independent regional airline partners, a variety of sources, including the annual report on Form 10-K offered by U.S. Securities and Exchange Commission (SEC), the airlines' official websites, and news articles, show that these contacts are long-term agreements usually with initial terms of at least 10 years and grants major airlines with the option to extend the initial term (Form 10-K, Delta, 2010-12-31).[5] According to the DB1B data, outsourcing contracts last for 36.6 quarters on average during our sample period with the following breakdown by airline: American (36.6 quarters with 9 regional airlines), Alaska (63 quarters with 2 regionals), Continental (30 quarters with 30.2 quarters with 9 regionals), Delta (38.9 quarters with 13 regionals), Northwest (28.6 quarters with 9 regionals), United (44.5 quarters with 15 regionals), US Airways (39.7 with 10 regionals). Given the long-term contracts between major airlines and independent regionals, frequent flight reallocation to endogenize market specific shocks might be hard so our instruments are less likely to be correlated with the error term.

We include several additional control variables in our regressions. Carriers serving a larger number of destinations out of an origin airport can offer more attractive frequent flyer programs and experience stronger demand so $Networksize_{ijt}$ is the per-

Pinnacle Airlines) emerged from Chapter 11 reorganization as a wholly owned subsidiary of Delta in 2013. Finally, Continental Micronesia was a wholly owned subsidiary of Continental Airlines prior to the Continental-United merger.

[5]See Gil, Kim, and Zanarone (2019) and Kim and Kim (2019) for more information on the long-term agreements between major airlines and independent regional airlines.

centage of all routes serviced out of an airport by an airline. We construct the variable $Roundtrip_{ijt}$ to be the proportion of round-trip tickets sold by an airline for a particular route in order to control for potential discounting of round-trip vs. one-way travel. Hub_{ijt} is a dummy variable that indicates whether at least one of the endpoint airports on a route serves as a hub airport for that airline. Finally, HHI_{jt} is the route-level Herfindahl-Hirschman Index, which is the sum of squared market shares.

Table 1.1: Summary Statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
$Fare$	267.375	107.137	41.390	1207.841	242,088
$Fare10$	129.676	47.344	25.740	807.559	242,088
$Fare90$	467.263	236.268	44.558	2050.327	242,088
$Gini$	0.270	0.061	0.000	0.609	242,088
$Gini\_lodd$	-1.017	0.315	-6.142	0.444	241,637
Avg_MMC	232.361	173.331	1.000	1058.000	242,088
$own\_outsourcing$	0.148	0.203	0.000	1.000	242,088
$competitor\_outsourcing$	0.081	0.142	0.000	1.000	242,088
$Networksize$	0.472	0.350	0.008	1.000	242,088
$Roundtrip$	0.727	0.178	0.000	1.000	242,088
Hub	0.649	0.477	0.000	1.000	242,088
HHI	0.620	0.218	0.143	1.000	242,088

Summary statistics are reported in Table 1.1. Our final data set contains 242,088 observations for 26 airlines, 4,409 routes, and 100 year-quarter time periods. Detailed directions of our data construction are outlined in the appendix.

1.4 Empirical Analysis

1.4.1 Estimation Strategy

Our empirical analysis combines the estimation strategy in Gerardi and Shapiro (2009) and Ciliberto and Williams (2014). We investigate the effect of multimarket contact along different points of the price distribution in order to provide insight on the resulting change in price dispersion. The main econometric specification is

$$y_{ijt} = \alpha + \beta Avg\_MMC/SD_{jt} + \gamma X_{ijt} + \rho_{ij} + \delta_{it} + \epsilon_{ijt}, \tag{1.1}$$

where y_{ijt} is either the Gini coefficient ($Gini_{ijt}$ or $Gini\_lodd_{ijt}$) or logged airfare ($lnFare_{ijt}$, $lnFare10_{ijt}$, or $lnFare90_{ijt}$) for airline i on route j at time t. Following Ciliberto and Williams (2014), we proxy for tacit collusion using Avg_MMC_{jt}; however, instead of dividing this variable by 1,000 as in their paper, we instead scale our variable by dividing Avg_MMC_{jt} by 173, which is the sample standard deviation as reported in Table 1.1. As such, the key variable of interest in Equation (1.1) is Avg_MMC/SD_{jt}. X_{ijt} includes additional control variables: $Networksize_{ijt}$, $Roundtrip_{ijt}$, Hub_{ijt}, and HHI_{jt}.[6] We include carrier-route fixed effects, ρ_{ij}, and carrier-time fixed effects, δ_{it}. We cluster standard errors by route to account for serial correlation and correlation between pricing decisions of carriers on the same route.

Following Gerardi and Shapiro (2009), we include three instruments for HHI in all of our regressions. The first instrument is $lnRoutePass_{jt}$, which is logged route-level passenger traffic in a given time period. The second instrument is $iRouteHHI_{ijt}$, which is a function of the route-level HHI, as well as observed and fitted values of an airline's route-level market shares.[7] Finally, our third instrument is $PassRatio_{ijt}$, which is a ratio based on an airline's airport-level passenger traffic and the overall passenger traffic at that airport.[8]

The effect of Avg_MMC_{jt} on airfares is uncertain. On the one hand, an increase in

[6]As a robustness check, we also include weather-related variables collected from the National Oceanic and Atmospheric Administration (NOAA), such as precipitation, snowfall, and temperature, as well as capacity-related variables obtained from the Bureau of Transportation Statistics, including peaktime and loadfactor. Our results are qualitatively similar and available upon request.

[7]Following both Borenstein and Rose (1994) and Gerardi and Shapiro (2009), $iRouteHHI$ is calculated as $\hat{S}^2_{ijt} + \frac{HHI_{jt} - S^2_{ijt}}{(1-\hat{S}_{ijt})^2} * (1 - \hat{S}_{ijt})^2$, where $\hat{S}^2_{ijt}$ is the fitted value for market share for carrier i on route j at time t.

[8]As in Gerardi and Shapiro (2009), $PassRatio_{ijt} = \frac{\sqrt{Pass_{i1} * Pass_{i2}}}{\sum_k \sqrt{Pass_{k1} * Pass_{k2}}}$, where i is the observed airline, k indexes all airlines, and $Pass_{k1}$ and $Pass_{k2}$ are quarterly airport-level passenger traffic at the two endpoint airports.

the number of rival firms in a market should strengthen competition and lead to lower airfares. If this holds, then we anticipate a negative value for β in our price regressions. On the other hand, the mutual forbearance hypothesis suggests that average multimarket contact should facilitate tacit collusion so that weaker price competition results in higher airfares. If this is true, then the sign for β should be positive.

Ciliberto and Williams (2014) suggest that average multimarket contact is endogenous since time-varying and market-specific unobservables may affect price, entry, and exit decisions by airlines. Therefore, we construct two instrumental variables: outsourcing ticket ratio of an airline ($own\_outsourcing_{ijt}$) and outsourcing ticket ratio of competing airlines ($competitor\_outsourcing_{ijt}$). In order to be a valid instrument, these outsourcing ticket ratios must be correlated with average multimarket contact and uncorrelated with the error term in Equation (1.1). As such, a major airline's outsourcing decision can affect multimarket contact. For example, suppose a route is serviced by two airlines, Delta (DL) and United (US), that also simultaneously compete head-to-head on 500 routes ($MMC_{DLUS} = MMC_{USDL} = 500$) in a given time period. In this case, the average multimarket contact on this route, $Avg\_MMC = \frac{2\times500}{2\times1} = 500$. Then suppose that American (AA) enters this route under an outsourcing contract with Sky-West (OO), an independent regional airline, such that $MMC_{AADL} = MMC_{DLAA} = 1000$ and $MMC_{AAUS} = MMC_{USAA} = 600$, respectively. Thus, AA's entry increases average multimarket contact since $Avg\_MMC = \frac{2\times500+2\times1000+2\times600}{3\times2} = 700$. Therefore, outsourcing can lead to changes in the average multimarket contact.

We expect a positive correlation between average multimarket contact and outsourcing as in the numerical example for two reasons. First, there is a higher level of pair-wise multimarket contact between airlines due to an increase in the set of operating firms when major airlines expand their outsourcing arrangements with independent regional airlines. Second, when a major carrier enters a new route but outsources the flight operations to an independent regional airline partner, the increase in the degree of overlap between a given pair of airlines results in an increase in pair-wise multimarket contact.[9]

One may argue that outsourcing decisions may not be valid if it is correlated with a carrier's entry/exit decisions to endogenize market specific-shocks. However, major airlines and independent regional airlines are typically engaged in long-term business relationships as discussed in Section 1.3.2. More importantly, since major airlines are motivated to outsource in order to exploit the independent regional airlines' benefit with respect to cost, economies of scale, and efficient resource allocations from a long-term point of view, unexpected market-specific shocks to demand or cost would not affect a major airline's decision on long-term outsourcing contracts.

Also, it is unlikely that major airlines want to terminate outsourcing contract in response to market-specific shocks to demand because a single route typically represents a relatively small portion of a major airline's revenues out of an airport. For example, SkyWest operated a variety of different routes for Delta in 2017 which connect around 46 different airports.[10] Therefore, shocks in one route may be offset by shocks in another route, which can neutralize a change in demand out of an airport and consequently leave outsourcing decisions unchanged. At the same time, independent regional airlines and major airlines have developed a symbiotic relationship as discussed in Tan (2018) since independent regional airlines depend on their contracts with major carriers for passengers, whereas major carriers rely on independent regional airlines as an important feeder of passengers within their route network. Thus, it appears that major airlines

[9] It is possible for an independent regional airline to contract with multiple major airlines on a given route. As a robustness check, we drop these cases and the results are qualitatively similar. See Kim and Kim (2019) for more details on this special type of partnership and its direct effect on tacit collusion.

[10] SkyWest primarily operates Delta-ticketed flights out of Delta's Atlanta (ATL), Detroit (DTW), Minneapolis/St. Paul (MSP), and Salt Lake City (SLC) hubs. SkyWest's route map can be found at `https://www.skywest.com/fly-skywest-airlines/skywest-airlines-route-map/`.

will not unilaterally terminate outsourcing contracts with independent regional airlines unless they are bankrupt.

According to Forbes and Lederman (2009), many contracts between major and regional airlines allocate the rights to decide on schedule adjustments to the major airlines. However, having the rights to order specific schedule changes is not equivalent to having the rights to actually implement those schedule changes. Schedule changes ordered by the major airlines must still be carried out by the regional airlines. The same logic seems reasonable to apply towards the rights to changing prices since airlines consider many aspects in their pricing decisions. Even though major airlines have a right to adjust prices in real time, they may not want to do so without the cooperation of regional airlines given the delays and high adaptation costs in the adjustment process between major and regional airlines.

1.4.2 Multimarket Contact and Price Dispersion

We start with the fixed effects (FE) regression results for Equation (1.1). Table 1.2 contains estimation results for all five dependent variables: logged average fare (Column 1), logged 10th percentile airfares (Column 2), logged 90th percentile airfares (Column 3), the Gini coefficient (Column 4), and the log-odds ratio of the Gini coefficient (Column 5). To be sure, there are less observations in the regression results for Column 5 since there were 447 observations in which $Gini = 0$ and therefore the value for $Gini\_lodd$ for these observations does not exist.

As in Evans and Kessides (1994) and Ciliberto and Williams (2014), we find a positive and significant coefficient for average multimarket contact (Avg_MMC) in Column 1, suggesting that an increase in average multimarket contact increases average fares. Thus, we provide corroborating evidence that tacit collusion leads to weaker price competition in the U.S. airline industry. More importantly, Columns 4 and 5 show that

Table 1.2: Multimarket Contact and Price Dispersion (FE)

VARIABLES	(1) lnFare	(2) lnFare10	(3) lnFare90	(4) Gini	(5) Gini_lodd
Avg_MMC/SD	0.017***	0.026***	0.013***	-0.003***	-0.014***
	(0.001)	(0.002)	(0.002)	(0.000)	(0.002)
$Networksize$	0.018	-0.022*	0.004	0.003	0.016
	(0.013)	(0.013)	(0.017)	(0.003)	(0.013)
$Roundtrip$	0.003	0.009	0.110***	0.022***	0.102***
	(0.013)	(0.012)	(0.019)	(0.003)	(0.016)
Hub	0.059***	0.015	0.102***	0.016***	0.086***
	(0.012)	(0.012)	(0.016)	(0.003)	(0.014)
$\widehat{HHI}$	0.338***	0.327***	0.334***	-0.009***	-0.045***
	(0.016)	(0.016)	(0.019)	(0.003)	(0.014)
Observations	240,527	240,527	240,527	240,527	240,080

Notes: (i) HHI is instrumented by $lnRoutePass$, $iRouteHHI$, and $PassRatio$. (ii) Carrier-route and carrier-time fixed effects are included in all regressions. (iii) Route-specific clustered standard errors in parentheses. (iv) *** p¡0.01, ** p¡0.05, * p¡0.1.

average multimarket contact has a negative and significant impact on price dispersion. Since 10th percentile airfares (Column 2) increase by more than 90th percentile airfares (Column 3), the price distribution becomes more compressed.

The results in Table 1.2 might be biased given endogeneity concerns so we report the regression results of the two-stage least squares fixed effects (FE 2SLS) estimations in Table 1.3 using both outsourcing ticket ratio of an airline ($own\_outsourcing$) and outsourcing ticket ratio of competing airlines ($competitor\_outsourcing$) as instrumental variables for average multimarket contact. As in Table 1.2, the coefficients for average multimarket contact in the price regressions (Columns 1-3) remain positive and statistically significant, whereas the coefficients for Avg_MMC in the price dispersion regressions (Columns 4 and 5) are still negative and statistically significant. Thus, the regression results in Table 1.3 provide the first main results of the paper, which is that tacit collusion has a negative effect on price dispersion due to higher 10th percentile airfares compared to mean airfares and 90th percentile airfares.

Table 1.3: Multimarket Contact and Price Dispersion (FE 2SLS - Second Stage)

VARIABLES	(1) lnFare	(2) lnFare10	(3) lnFare90	(4) Gini	(5) Gini_lodd
Avg_MMC/SD	0.280***	0.351***	0.272***	-0.024***	-0.132***
	(0.027)	(0.030)	(0.032)	(0.004)	(0.023)
$Networksize$	0.020	-0.019	0.006	0.002	0.015
	(0.017)	(0.018)	(0.021)	(0.003)	(0.014)
$Roundtrip$	-0.046***	-0.052***	0.062***	0.026***	0.124***
	(0.016)	(0.016)	(0.022)	(0.003)	(0.017)
Hub	0.032**	-0.019	0.075***	0.019***	0.098***
	(0.016)	(0.018)	(0.019)	(0.003)	(0.015)
$\widehat{HHI}$	0.091***	0.022	0.091**	0.012**	0.066***
	(0.030)	(0.033)	(0.035)	(0.005)	(0.025)
Observations	240,527	240,527	240,527	240,527	240,080

Notes: (i) Avg_MMC/SD is instrumented by *own_outsourcing* and *competitor_outsourcing*. (ii) HHI is instrumented by *lnRoutePass*, *iRouteHHI*, and *PassRatio*. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

Table 1.4: Multimarket Contact and Price Dispersion (FE 2SLS - First Stage)

VARIABLES	(1) Avg_MMC/SD	(2) Avg_MMC/SD	(3) Avg_MMC/SD	(4) Avg_MMC/SD	(5) Avg_MMC/SD	(6) Gini
own_outsourcing	0.460***	0.460***	0.460***	0.460***	0.459***	
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	
competitor_outsourcing	0.383***	0.383***	0.383***	0.383***	0.382***	
	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	
$Networksize$	0.007	0.007	0.007	0.007	0.007	0.002
	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)	(0.003)
$Roundtrip$	0.182***	0.182***	0.182***	0.182***	0.186***	0.026***
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.003)
Hub	0.113**	0.113**	0.113**	0.113**	0.109**	0.019***
	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.003)
$\widehat{HHI}$	0.952***	0.952***	0.952***	0.952***	0.951***	0.012**
	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)	(0.005)
Avg_MMC/SD						-0.024***
						(0.004)
Residual						0.022***
						(0.004)
F-stat	97.257	97.257	97.257	97.257	97.136	36.986
Observations	240,527	240,527	240,527	240,527	240,080	240,527

Notes: (i) HHI is instrumented by *lnRoutePass*, *iRouteHHI*, and *PassRatio*. (ii) Carrier-route and carrier-time fixed effects are included in all regressions. (iii) Route-specific clustered standard errors in parentheses. (iv) *** p¡0.01, ** p¡0.05, * p¡0.1.

Columns 1-5 in Table 1.4 shows the first stage regression results for Columns 1-5 in Table 1.3, respectively. The outsourcing ticket ratio of an airline (*own_outsourcing*) has a positive effect on average multimarket contact, meaning that major carriers that outsource a higher proportion of their own tickets to regional airlines tend to compete head-to-head more frequently against rival carriers. Similarly, the estimated coefficient for the outsourcing ticket ratio of competing airlines (*competitor_outsourcing*) is also positive and significant. Moreover, F-statistics are all greater than 10, implying that our instruments satisfy the relevance assumption for 2SLS. Lastly, Column 6 shows an Hausman test result, confirming that average multimarket contact is indeed endogenous due to correlation with the error term.

As a robustness check, we consider two alternative measures for tacit collusion introduced in Ciliberto and Williams (2014): 1) $Avg\_pct\_MMC_{jt}$ and 2) $Avg\_pct\_weighted\_MMC_{jt}$. Using the notation for Avg_MMC_{jt} in Section 1.3.2, we construct $pct\_MMC^t_{AB}$ to be equal to MMC^t_{AB} divided by the total number of markets served by airline A such that Avg_MMC_{jt} factors for the potential risk of smaller airlines having more to lose than larger airlines by deviating from the collusive agreement.[11] Feinberg (1985) weights multimarket contact by the sales at stake in the markets in which multimarket contact occur so we define $Avg\_pct\_weighted\_MMC^t_{AB}$ as the weighted average of $pct\_MMC^t_{AB}$ based on airline B's market share, which allows for airlines with the same number of markets, but varying passenger volumes to benefit differently from the collusive agreement.[12]

In order to be concise, we truncate the regression results in Table 1.5 by only presenting the estimated coefficients of the three collusion variables and their standard error for each of the five dependent variables. As such, the regression results for Avg_MMC/SD are identical to the results presented in Table 1.3. As in Ciliberto and Williams (2014), our regression results are qualitatively similar for all three measures

[11] $Avg\_pct\_MMC_{jt} = \frac{1}{F_{jt}(F_{jt}-1)} \sum_{A=1}^{F} \sum_{B=1, A\neq B}^{F} I[A \text{ and } B \text{ active}]_{jt} * pct\_MMC^t_{AB}$.

[12] $Avg\_weighted\_pct\_MMC_{jt} = \frac{1}{F_{jt}(F_{jt}-1)} \sum_{A=1}^{F} \sum_{B=1, A\neq B}^{F} I[A \text{ and } B \text{ active}]_{jt} * pct\_MMC^t_{AB} * Mktshare^t_B$.

Table 1.5: Multimarket Contact and Price Dispersion (Alternative Measures)

VARIABLES	(1) lnFare	(2) lnFare10	(3) lnFare90	(4) Gini	(5) Gini_lodd
Avg_MMC/SD	0.280***	0.351***	0.272***	-0.024***	-0.132***
	(0.027)	(0.030)	(0.032)	(0.004)	(0.023)
Avg_pct_MMC	8.088***	9.541***	7.522***	-0.701***	-3.749***
	(1.579)	(1.833)	(1.591)	(0.167)	(0.880)
Avg_pct_weighted_MMC	2.997***	2.178***	2.008*	-0.268*	-1.172*
	(0.916)	(0.767)	(1.043)	(0.141)	(0.700)
Observations	240,527	240,527	240,527	240,527	240,080

Notes: (i) Each of the three multimarket contact measures is instrumented by *own_outsourcing* and *competitor_outsourcing*. (ii) *HHI* is instrumented by *lnRoutePass*, *iRouteHHI*, and *PassRatio*. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

for tacit collusion.

1.4.3 Multimarket Contact and Southwest Airlines

Previous papers have studied the so-called Southwest Effect, in which the presence of Southwest Airlines puts downward pressure on airfares. Vowles (2001) documents that rival airlines decrease their airfares when they compete head-to-head with Southwest Airlines, as well as when Southwest services a nearby airport. Morrison (2001) not only documents Southwest's pro-competitive effect on direct and adjacent competition as in Vowles (2001), but also shows that incumbent airlines lower price due to potential competition, which occurs when Southwest services both endpoint airports, but not the direct route itself. Indeed, Goolsbee and Syverson (2008) finds strong evidence of this pro-competitive effect from potential competition with Southwest Airlines and documents that this pricing phenomenon does not exist on routes where Southwest does not service either endpoint. More recently, Brueckner, Lee, and Singer (2013) show that competition against legacy carriers yields weak effects on average airfares, while the presence of low-cost carriers, particularly Southwest, exhibits strong downward pressure on prices. Finally, Tan (2015) find that entry by Southwest Airlines leads to lower price dispersion since incumbent airlines lower their 90th percentile airfares by more than their 10th percentile airfares.

We analyze the effect of Southwest Airlines on the pricing phenomenon addressed in Section 1.4.2 by splitting our data set into two subsamples: one with only routes served by Southwest and another with routes not served by Southwest. We then run separate regressions based on Equation (1.1) for each subsample while dropping observations for Southwest. Table 1.6 breaks down the summary statistics between the two subsamples. Based on the existing literature, it is unsurprising that the mean airfare ($Fare$), 10th percentile airfare ($Fare10$), and 90th percentile airfare ($Fare90$) are all lower, on average, for routes serviced by Southwest than for routes not serviced by Southwest. In addition, price dispersion ($Gini$) is also lower for Southwest markets, which means that the price distribution is more compressed, on average, given the presence of Southwest Airlines. However, there is a higher incidence of multimarket contact (Avg_MMC), our key variable of interest, on routes serviced by Southwest Airlines. In Section 1.4.4, we implement a PSM estimation strategy to account for the possible heterogeneity of market types.

Table 1.6: Summary Statistics for Non-Southwest vs. Southwest Markets

Variable	Non-Southwest Markets Mean	Non-Southwest Markets Std. Dev.	Southwest Markets Mean	Southwest Markets Std. Dev.
$Fare$	287.558	109.385	216.715	85.644
$Fare10$	135.208	47.992	113.341	44.719
$Fare90$	511.537	246.244	356.456	166.605
$Gini$	0.279	0.061	0.255	0.049
$Gini\_lodd$	-0.966	0.309	-1.085	0.261
Avg_MMC	1.256	1.010	1.427	0.887
$own\_outsourcing$	0.155	0.206	0.186	0.203
$competitor\_outsourcing$	0.079	0.142	0.056	0.108
$Networksize$	0.483	0.364	0.412	0.327
$Roundtrip$	0.758	0.166	0.672	0.184
Hub	0.685	0.465	0.600	0.490
HHI	0.635	0.217	0.570	0.218
$Airport\_cost1$	0.363	0.177	0.166	0.116
$Airport\_cost2$	0.363	0.183	0.163	0.118
Observations	177,696		45,002	

Figure 1.1: Multimarket Contact and Price Dispersion for Non-Southwest vs. Southwest Markets

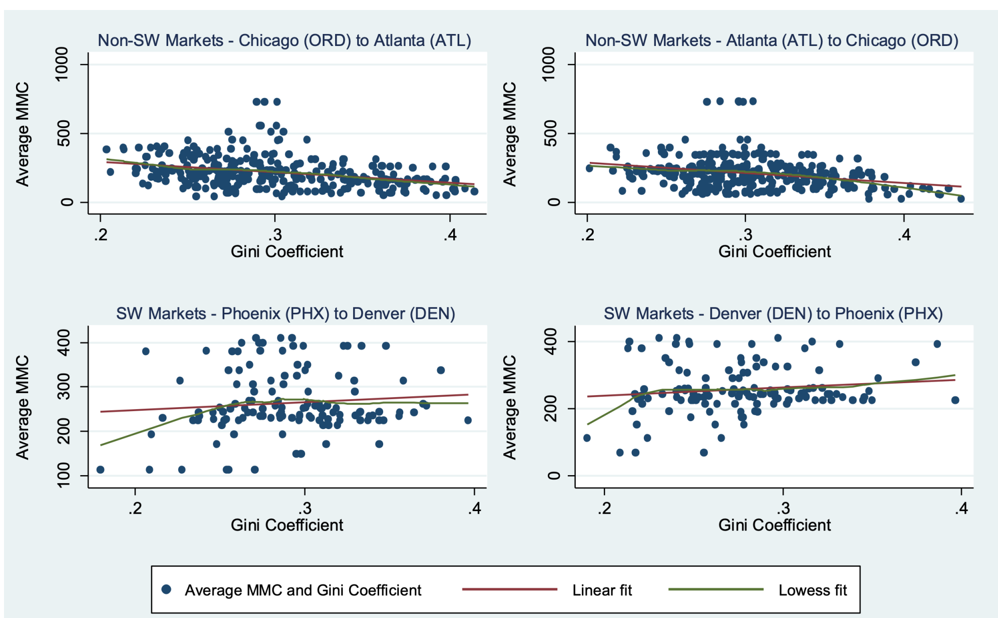


Figure 1.1 illustrates the relationship between Avg_MMC and $Gini$ for four routes. The regression results discussed in Section 1.4.2 pertain to the two markets that are not serviced by Southwest Airlines (top row), whereas the negative correlation between

multimarket contact and price dispersion no longer holds for the two markets where Southwest is present (bottom row).[13] This novel result motivates the regression analysis presented in the rest of this section.

Table 1.7: Multimarket Contact and Southwest Airlines (FE 2SLS)

	Non-Southwest Markets					Southwest Markets				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	lnFare	lnFare10	lnFare90	Gini	Gini_lodd	lnFare	lnFare10	lnFare90	Gini	Gini_lodd
Avg_MMC/SD	0.192***	0.253***	0.184***	-0.019***	-0.101***	0.044	0.174**	0.040	-0.025	-0.146
	(0.022)	(0.022)	(0.027)	(0.004)	(0.021)	(0.055)	(0.071)	(0.073)	(0.017)	(0.090)
$Networksize$	0.009	-0.026	-0.010	0.001	0.010	0.032	-0.021	0.051*	0.009	0.045
	(0.017)	(0.017)	(0.022)	(0.003)	(0.015)	(0.023)	(0.028)	(0.031)	(0.007)	(0.036)
$Roundtrip$	-0.018	-0.051***	0.139***	0.041***	0.190***	-0.028	0.028	-0.044	-0.017***	-0.081**
	(0.017)	(0.017)	(0.023)	(0.004)	(0.019)	(0.024)	(0.026)	(0.035)	(0.006)	(0.034)
Hub	0.029	0.001	0.078**	0.018***	0.086***	0.010	-0.033**	0.047***	0.015***	0.085***
	(0.025)	(0.031)	(0.033)	(0.006)	(0.026)	(0.011)	(0.016)	(0.013)	(0.004)	(0.021)
$\widehat{HHI}$	0.150***	0.032	0.166***	0.021***	0.110***	0.245***	0.145**	0.254***	0.016	0.094
	(0.031)	(0.032)	(0.038)	(0.006)	(0.029)	(0.047)	(0.063)	(0.061)	(0.014)	(0.074)
$lnAirport\_cost1$	0.083***	0.054***	0.099***	0.005***	0.027***	0.004	0.007**	0.004	-0.001	-0.004
	(0.005)	(0.005)	(0.006)	(0.001)	(0.004)	(0.003)	(0.003)	(0.003)	(0.001)	(0.004)
$lnAirport\_cost2$	0.080***	0.053***	0.095***	0.005***	0.024***	0.004	0.008**	0.004	-0.000	-0.004
	(0.005)	(0.005)	(0.006)	(0.001)	(0.004)	(0.003)	(0.004)	(0.004)	(0.001)	(0.004)
Observations	176,282	176,282	176,282	176,282	175,861	44,383	44,383	44,383	44,383	44,357

Note: (i) Avg_MMC/SD is instrumented by $own\_outsourcing$ and $competitor\_outsourcing$. (ii) HHI is instrumented by $lnRoutePass$, $iRouteHHI$, and $PassRatio$. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

Table 1.7 presents separate regression results based on Equation (1.1) for the non-Southwest markets subsample and the Southwest markets subsample. In addition to the control variables listed in Section 1.4.1, we include two additional covariates related to airport costs in order to avoid a possible endogeneity issue associated with Southwest's entry decision and demand or supply shocks in the market. For example, Southwest may choose to enter routes with falling operating costs, and thus incumbents may cut their prices in response to lower operating costs rather than Southwest's entry. Following Goolsbee and Syverson (2008) and Ma (2019), the airport operating cost measure for an origin airport ($lnAirport\_cost1_{ijt}$) is defined as carrier i's average logged

[13] Other popular Southwest routes listed on its official website (`https://www.southwest.com/html/air/routes/index.html?clk=GFOOTER-FLY-ROUTES`), such as Atlanta (ATL) to Chicago (MDW), Las Vegas (LAS) to Denver (DEN), and Oakland (OAK) to Las Vegas (LAS), are associated with a similar non-negative correlation between multimarket contact and price dispersion. These figures are available upon request.

airfare divided by distance for routes between the origin airport of route j and airports not serviced by Southwest. Similarly, $lnAirport\_cost2_{ijt}$ is the airport operating cost measure for a destination airport and is calculated as carrier i's average logged airfare divided by distance for routes between the destination airport of route j and airports not serviced by Southwest.

The estimated sign for Avg_MMC for routes without a Southwest presence is positive and statistically significant in the three fare regressions (Columns 1-3), whereas the results for Avg_MMC for Southwest markets are generally statistically insignificant (Columns 6-8). This provides evidence for tacit collusion on routes where Southwest does not exist; however, the presence of Southwest Airlines precludes this type of collusive behavior. Similar to our key result in Section 1.4.2, Columns 4 and 5 suggest that price dispersion decreases in non-Southwest markets; however, price dispersion does not change significantly for markets that include Southwest (Columns 9 and 10). In other words, the price distribution shifts to the right and becomes more compressed for routes that Southwest does not service. On the other hand, the price distribution for Southwest markets weakly shifts to the right with no alteration to its standard deviation. Thus, Table 1.7 presents the second main result of the paper, which is that multimarket contact softens route-level price competition except when Southwest is present on that route.

As a robustness check, we substitute Avg_MMC/SD with two alternative measures for multimarket contact (Avg_pct_MMC and $Avg\_pct\_weighted\_MMC$) as with Table 1.5. The results in Table 1.8 are qualitatively similar to the results presented in Table 1.7. By construction, the regression results for Avg_MMC/SD are identical in both Tables 1.7 and 1.8. More importantly, the estimated coefficients for Avg_pct_MMC and $Avg\_pct\_weighted\_MMC$ are generally statistically insignificant for Southwest markets, further suggesting that the presence of Southwest exhibits anti-collusive behavior

Table 1.8: Multimarket Contact and Southwest Airlines (Alternative Measures)

	Non-Southwest Markets					Southwest Markets				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	lnFare	lnFare10	lnFare90	Gini	Gini_lodd	lnFare	lnFare10	lnFare90	Gini	Gini_lodd
Avg_MMC/SD	0.192***	0.253***	0.184***	-0.019***	-0.101***	0.044	0.174**	0.040	-0.025	-0.146
	(0.022)	(0.022)	(0.027)	(0.004)	(0.021)	(0.055)	(0.071)	(0.073)	(0.017)	(0.090)
Avg_pct_MMC	5.305***	6.264***	4.567***	-0.595***	-3.094***	-1.243	-0.913	-1.041	-0.062	-0.644
	(1.068)	(1.197)	(1.088)	(0.137)	(0.696)	(0.780)	(0.804)	(1.077)	(0.193)	(1.008)
$Avg\_weighted\_pct\_MMC$	3.432***	3.337***	2.447**	-0.453***	-2.277***	-3.390*	-4.103*	-2.882	0.140	0.239
	(0.884)	(0.808)	(0.964)	(0.134)	(0.666)	(1.870)	(2.168)	(2.349)	(0.403)	(2.078)
Observations	176,282	176,282	176,282	176,282	175,861	44,383	44,383	44,383	44,383	44,357

Notes: (i) Each of the three multimarket contact measures is instrumented by $own\_outsourcing$ and $competitor\_outsourcing$. (ii) HHI is instrumented by $lnRoutePass$, $iRouteHHI$, and $PassRatio$. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

in the airline industry.

1.4.4 Robustness Checks

To mitigate the potential concern that the effect of multimarket contact on the dependent variables between non-Southwest markets and Southwest markets is driven by the systemic differences between the two subsamples, we constructed a sample of non-Southwest markets using propensity score matching (PSM) following Ma (2019), in which we fit a multinomial logistic regression as a function of the set of covariates that we include in our specifications such as HHI, distance, a dummy variable indicating whether there is a low-cost carrier other than Southwest on a route, as well as other carrier-route characteristics. Following Cochran and Rubin (1973) and Austin (2011), we use a Caliper Matching Process with a caliper of width equal to 0.2 of the standard deviation of the logit of the propensity score.

Table 1.9 presents the regression results using this PSM estimation method.[14] Consistent with Table 1.7, airfares for non-Southwest markets increase all along the price distribution, while price dispersion significantly decreases due to the relatively larger increase on 10th percentile airfares compared to the 90th percentile airfares. In con-

[14]Although we report the results using PSM two times, we obtain qualitatively similar estimates when using PSM either one time or three times. These results are available upon request.

trast, estimates for Avg_MMC/SD across the five regression specifications for Southwest markets are generally insignificant such that there is no change in airfares or the Gini coefficient. These consistent results imply that our main results in Table 7 neither suffer from heterogeneous market characteristics nor result from spurious effects. Therefore, we conclude that Southwest Airlines exhibits an anti-collusive impact on price competition.

Table 1.9: Multimarket Contact and Southwest Airlines (PSM Estimation)

	Non-Southwest Markets					Southwest Markets				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	lnFare	lnFare10	lnFare90	Gini	Gini_lodd	lnFare	lnFare10	lnFare90	Gini	Gini_lodd
Avg_MMC/SD	0.184***	0.253***	0.163***	-0.021***	-0.114***	0.044	0.174**	0.040	-0.025	-0.146
	(0.029)	(0.031)	(0.035)	(0.005)	(0.027)	(0.055)	(0.071)	(0.073)	(0.017)	(0.090)
$Networksize$	-0.001	-0.023	-0.018	-0.000	0.001	0.032	-0.021	0.051*	0.009	0.045
	(0.027)	(0.025)	(0.034)	(0.004)	(0.019)	(0.023)	(0.028)	(0.031)	(0.007)	(0.036)
$Roundtrip$	-0.057***	-0.052**	0.027	0.021***	0.094***	-0.028	0.028	-0.044	-0.017***	-0.081**
	(0.021)	(0.021)	(0.029)	(0.005)	(0.024)	(0.024)	(0.026)	(0.035)	(0.006)	(0.034)
Hub	0.042	0.061	0.065	0.004	0.021	0.010	-0.033**	0.047***	0.015***	0.085***
	(0.039)	(0.040)	(0.048)	(0.007)	(0.032)	(0.011)	(0.016)	(0.013)	(0.004)	(0.021)
$\widehat{HHI}$	0.212***	0.105***	0.244***	0.020***	0.111***	0.245***	0.145**	0.254***	0.016	0.094
	(0.034)	(0.036)	(0.041)	(0.006)	(0.032)	(0.047)	(0.063)	(0.061)	(0.014)	(0.074)
$lnAirport\_cost1$	0.081***	0.056***	0.098***	0.005***	0.027***	0.004	0.007**	0.004	-0.001	-0.004
	(0.006)	(0.006)	(0.007)	(0.001)	(0.006)	(0.003)	(0.003)	(0.003)	(0.001)	(0.004)
$lnAirport\_cost2$	0.070***	0.048***	0.085***	0.004***	0.020***	0.004	0.008**	0.004	-0.000	-0.004
	(0.006)	(0.006)	(0.008)	(0.001)	(0.006)	(0.003)	(0.004)	(0.004)	(0.001)	(0.004)
Observations	87,745	87,745	87,745	87,745	87,229	44,383	44,383	44,383	44,383	44,357

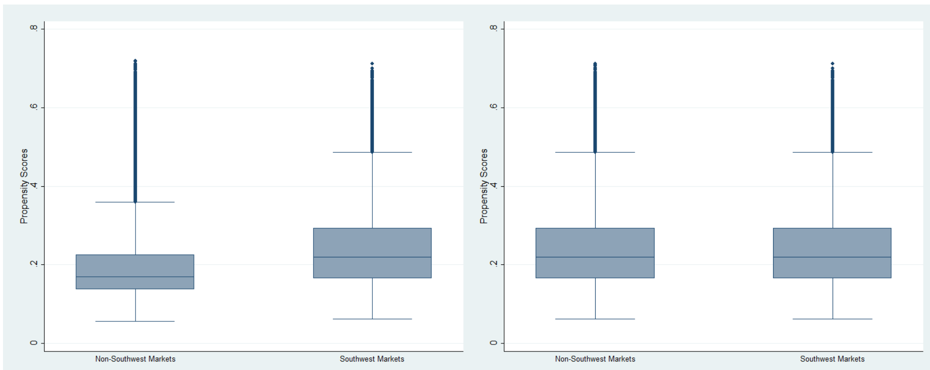
Note: (i) Avg_MMC/SD is instrumented by *own_outsourcing* and *competitor_outsourcing*. (ii) HHI is instrumented by *lnRoutePass*, *iRouteHHI*, and *PassRatio*. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

Table 1.10 and Figure 1.2 both provide robustness checks for the results in Table 1.9. Table 1.10 reports the standardized mean difference, which is the difference in means divided by the standard deviation. Following Cohen (2013), a standardized mean difference of less than 0.20 is considered "small," 0.40 is considered "moderate," and 0.60 is considered "large." With no established standards for determining substantial overlap of propensity scores, we use a combination of balancing (Table 1.10) and overlap tests (Figure 1.2) to assess whether the groups are similar enough to support causal inference. Overall, it appears reasonable to consider the covariates' distributions are balanced between the two different groups after PSM.

Table 1.10: Balance Test of Covariates Before and After PSM

	Mean in Treated (Southwest Markets)	Mean in Untreated (Non-Southwest Markets)	Unweighted Standardized Diff.	Weighted Standardized Diff.
Networksize	0.41	0.48	-0.205	0.017
Roundtrip	0.67	0.76	-0.491	0.006
Hub	0.60	0.68	-0.176	0.003
Bankruptcy	0.00	0.01	-0.051	0.002
Legacy	0.77	0.88	-0.289	0.003
Distance	1021.18	979.35	0.067	-0.004
HHI	0.57	0.63	-0.300	-0.015

Figure 1.2: Overlap Test of Propensity Scores Before and After PSM



It is natural to wonder whether our results pertain only to Southwest Airlines. Indeed, Tan (2015) showed that the Southwest Effect, in which incumbents significantly reduce airfares as a response to entry by Southwest, can loosely be applied to other low-cost carriers. Table 1.11 reports the regression results when we separate our original data set into two subsamples: markets not serviced by other low-cost carriers and markets serviced by other low-cost carriers.[15] To be sure, we exclude all observations pertaining to Southwest markets so that we can assess whether other low-cost carriers exhibit a similar anti-collusive effect as discussed in Section 1.4.3. As such, low-cost carrier markets consist of routes serviced by a low-cost carrier other than Southwest Airlines. As with our analysis for Southwest markets (Table 1.7), we drop observations pertaining to low-cost carriers in the regressions for low-cost carrier markets so that we are able to compare only the same carrier groups (i.e. major airlines) in both markets.

[15]A list of low-cost carriers can be found in the Data Appendix at the end of the paper.

Table 1.11: Multimarket Contact and Low-Cost Carriers

	Non-Low-Cost Carrier Markets					Low-Cost Carrier Markets				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	lnFare	lnFare10	lnFare90	Gini	Gini_lodd	lnFare	lnFare10	lnFare90	Gini	Gini_lodd
Avg_MMC/SD	0.224***	0.230***	0.223***	-0.016**	-0.088***	0.170***	0.280***	0.124	-0.037**	-0.178**
	(0.039)	(0.037)	(0.048)	(0.007)	(0.033)	(0.065)	(0.076)	(0.084)	(0.016)	(0.079)
$Networksize$	0.005	-0.034*	-0.021	0.000	0.005	0.012	0.034	-0.014	-0.008	-0.034
	(0.019)	(0.020)	(0.023)	(0.003)	(0.016)	(0.036)	(0.026)	(0.048)	(0.007)	(0.034)
$Roundtrip$	-0.088***	-0.141***	0.150***	0.067***	0.317***	0.028	-0.004	0.090*	0.012*	0.054
	(0.023)	(0.023)	(0.030)	(0.005)	(0.024)	(0.033)	(0.034)	(0.046)	(0.007)	(0.034)
Hub	0.024	-0.023	0.076***	0.022***	0.097***	-0.006	0.004	-0.004	-0.002	0.012
	(0.023)	(0.030)	(0.028)	(0.006)	(0.027)	(0.062)	(0.044)	(0.095)	(0.013)	(0.064)
$\widehat{HHI}$	0.091**	0.034	0.072*	0.007	0.042	0.282***	0.141***	0.374***	0.038***	0.178***
	(0.036)	(0.033)	(0.043)	(0.006)	(0.029)	(0.045)	(0.043)	(0.058)	(0.009)	(0.045)
$lnAirport\_cost1$	0.092***	0.064***	0.107***	0.005***	0.023***	0.078***	0.041***	0.103***	0.008***	0.041***
	(0.007)	(0.006)	(0.008)	(0.001)	(0.005)	(0.008)	(0.007)	(0.010)	(0.002)	(0.009)
$lnAirport\_cost2$	0.093***	0.062***	0.105***	0.005***	0.021***	0.071***	0.044***	0.089***	0.006***	0.029***
	(0.006)	(0.006)	(0.007)	(0.001)	(0.005)	(0.008)	(0.008)	(0.010)	(0.002)	(0.009)
Observations	120,686	120,686	120,686	120,686	120,646	33,919	33,919	33,919	33,919	33,908

Note: (i) Avg_MMC/SD is instrumented by $own\_outsourcing$ and $competitor\_outsourcing$. (ii) HHI is instrumented by $lnRoutePass$, $iRouteHHI$, and $PassRatio$. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

Unlike with Tables 1.7 and 1.9, the estimated coefficients for Avg_MMC/SD in the price regressions presented in Table 1.11 are positive and significant for both non-low-cost carrier markets (Columns 1-3) and low-cost carrier markets (Columns 6-8). In particular, there is evidence of tacit collusion in other low-cost carrier markets among major airlines, especially on 10th percentile airfares (Column 7), which makes sense given that low-cost carriers' markets are relatively small and more likely serve leisure travelers with lower prices. Moreover, price dispersion significantly decreases in both non-low-cost carrier markets (Columns 4 and 5) and low-cost carrier markets (Columns 9 and 10). Thus, the regression results suggest that there is something special about the presence of Southwest Airlines that precludes tacit collusion. When we turn our attention from Southwest markets to other low-cost carriers' markets, the anti-collusive behavior disappears. Thus, the regression results suggest that Southwest Airlines plays a unique role by exhibiting not only a pro-competitive effect already established in the existing literature but also an anti-collusive effect in the U.S. airline industry.

Although other low-cost carriers like JetBlue or Frontier benefit from low marginal costs like Southwest, Table 1.11 shows that other low-cost carriers do not have the same effect as Southwest on tacit collusion. According to Kang, Bayus, and Balasubramanian (2010), relative firm size can affect a firm's strategy when faced with high levels of multimarket contact. They find that dominant firms ignore rivals with relatively small market shares and tacitly collude with other dominant firms in markets with intense multimarket contact. According to our sample, Southwest's market share is similar to the market share for major airlines with an average difference of 0.09%. However, other low-cost carriers have a significantly lower market share (19.6%) than major airlines. As such, major airlines still tacitly collude with each other when low-cost carriers other than Southwest are present. Therefore, Southwest is unique compared to other low-cost carriers because of Southwest's similar relative firm size with major airlines along with their lower marginal cost.[16]

Although we believe that our instruments are valid, we lag them in order to mitigate concerns about the possible correlation of our instruments with contemporaneous demand shocks. Tables 1.12 and 1.13 provide a robustness check for Table 1.3 in Section 1.4.2 and Table 1.7 in Section 1.4.3, respectively. However, we replace our instruments for Avg_MMC_{jt} with a four quarter lag of airline i's own outsourcing ticket ratio on route j ($own\_outsourcing_{ij,t-4}$) and a four quarter lag of the outsourcing ticket ratio for airline i's competitors servicing route j ($competitor\_outsourcing_{ij,t-4}$).

As with Table 1.3, the estimated coefficients for Avg_MMC/SD in the airfare regressions (Columns 1-3) are positive and significant, whereas these coefficients are negative and significant in the price dispersion regressions (Columns 4-5). Moreover, our results in Table 1.13 are qualitatively similar to the results in Table 1.7 in Section 1.4.3. In other words, there is evidence of tacit collusion softening price competition in non-Southwest markets (Columns 1-5); however, the presence of Southwest appears to

[16]Demand and marginal cost analysis showing Southwest marginal cost is the lowest compared to major airlines are available in the online appendix.

Table 1.12: Multimarket Contact and Price Dispersion (One Year Lag IVs)

	(1)	(2)	(3)	(4)	(5)
VARIABLES	lnFare	lnFare10	lnFare90	Gini	Gini_lodd
Avg_MMC/SD	0.326***	0.485***	0.275***	-0.051***	-0.286***
	(0.072)	(0.087)	(0.081)	(0.012)	(0.065)
$Networksize$	0.030	-0.001	0.010	-0.001	-0.004
	(0.021)	(0.025)	(0.024)	(0.004)	(0.019)
$Roundtrip$	-0.056**	-0.077***	0.056**	0.031***	0.145***
	(0.023)	(0.025)	(0.029)	(0.004)	(0.023)
Hub	0.056**	0.027	0.085***	0.010**	0.056**
	(0.025)	(0.030)	(0.029)	(0.004)	(0.023)
$\widehat{HHI}$	0.065	-0.088	0.108	0.037***	0.210***
	(0.069)	(0.083)	(0.078)	(0.012)	(0.063)
Observations	203,634	203,634	203,634	203,634	203,455

Notes: (i) *Avg_MMC/SD* is instrumented by one year lags of *own_outsourcing* and *competitor_outsourcing*. (ii) *HHI* is instrumented by *lnRoutePass*, *iRouteHHI*, and *PassRatio*. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

Table 1.13: Multimarket Contact and Southwest Airlines (One Year Lag IVs)

	Non-Southwest Markets					Southwest Markets				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	lnFare	lnFare10	lnFare90	Gini	Gini_lodd	lnFare	lnFare10	lnFare90	Gini	Gini_lodd
Avg_MMC/SD	0.172***	0.315***	0.141**	-0.037***	-0.210***	0.076	0.052	0.083	0.006	0.008
	(0.051)	(0.058)	(0.063)	(0.011)	(0.054)	(0.064)	(0.073)	(0.083)	(0.018)	(0.095)
$Networksize$	0.020	-0.014	-0.004	-0.001	-0.002	0.035	-0.020	0.052	0.009	0.046
	(0.019)	(0.020)	(0.024)	(0.004)	(0.019)	(0.026)	(0.029)	(0.035)	(0.008)	(0.039)
$Roundtrip$	-0.013	-0.065***	0.147***	0.045***	0.212***	-0.025	0.043	-0.042	-0.019***	-0.101***
	(0.021)	(0.021)	(0.029)	(0.005)	(0.024)	(0.028)	(0.028)	(0.041)	(0.007)	(0.035)
Hub	0.054	0.086*	0.073	-0.002	-0.007	-0.006	-0.023	0.012	0.008*	0.043*
	(0.039)	(0.045)	(0.049)	(0.007)	(0.035)	(0.013)	(0.020)	(0.015)	(0.004)	(0.022)
$\widehat{HHI}$	0.185***	-0.039	0.231***	0.044***	0.241***	0.223***	0.250***	0.227***	-0.008	-0.022
	(0.061)	(0.071)	(0.076)	(0.013)	(0.066)	(0.054)	(0.063)	(0.069)	(0.014)	(0.078)
$lnAirport\_cost1$	0.083***	0.048***	0.099***	0.006***	0.032***	0.003	0.003	0.003	-0.000	-0.002
	(0.006)	(0.006)	(0.007)	(0.001)	(0.005)	(0.003)	(0.004)	(0.004)	(0.001)	(0.004)
$lnAirport\_cost2$	0.080***	0.048***	0.094***	0.005***	0.027***	0.006	0.004	0.007*	0.001	0.002
	(0.006)	(0.006)	(0.007)	(0.001)	(0.005)	(0.003)	(0.004)	(0.004)	(0.001)	(0.004)
Observations	149,477	149,477	149,477	149,477	149,309	38,882	38,882	38,882	38,882	38,872

Note: (i) *Avg_MMC/SD* is instrumented by one year lags of *own_outsourcing* and *competitor_outsourcing*. (ii) *HHI* is instrumented by *lnRoutePass*, *iRouteHHI*, and *PassRatio*. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

preclude this type of behavior (Columns 6-10). Thus, our two key results are robust to a one-year lag in our instruments.

1.5 Conclusion

This paper studies how tacit collusion in the U.S. airline industry affects price dispersion for airfares. Our results imply that average multimarket contact increases average fare, 10th percentile airfares, and 90th percentile airfares such that tacit collusion has a negative effect on price dispersion. Given that the mutual forbearance hypothesis implies that multimarket contact softens competition, our results suggest that airlines are tacitly colluding by raising their fares all along the price distribution, on average, when they directly compete more frequently across all routes, but the distribution becomes more compressed since prices on the left tail increase by more than prices on the right tail.

To the best of our knowledge, we are the first to document the role that Southwest Airlines has on limiting the prevalence of tacit collusion. Our results show that multimarket contact leads to softer price competition on routes where Southwest does not exist. More importantly, the presence of Southwest Airlines on a route results in an insignificant impact of multimarket contact on price dispersion and thus no empirical evidence of tacit collusion. The upshot is that Southwest Airlines remarkably inhibits the potential for collusive behavior in the airline industry.

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Appendix: Data Construction

In this appendix, we discuss our methods to construct the sample from DB1B and T-100 Domestic Segment databases. We closely follow the approaches in Gerardi and Shapiro (2009) and Ciliberto and Williams (2014). To construct our panel data of airline-route-time ticket observations, we use only domestic, coach-class and tickets containing direct fights from 1993 to 2017. Here, direct flights encompasses both nonstop flights and flights in which there is a stop but no change of plane. The BTS includes a variable, $DollarCred$, that describes the reliability of each ticket price. Dollar credit is zero if the ticket fare is of questionable magnitude, and one if it is credible. We drop all tickets for which $DollarCred$ is equal to zero.

We drop all fares less than \$25 for one-way tickets and \$50 for round-trip tickets. Also, we drop exceedingly high fares greater than \$2500 for one-way tickets, which are likely key punch errors. Fares are then deflated using the consumer price index to 2017 dollars from the Bureau of Labor Statistic. The DB1B also provides information on the fare class of each ticket (coach-class, business-class, or first-class) so we drop all business-class and first-class tickets.

We also drop tickets if the ticketing and operating carriers are different due to code-sharing arrangements among major airlines but not due to outsourcing subcontract between major and regional airlines. Code-sharing occurs when a ticket is sold by a major airline and the flight is operated by a rival major airline, whereas outsourcing occurs when a ticket is sold by a major airline and the flight is operated by a regional airline.

Next, we drop tickets in which the ticketing carrier or operating carrier are not reported. Following Gerardi and Shapiro (2009) and Ciliberto and Williams (2014), we drop airline-route observations that do not have at least 100 passengers in DB1B in order to not only eliminate possible coding errors but also have adequate coverage to calculate reliable price dispersion statistics. We treat round-trip tickets as two one-way tickets, and divide the round-trip fare by two, and drop one of two observations to avoid double-counting.

There is an average of 1.66 ticketing carriers per route, where the minimum is 1 and the maximum is 7 of ticketing carriers. Since major airlines determine the prices for flights operated by regional airlines and airfares are calculated by ticketing airline, regional airlines are not counted as separate competitors and their capacity is merged with that of the major carriers for the purpose of market share computation.

Each airline appears on average for 48.89 quarters (around 12 years). Based on our category of majors and low-cost carriers, major carriers appear on average for 83.88 quarters (around 21 years) and low-cost carriers appear on average for 33.3 quarters (around 8 years). The shorter time span for low-cost carriers is due to the a high incidence of entry and exit. Lastly, we drop monopoly markets since average multimarket contact is undefined for monopoly markets.

Our final unbalanced panel sample contains 242,088 carrier-route-time observations spanning 26 airlines, 4,409 distinct routes, and 100 quarters between 1993:Q1 and 2017:Q4.[17] There have been a decreasing time trend in the number of carriers over the 25 years in our sample with an average of around 13 ticketing carriers operating per year-quarter (the minimum and maximum are 8 in recent years and 19 in the late 1990s). In our sample, the number of routes in our sample is larger than the 2,902 routes in Gerardi and Shapiro (2009) because of entry and exit that occurred in the differing time range. However the number of routes in our sample is smaller than the

[17] Following Gerardi and Shapiro (2009), our sample includes 26 airlines. The 8 major carriers are American (AA), Alaska (AS), Continental (CO), Delta (DL), Northwest (NW), Trans World (TW), United (UA), and US Airways (US). The 18 low-cost carriers are JetBlue (B6), Frontier (F9), AirTran (FL), ValuJet (J7), Morris Air (KN), Kiwi (KP), National (N7), Vanguard (NJ), Spirit (NK), Pro Air (P9), Reno (QQ), Sun Country (SY), American Trans Air (TZ), Western Pacific (W7), Eastwind (W9), Southwest (WN), Air South (WV), and Access Air (ZA).

6,366 routes in Ciliberto and Williams (2014) since we drop ticketing airline-route-time observations that do not have at least 100 passengers as discussed above.

Appendix: Demand and Marginal Cost Analysis

In this online appendix, we investigate why tacit collusion is unlikely to occur in Southwest markets by estimating a demand equation using OLS and 2SLS as in Gayle (2013). Market miles flown and the interaction between jet fuel price and market miles flown are used as instruments for airfare since the price of a product (e.g. a flight) is typically influenced by changes in its marginal cost. Table 1.A1 presents the regression results for the demand estimations. As expected, the coefficient estimate on $lnFare$ is negative, implying that higher prices are associated with lower levels of utility. In other words, passengers prefer cheaper air travel products, all else equal.

Table 1.A1: Demand Estimation

	(1)	(2)
VARIABLES	OLS	2SLS
$lnFare$	-0.465***	-3.024***
	(0.014)	(0.281)
$Networksize$	0.776***	0.976***
	(0.052)	(0.071)
$Roundtrip$	2.431***	1.238***
	(0.013)	(0.129)
Hub	0.310***	0.391***
	(0.039)	(0.046)
Under-id		90.734
		(0.000)
Over-id		0.211
		(0.646)
Observations	405,201	405,201

Notes: (i) The Kleibergen-Paap rk LM statistic is used for under-identification test while the Hansen's J statistic is used for over-identification test. (ii) $lnFare$ is instrumented by market miles flown and the interaction between jet fuel price and market miles flown. (iii) Carrier-route and carrier-time fixed effects are included in all regressions. (iv) Route-specific clustered standard errors in parentheses. (v) *** p¡0.01, ** p¡0.05, * p¡0.1.

Following Gayle (2013), we then impute the average marginal costs for eight airlines (listed in alphabetical order by IATA code). Table 1.A2 reports that Southwest has the lowest average marginal cost compared to major airlines. This is consistent with the calculations in the existing literature; for example, Gayle (2013) estimates that Southwest's average marginal cost is \$117.95. Since Scherer (1980) explains that a collusive agreement is more likely to break down if the participating firms have different marginal costs, it makes sense that tacit collusion could occur in non-Southwest markets since the major airlines have similar average marginal costs. However, it would be difficult to maintain tacit collusion in Southwest markets given the stark contrast in Southwest's marginal cost compared to the major airlines.

Table 1.A2: Average Marginal Costs

Carrier	Code	MC (\$)
American Airlines	AA	218.698
Continental Airlines	CO	250.434
Delta Air Lines	DL	209.179
Northwest Airlines	NW	240.825
Trans World Airlines	TW	213.644
US Airways	US	215.254
United Airlines	UA	227.558
Southwest Airlines	WN	137.179

Chapter 2

Mergers and Labor Market Outcomes in the US Airline Industry

2.1 Introduction

Thousands of merger and acquisition transactions take place in the U.S. each year, affecting millions of involved workers' labor market outcomes. Since protecting consumer welfare is at the core of antitrust policies and regulations, researchers and antitrust authorities have long been interested in examining the competitive effects of mergers in consumer product markets, with existing literature addressing both pricing (e.g., Prager and Hannan 1998; Weinberg 2008; Hunter et al. 2008; Ashenfelter et al. 2013; Miller and Weinberg 2017, among others) and quality dimensions (e.g., Sheen 2014; Prince and Simon 2017; Chen and Gayle 2019; Rupp and Tan 2019, among others). However, despite its direct policy relevance and importance, the labor market impact of mergers and acquisitions has received considerably less albeit revived attention.

A priori, the theoretical prediction of how mergers affect employees' wage and employment outcomes is inherently ambiguous. On the one hand, according to Naidu et al. (2018), mergers can lead to concentration in both the product and labor markets, giving merged firms monopoly power in the product market and monopsony power in the labor market. By exercising their monoposony power, employers can thus hold wage below workers' marginal revenue product and reduce employment as an effort to reduce labor cost (Naidu et al., 2018). On the other hand, the presence of labor unions can strengthen employees' bargaining power and in turn dampen the monopsony impact of mergers as well as employers' ability to lower wages and reduce employment (Benmelech et al., 2018). Indeed, existing literature generally finds mixed evidence regarding how mergers affect wage and employment based on cross-industry analyses (Brown and Medoff 1988; Conyon et al. 2001; Conyon et al. 2004; He and le Maire 2019; Arnold 2019), or case studies on specific sectors and industries, such as manufacturing (Peoples 1989; McGuckin and Nguyen 2001; Siegel and Simons 2010), healthcare (Currie et al. 2005; Prager and Schmitt 2019), and railroad (Davis and Wilson 1999; Davis and Wilson 2003). Our study contributes to these ongoing discussions by examining the wage and employment impact of mergers in the airline industry featuring unique institutional background that may further shed light on the topic.

Utilizing airline wage and employment information from Form 41 Air Carrier Financial Reports and Schedules P-1, P-6, and P-12 from the Bureau of Transportation Statistics (BTS), we consider horizontal airline merger episodes between 1993 and 2018 and examine both short-term and long-term impact of mergers on the merging airlines' overall wage and employment outcomes. The US airline industry presents a unique opportunity to study the labor market impact of mergers because 1) the industry has witnessed numerous merger episodes since the deregulation era, involving legacy airlines, low cost carriers (LCCs) and regional carriers; 2) the airline industry has the highest union presence among all private sector industries with substantial collective bargaining power to influence employees' compensation and welfare (Hirsch, 2007); and 3) airline mergers are typically accompanied by a contentious workforce seniority integration process that can directly affect the wage and employment outcomes of the involved employees.[1] On the other hand, studying the labor market impact of airline mergers faces an identification challenge due to potentially endogenous merger decisions, e.g., during bankruptcy buyouts, cost considerations can affect both merger target choices and employment outcomes. To this end, we adopt a difference-in-differences framework similar to Ashenfelter et al. (2013), Prince and Simon (2017) and Chen and Gayle (2019) by comparing merging airlines with the uninvolved non-merging airlines before and after the merger announcements. To further alleviate the endogeneity concerns, we also validate our findings by re-estimating the specifications with a subsample of non-bankrupt airlines and by applying inverse probability of treatment weighting (IPTW) with propensity scores.

Overall, our results point to a consistently negative labor market impact of airline mergers in the U.S. Specifically, we find an approximately 5% significant reduction in the merging airlines' per capita salaries and fringe benefits in the three to five years following the merger announcements. The negative wage impact is larger among large-scale mergers involving major airlines and low cost carriers. Our estimates also indicate a negative short-term (i.e., within the first two years of the merger announcements) employment impact of mergers that varies with occupation types.[2] Our results are robust to a number of sensitivity tests, including the implementation of the IPTW

[1]Seniority integration involves integrating unionized workers from both airlines into a single workforce. While all mergers need to resolve the workforce integration issue, regardless of their industries, the integration issue is particularly acute in the airline industry and often requires independent arbitration when the merging airlines rank employees by seniority (Lee and Singer, 2014).

[2]The occupation types that we consider include pilots, flight attendants, mechanics and maintenance crew, traffic and passenger/cargo handling units, managerial staff, and other miscellaneous staff members. Due to data limitations, we are unable to analyze the wage impact by airline occupation types.

method and the inclusion of additional controls for airlines' unionization levels and financial leverage as well as subsample analyses focusing on non-bankrupt airlines.

Our findings are consistent with merger-induced monopsony power discussed in recent studies such as Benmelech et al. (2018), Naidu et al. (2018) and Marinescu and Hovenkamp (2019). The within-airline seniority ranks impose significant switching costs to airline employees (Hirsch, 2007) and can in turn hinder cross-airline mobility (Fox 2010; Ransom 2019). Such barriers to mobility can effectively further reduce the elasticity of labor supply, strengthen the monopsony power, and impose downward wage pressure according to the monopsony model in Card et al. (2018). On the other hand, the monopsony impact of mergers in the airline industry is also complicated by its high union presence because unionization reduces the employer's ability to lower wages by improving employees' bargaining power (Benmelech et al., 2018). Our results regarding the long-run wage suppression therefore imply that while the seniority integration process can retard the efficient integration of the merging airlines, it may in fact reinforce the integrated airlines' monopsony power over time even in the presence of industry-wide high level of unionization.

Our paper contributes to the literature in several ways. First, to our knowledge, this paper is the first to study the impact of mergers on the labor market outcomes in the airline industry. Previous retrospective merger studies in the airline industry tend to focus exclusively on product market competition, e.g., Kwoka and Shumilkina (2010) and Luo (2014) on price competition; Prince and Simon (2017) and Chen and Gayle (2019) on quality competition; and Rupp and Tan (2019) on de-hubbing effects. Such focus presents an unfortunate void given the industry's unique institutional features, such as its high union presence and the integration of workforce seniority ranks during mergers, that may lead to competing channels affecting the post-merger monopsony power and labor market outcomes. Our paper seeks to fill this gap and contribute additional empirical evidence toward the growing discussions regarding the monopsony impact of mergers (Naidu et al. 2018; Marinescu and Hovenkamp 2019; Arnold 2019).

Secondly, standard monopsony models do not offer predictions regarding the short-run versus long-run labor market impact of mergers. While related studies mostly focus on the short-run impact, our paper investigates both short-term and long-term impact of mergers on labor market outcomes. Similar to the reasoning offered by merger studies focusing on product markets (e.g., Focarelli and Panetta 2003; Prince and Simon 2017), the impact of mergers on the labor market can also be time-varying because merged firms could struggle with the integration process in the short-run and may only begin to achieve efficiency gains and exercise monopsony power in the longer-run. Indeed, our results document a short-term downsizing in labor force with a long-term wage suppression following airline merger episodes.

Lastly, our paper exploits airline-level data from the BTS and adds to the limited discussions regarding the airline labor market, where existing studies tend to utilize data from the Current Population Survey (CPS) or the Census (e.g., Card and Saunders 1998; Hirsch and Macpherson 2000; Hirsch 2007;). While such data sources offer rich accounts of individual attributes of airline employees, they do not allow differentiation across carriers as well as matching with airline merger episodes (see Hirsch (2007) for a discussion on the advantages and drawbacks of airline-level vs. individual-level data). Additionally, with the exception of theoretical studies (e.g., Lee and Singer 2014), the impact of workforce seniority integration processes has not been examined. Although our data do not allow for a direct analysis, our empirical results imply that the seniority integration process may help reinforce merged airlines' monopsony power.

The remainder of the paper is organized as follows. Section 2 outlines the institutional background and conceptual framework for our study. Section 3 describes the data and empirical strategy in our study. Section 4 presents our main findings and performs robustness checks, followed by discussion and concluding remarks in Section 5.

2.2 Background and Conceptual Framework

Labor markets mirror product markets in that concentration can give rise to market power (Naidu et al., 2018). Specifically, concentration in the labor market could give employers monopsony power over employees' wage and employment outcomes. Since its deregulation in 1978, the U.S. airline industry has witnessed over two dozen merger cases. It has been well documented that these merger episodes led to concentration and market power in the consumer product market (e.g., Prince and Simon 2017; Chen and Gayle 2019), which is typically defined as either airport or city pairs in the relevant literature. In the labor market context, Arnold (2019) utilizes matched employer-employee data across different industries and documents variations in local labor market concentration following mergers and acquisitions. Specific to the airline industry, while its labor market is arguably less well defined and typically lacks supporting administrative level data,[3] we would still expect an overall increase in airlines' monopsony power as the total number of airlines falls following merger episodes. A standard monopsony model would then predict a lower wage rate coupled with a lower level of employment in the integrated firm. Such prediction on the labor market impact of mergers has been (partially) supported by a number of recent retrospective merger studies across different industries (e.g., Currie et al. 2005; Siegel and Simons 2010; Prager and Schmitt 2019; Arnold 2019). Additionally, similar to Arnold (2019), we also hypothesize a larger impact from mergers with larger scale, e.g., legacy airline

[3] Compared to the healthcare industry, labor market in the airline industry is far less geographically defined. For instance, pilots are generally assigned to their aircraft, flight schedules and bases through an unpredictable bidding process.

mergers are expected to see a larger wage and employment impact compared to mergers involving regional affiliates.

Two unique institutional features in the airline industry can lead to competing channels that may complicate the post-merger monopsony power and labor market outcomes. First, the airline industry features the highest union representation among all private sectors in the U.S. (Hirsch, 2007). By strengthening employees' bargaining power, unionization can dampen the monopsony impact of mergers and reduce the employer's ability to lower wages. Such prediction is consistent with findings in Benmelech et al. (2018) that demonstrate a more pronounced negative relationship between labor market concentration and wages when unionization rate is low.

Secondly, employment in the airline industry is also characterized by a highly hierarchical within-firm and by-position seniority ranking system.[4] Despite the high within-industry transferability of skills, such seniority ranking system essentially increases employees' switching costs and in turn limits cross-airline mobility of employees in the airline industry (Hirsch 2007; Fox 2010; Ransom 2019).[5] While the antitrust authorities focus on market power and consumer welfare when reviewing firms' merger proposals, the success of the eventual efficient integration of the merging airlines also relies on the full seniority integration of the respective heavily unionized labor forces. The seniority integration process involves combining the merging airlines' seniority ranks for each position and has direct impact on employees' current and future compensations and benefits in the merged airline. While the recently enacted McCaskill-Bond Amendment required airlines to integrate the seniority lists in a "fair and equitable" manner (Lee and Singer, 2014), there are no legal or practical guidelines on how to integrate relative

[4]When switching employers, an airline employee would typically need to given up the current rank and restart the seniority clock at the new employer.

[5]Note that this does not necessarily imply an infinite switching cost or perfectly inelastic labor supply. For instance, switching costs tend to increase with job seniority, which implies that lowly ranked entry-level employees would typically face much lower switching costs.

ranks of employees in the same occupation across two merging airlines. It comes as no surprise that despite approvals from the antitrust authorities, many past airline mergers in the U.S. needed to resort to binding and independent arbitration to resolve seniority integration issues and the resulting labor strife (Lee and Singer, 2014).

An important implication of the airline industry's seniority-ranked labor force and the resulting complications when integrating the ranks across merging airlines is that by combining two already inelastic labor forces into a single seniority rank system, the merger may further raise switching frictions (e.g., there are even fewer outside options as a result of the merger) and depress the elasticity of labor supply. Card et al. (2018) theorize a monopsony model in which a monopsonist sets wage premiums that are highly correlated across skill groups but inversely related to the elasticity of labor supply to the firm. Consistent with this model, we therefore expect that the employer's merger-induced monopsony power would be strengthened through the integration of the seniority rank and there would correspondingly be a stronger overall downward pressure on both wage and employment levels.

Overall, given the unique institutional features of the airline industry, the strength of post-merger monopsony power would depend on whether the unionization effect or the impact of the seniority integration process dominates. If it is the latter, we would expect airline mergers to result in lower wage rates and more workforce downsizing. In addition, while standard monopsony models do not necessarily differentiate between short-run and long-run labor market impact, the product market impact of mergers can vary over time because merged firms could still struggle with the integration process in the short-run and may only begin to achieve efficiency gains in the longer-run (Focarelli and Panetta 2003; Prince and Simon 2017). We would thus hypothesize a similar time-varying impact of mergers in the labor market. Our empirical investigation will take into account short-term versus long-term labor market impact of airline mergers as well as potential heterogeneity across different occupation groups.

2.3 Data and Empirical Strategy

2.3.1 Data Sources and Description

Our study focuses on 13 horizontal airline mergers between 1993 and 2018.[6] Unlike many retrospective merger studies in the airline industry, the merger cases we consider involve legacy airlines, low cost carriers and regional airlines, i.e., our sample is not limited to mergers between legacy airlines as typically considered in related literature. We construct our merger incident timelines based on extensive searches from air carriers' official websites as well as a number of major news article databases.[7] We also distinguish mergers that involved airlines that declared bankruptcy prior to the merger incidents. Table 2.1 details the list of involved airlines and the respective merger announcement dates. The full integration date entails that the merging airlines have fully integrated their ticketing systems and joint itineraries with a single operating certificate from the FAA, which previous airline merger studies such as Chen and Gayle (2019) consider as the official completion of the merger. We, on the other hand, choose to rely on the merger announcement date to identify the post-merger labor market impact because it typically takes years for airlines to fully integrate and the merging airlines' wage and employment decisions may have already been drafted at the time of the official merger announcement.[8]

[6]We only consider merger episodes where the merging airlines were full integrated within the sample period. More recent mergers such as Republic (YX)-Shuttle America (S5) and Alaska (AS)-Virgin America (VX) mergers in 2016 are not included in our study since their full integration extends beyond our sample period.

[7]The news databases include `archive.org`, *The New York Times*, *The Wall Street Journal*, *USA Today*, etc.

[8]Another constraint with relying on full integration date to identify merger impact is that the date may be less reliably documented in public data sources, particularly for mergers involving regional affiliates.

Table 2.1: Merger Episodes

Merging Carriers	Carrier Codes	Pre-Merger Bankruptcy	Announcement	Full Integration
Majors				
American-Reno	AA-QQ	X	11/19/1998	8/30/1999
American-Trans World	AA-TW	TW:1/10/2001	1/10/2001	12/1/2001
US Airways-America West	US-HP	US:9/12/2004	5/19/2005	9/25/2007
Delta-Northwest	DL-NW	X	4/14/2008	1/31/2010
United-Continental	UA-CO	X	5/3/2010	11/3/2011
American-US Airways	AA-US	AA:11/29/2011	2/14/2013	10/17/2015
LCCs				
Southwest-Morris	WN-KN	X	12/13/1993	10/4/1994
AirTran-Valujet	FL-J7	Δ J7	7/11/1997	11/17/1997
Southwest-ATA	WN-TZ	TZ:4/2/2008	11/19/2008	-
Southwest-AirTran	WN-FL	X	9/27/2010	3/1/2012
Regionals				
Republic-Midwest	YX-YX (1)	Δ YX (1)	6/23/2009	11/3/2009
Endeavor-Mesaba	9E-XJ	X	7/1/2010	1/4/2012
Atlantic Southeast-ExpressJet	EV-XE	X	8/4/2010	12/31/2011

Notes: (i) All merger episodes are horizontal mergers. (ii) Announcement date is the first date that a merger was announced to the public, and it is obtained through sources including carriers' official websites and news databases. (iii) Cases marked with Δ in the Pre-Merger Bankruptcy column indicates that the merging carriers were not officially bankrupt but suffered from serious financial issues. (iv) Full integration entails that the merging airlines have fully integrated their ticketing systems and joint itineraries with a single operating certificate from the FAA. (v) The WN-TZ merger was finalized but its full integration date is not reliably documented in publicly available sources.

Carrier-level wage and employment information is derived from a number of data sources provided by the Bureau of Transportation Statistics. In particular, Form 41 Financial Reports contains financial information for certified U.S. air carriers, including balance sheet, cash flow, employment, income statement, fuel cost and consumption, aircraft operating expenses, and operating expenses. Schedule P-6 Expenses by Objective Grouping contains quarterly operating expenses for carriers with annual operating revenues of $20 million or more and includes items such as salaries, benefits, materials purchased, services purchased, depreciation, amortization, food, and other operating expenses. In addition, Schedule P-1A Employees provides monthly carrier employment information, which breaks down the total number of employees by full time and part time employees. We aggregate the employee count information to quarterly level and use it to calculate the per capita benchmark for quarterly salary and salary plus fringe benefits.

Additionally, we utilize Schedule P-10 Annual Employees Statistics by Labor Category that provides annual numbers of total employees by occupation types.[9] Other carrier-year-quarter level characteristics such as revenue passenger miles, market miles flown, load factor, and number of enplanements that capture the size and efficiency of various dimensions of airline operations are obtained from Form T-100. In addition, we account for an airline's unionization level and construct the debt-to-asset ratio as a proxy for the carrier's financial leverage.[10] Lastly, all nominal variables are converted into real values in 2018 prices using the Consumer Price Index (CPI) from the Bureau of Labor Statistics (BLS).

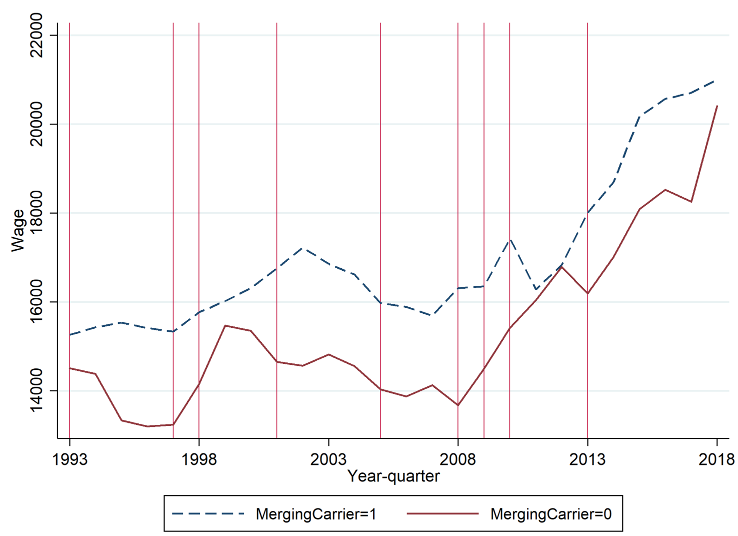
It is worth noting that prior airline labor market studies tend to utilize data from the Current Population Survey (CPS) or the Census (e.g., Card and Saunders 1998; Hirsch and Macpherson 2000; Hirsch 2007). While such data sources offer rich accounts of individual attributes of airline employees, they do not provide the appropriate data support for our study as individual level data do not allow differentiation across carriers as well as matching with airline merger episodes (see Hirsch (2007) for a discussion on the advantages and drawbacks of airline level vs. individual level data).

[9] Note that Schedule P-10 groups workers into 15 different categories, while the wage information from Schedule P-6 classifies them into six categories. The specific categories from Schedule P-6 are as follows: 1) Pilots (Pilots and Co-pilots + Other Flight Personnel); 2) Flight Attendants (Passenger/General Services and Administration); 3) Mechanics and Maintenance (Maintenance); 4) Traffic and Handling (Aircraft and Traffic Handling Group 1 + General Aircraft and Traffic Handling + Aircraft Control + Passenger Handling + Cargo Handling); 5) General Management (General Manager); and 6) Other (Trainees and Instructors + Statistical Personnel + Traffic Solicitors + Other Transport Related).

[10] The unionization and debt-to-asset measures are described in detail in Section 4.2.

Tables 2.2 and 2.3 provide a list of the variables employed in our study and their descriptive statistics (at carrier-year-quarter level), respectively. Table 2.A1 in the Appendix also shows the summary statistics for the legacy airline and LCC subsample. Since the mergers in our study involve airlines of different scales (mostly legacy airlines and LCCs), it is not surprising that our variables of interest demonstrate large variances and are driven by the legacy airline and LCC subsample. In addition, Figure 2.1 (Figure 2.A1) plots the evolution of quarterly average wage (average wage plus fringe benefits) with respect to the merger episodes for employees in merging and non-merging airlines. While employees in merging airlines on average enjoy a higher wage (and wage plus fringe benefits) compared to their non-merging airline counterparts, the two trajectories follow fairly similar common trends over time. Such observations help motivate our difference-in-differences approach that we will describe in detail in the next subsection.

Figure 2.1: Merging and Non-Merging Carrier's Quarterly Wage Time Trend



Note: Vertical lines indicate merger announcement years. Merging carriers are those that were involved in mergers at any point during our sample period. Non-merging carriers are those that were never involved into any merger.

Table 2.2: Variable Sources and Descriptions

Variable Descriptions	
Carrier	Operating carrier, i
Time	Year-quarter, t
Year	Year, y
Schedule P-6	
Wage	Total salaries of all employees/Number of total employees, it
WageBene	Total salaries + fringe benefits of all employees/ Number of total employees, it
KMP	Total capital and material costs/Revenue departures performed, it
FC	Total fuel costs/Total miles flown, it
Schedule P-1A	
Empfull	Number of full-time employees, it
Emppart	Number of part-time employees, it
Emptotal	Number of total employees, it
Schedule P-10	
GeneralManage	Number of general management employees, iy
PilotsCopilots	Number of pilot and co-pilot employees, iy
FlightAttendents	Number of flight attendant employees, iy
Maintenance	Number of mechanic and maintenance employees, iy
Traffic	Number of aircraft and traffic handling employees, iy
OthersEmp	Number of other employees, iy
Emptotaly	Number of total employees, iy
Form T-100	
RPM	Revenue Passenger Miles= N_enplanements×Total miles flown, it
ASL	Average Stage Length= Total miles flown/N_departures, it
ASM	Available Seat Miles=N_seats×Total miles flown, it
N_enplanements	Total number of enplanements, it
N_routes	Total number of directional routes, it
Loadfactor	N_enplanements/N_seats, it
Networksize	Total number of routes from origin of carrier i / Total number of routes from origin, it
Bankruptcy	Indicator equal 1 if carrier i is bankrupt at time t, it
N_hub	Total number of routes involving hubs, it
Distance	Average miles flown, it
N_departures	Revenue departures performed, it
N_seats	Total number of seats, it

Table 2.3: Summary Statistics for All Carriers Sample

	Mean	S.D.	Min	Max
Wage	15,512	5,572	450	70,820
Wage+Benefits	21,312	7,938	545	89,641
Total Employment	11,849	22,331	25.30	108,767
Merger02	0.042	0.200	0	1
Merger35	0.036	0.186	0	1
Capital Material Costs (KMP)	596,234	7,497,692	449	388,615,776
Fuel Costs (FC)	641	5,136	0.000	155,760
Revenue Passenger Miles (RPM)	2,474,362	3,114,997	468	26,912,346
Average Stage Length (ASL)	135	313	0.431	6,035
Average Seat Miles (ASM)	3,288,074	3,894,663	1,459	29,481,190
Debt-to-Asset	0.921	0.645	-2.45	10.70
Unionization	0.148	0.355	0	1
N_enplanements	3,932,740	6,684,090	5	42,872,537
N_routes	311	350	1	1,913
Load Factor	0.661	0.137	0.006	1
Network Size	0.265	0.149	0.007	1
Bankruptcy	0.003	0.057	0	1
N_hub	73.40	182	0	1,055
Distance	804	489	42	6,035

Note: (i) Observations are at carrier-year-quarter level, and sample size is 4,084 for all variables. (ii) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (iii) Carrier-level time-varying controls are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (iv) All nominal variables are converted into real values in 2018 prices using the CPI from the BLS.

2.3.2 Empirical Strategy

We follow prior retrospective merger studies in the airline industry, e.g., Prince and Simon (2017) and Chen and Gayle (2019), and adopt a difference-in-differences (DID) approach that compares the change in labor market outcomes of merging airlines before and after the merger episode, relative to the non-merging airlines over the same pre- and post-merger periods.[11] Similar to Prince and Simon (2017), we also allow the

[11] In our DID setup, for a given merger, the non-merging carriers in the control group consist of carriers that were not involved in that specific merger. In other words, this would include carriers that were never involved in any merger episode and carriers that merged as part of a different merger during our sample period. For instance, UA and CO would serve as the time-wise control for the treated (WN and FL) when analyzing the WN-FL merger. The identification strategy assumes that,

impact of mergers to vary over time by distinguishing short-term versus long-term effects. Specifically, we consider the following DID specification:

$$y_{it} = \alpha + \beta_1 Merger02_{it} + \beta_2 Merger35_{it} + \gamma X_{it} + \rho_i + \sigma_t + \epsilon_{it} \tag{2.1}$$

where y_{it} denotes the labor market outcomes including wage per worker, wage plus fringe benefits per worker, and total employment, for carrier i at time t; X_{it} is the set of time-varying carrier-level controls derived from Schedule P-6 and Form T-100 as described in Table 2.2;[12] ρ_i and σ_t are the carrier and time fixed effects, respectively. Following Prince and Simon (2017), $Merger02_{it}$ is a dummy variable denoting whether the carrier of interest merged during the previous two years (eight quarters). Similarly, $Merger35_{it}$ indicates whether the carrier merged within the past three to five years (i.e., between the past 9 and 20 quarters). We define the merger dummies equal 1 upon the official announcement from the acquiring and acquired airlines.[13] To study the labor market impact of mergers, the coefficient on $Merger02_{it}$ thus identifies the short-term effect of the merger, while the coefficient on $Merger35_{it}$ absorbs the longer-term impact. Specifically, the coefficient on $Merger02_{it}$ captures the change in labor

as is the case in our sample, the announcement (or completion) quarters do not perfectly coincide between mergers.

[12]Following prior literature (e.g., Banker and Johnston 1993), we include these time-varying carrier level characteristics to control for cost drivers of airline operations that may affect wage and employment determination. For instance, Capital Material Costs (KMP) and Fuel Costs (FC) capture the costs of other factor inputs; while Revenue Passenger Miles (RPM), Average Stage Length (ASL), and Average Seat Miles (ASM) are proxies for airlines' output capacities. Overall, we would expect these controls to be positively correlated with an airline's average employment levels and average wages.

[13]In many cases, the acquiring and acquired airlines continue to operate under their separate brands post-merger. But in cases like the Southwest-Morris merger, where Morris was acquired and stopped operating as a separate brand post-merger, we would focus on the acquiring carrier for the pre- and post-merger comparison – pre-merger data from the acquired carrier are only utilized as a potential control group for other horizontal mergers (if applicable). Our study also considers two alternative treatments of acquiring and acquired carriers, including 1) dropping all acquired airlines in our sample; and 2) modifying the carrier identifiers to combine the acquiring and acquired carriers' pre-merger data. The corresponding results are qualitatively similar to our baseline findings and are available upon request.

market outcomes for carrier i in the first two years after merging relative to the change in labor outcomes for carriers that did not engage in mergers during the previous two years, within the same pre- and post-merger periods. Similarly, the coefficient on $Merger35_{it}$ captures the change in labor market outcomes for carrier i in the third to fifth years after merging relative to the change in labor outcomes for carriers that did not merge during the previous three to five years, within the same pre- and post-merger periods.

We first estimate Equation 2.1 using the full list of 13 merger episodes with the control group being based on non-merging major carriers, LCCs, and regional carriers. Because there exist significant wage premiums for major airline employees over their regional airline counterparts (Hirsch and Macpherson 2000; Hirsch 2007) and given the large variability in the regional airlines in terms of their sizes, routing, financial conditions, etc, we also consider a subsample of six large-scale merger cases that only involve major carriers and LCCs and exclude regional carriers in the comparison group in order to define a more comparable control group of airlines not involved in the mergers.[14]

As outlined in Section 3.1, Figures 2.1 and 2.A1 provide some preliminary visual evidence supporting the parallel trends assumption in our empirical context. To further assess the validity of the parallel trends assumption for our difference-in-differences model, we consider the following generalized DID specification:

$$y_{it} = \alpha + \sum_{\tau=-8}^{-1} \beta_\tau Leads_{it+\tau} + \beta_1 Merger02_{it} + \beta_2 Merger35_{it} + \gamma X_{it} + \rho_i + \sigma_t + \epsilon_{it} \quad (2.2)$$

where $Leads_{it+\tau}$ are time dummies that equal 1 if it is the τ^{th} quarter (for up to eight

[14]The six merger cases involving majors and LCCs include AA-TW, US-HP, DL-NW, UA-CO, WN-FL, and AA-US. The selection is based on the five merger cases studied in Prince and Simon (2017), which we then augment with the more recent AA-US merger.

quarters) before the merger of airline i at time t was announced and 0 otherwise.[15] The time dummies are thus mutually exclusive of each other so that the implied effects on the dependent variable given by their coefficients are not additive.

Similar to other retrospective merger studies, a potential endogeneity issue in our empirical design is that the merger outcomes could be the result of endogenous decisions made by the acquiring and target airlines. In airline retrospective studies that consider impact of mergers on product quality (e.g., Prince and Simon 2017 and Chen and Gayle 2019), the endogeneity issue may not be as apparent since they consider the impact of mergers on product quality, where the motivation to merge may only be indirectly correlated with product quality changes. In our context, however, according to Arnold (2019), acquiring firms may selectively target firms that will be profitable in the future, e.g., new low cost carriers experiencing fast growth. We would thus expect a target airline to grow due to the increased productivity even absent of any merger. Therefore, the estimated impact of mergers on labor market outcomes could be biased upward. On the other hand, acquiring firms could also target mismanaged businesses that are underperforming. If merger targets are chosen in such a way, we may expect wage and employment to be falling in target firms before the merger. Therefore, the estimates could also be downward biased if the falling employment at target firms would have been even greater absent of the merger.

As an effort to alleviate these identification concerns, we first conduct a sensitivity test on the main DID model itself. Specifically, despite the checks for the parallel trends assumption described earlier, one might still be concerned about the pre-treatment characteristics being related to the dynamics of the dependent variables and unbalanced between groups. We thus employ an inverse probability of treatment weighting

[15] Here, we define two years preceding a given merger as the pre-merger period and up to five years following the merger as the post-merger period. Note that data beyond five years post-merger are still retained.

method using propensity scores with its implementation procedures described in detail in Section 4.2. Next, we also re-estimate our main DID specifications for a subsample of large-scale airline mergers that only involve legacy carriers and LCCs (i.e., excluding mergers involving regional affiliates), which presents more consistent comparison groups compared to mergers involving regional carriers. Lastly, we consider a subsample of mergers that exclude bankrupt airlines to mitigate the concern regarding the acquiring airlines strategically targeting underperformers.[16]

2.4 Results

2.4.1 Baseline Findings

We first examine the wage impact of mergers by estimating Equation 2.1 using natural log of salary per employee and salary plus fringe benefit per employee as the dependent variables. Since the dependent variables of interest already account for carriers' quarterly total employment, our study will thus focus on wage impact as the main labor market outcome of interest. Table 2.4 presents our baseline DID estimates based on the full sample of carriers included in our data (Columns (1) and (2)) as well as a subsample focusing on large-scale mergers involving major carriers and LCCs (Columns (3) and (4)). Our main variables of interest are *Merger02* and *Merger35*, which capture the short-term (2 years) and long-term (3-5 years) wage impact of mergers. While we do not observe any short-run wage impact of mergers, the merging airlines consistently experience an approximately 5% decrease in the per capita salary (and fringe benefits) in the three to five years following the merger episodes, relative to the non-merging

[16] As argued in McGuckin and Nguyen (2001), the difference in pre-merger performance is systematic of differing motivations for the merger – poorly performing plants are more likely to involve managerial discipline motives, while synergies are more likely to drive acquisitions of above-average performers. In this sense, robustness check with a subsample that excludes bankruptcy related mergers may only partially solve the endogeneity issue.

control group airlines during the same pre- and post-merger periods. When we focus on large-scale mergers involving major airlines and LCCs, the magnitude of the long-term wage reduction increases to approximately 9%, which echoes the findings in Arnold (2019) that the labor market impact is larger in mergers with higher stakes.

Table 2.4: Wage Impact (DID)

	All Carriers		Majors and LCCs	
	(1)	(2)	(3)	(4)
	lnWage	lnWageBene	lnWage	lnWageBene
Merger02	0.015	0.004	0.000	-0.016
	(0.013)	(0.013)	(0.015)	(0.016)
Merger35	-0.050***	-0.058***	-0.084***	-0.093***
	(0.010)	(0.011)	(0.013)	(0.013)
Capital Material Costs (KMP)	0.104**	0.107**	0.020	0.000
	(0.044)	(0.045)	(0.050)	(0.059)
Fuel Costs (FC)	0.004	0.005	0.071	0.070
	(0.005)	(0.005)	(0.046)	(0.054)
Revenue Passenger Miles (RPM)	-0.093***	-0.075***	0.077	0.184
	(0.026)	(0.027)	(0.124)	(0.122)
Average Stage Length (ASL)	0.016	-0.060	0.234***	0.181**
	(0.045)	(0.045)	(0.076)	(0.077)
Average Seat Miles (ASM)	0.131***	0.045	0.105	-0.066
	(0.032)	(0.035)	(0.122)	(0.123)
Observations	4,082	4,082	1,462	1,462
Number of Carriers	108	108	25	25
R-squared	0.696	0.746	0.808	0.855
Carrier FE	y	y	y	y
Time FE	y	y	y	y

Note: (i) All models are estimated using DID. (ii) The Majors and LCCs subsample is based on six large-scale mergers involving major carriers and LCCs with the control group being non-merging major carriers and LCCs. (iii) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (iv) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natual logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (v) Other time-varying carrier characteristics such as *N_enplanements*, *Nroutes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (vi) All models include carrier and year-quarter fixed effects. (vii) Robust standard errors are in parenthesis. (vii) *** p<0.01, ** p<0.05, * p<0.1.

To complement to our findings on the wage impact, we next present the baseline employment impact. Table 2.5 presents the DID estimates based on the full sample as well as the major airline and LCC subsample. In addition, we also distinguish between full-time and part-time employment in Columns (2) and (3) and Columns (5) and (6) for the full sample and the major carrier/LCC subsample, respectively. The coefficient on the *Merger02* variable in Column (1) suggests that the merging airline with a merger within the past two years is associated with approximately an 15% decrease in its overall employment level, relative to the non-merging control group airlines in the same pre- and post-merger periods. On the other hand, we do not observe a similar reduction in employment in the longer-term as indicated by the statistically insignificant coefficient on the *Merger35* variable. When focusing on large-scale mergers involving major carriers and LCCs, we observe in Column (4) a much smaller short-term reduction but a larger long-term decrease in employment compared to the full sample results. In Table 2.A2 in the Appendix, we also provide evidence of potential heterogeneity in the employment impact of airline mergers by occupation types, with short-term downsizing primarily among flight attendants and ground crew, and long-term expansion in managerial staff.[17]

Overall, our baseline results point to a significant reduction in merging airlines' long-term per capita salaries and fringe benefits following the mergers. The effect is particularly salient among large-scale mergers involving major airlines and LCCs. Our results also document a negative short-term employment impact of mergers with varying magnitudes depending on the time frame and occupation types.

[17]While Schedule P-10 categorizes the total employment into specific occupation types, it does not offer the matching salary measure for each occupation type. Meanwhile, Schedule P-6 only provides carrier-level total salaries and benefits. Our data limitation thus unfortunately does not allow us to further parse the wage impact by occupation types.

Table 2.5: Employment Impact (DID)

	All Carriers			Majors and LCCs		
	(1) lnEmptotal	(2) lnEmpfull	(3) lnEmppart	(4) lnEmptotal	(5) lnEmpfull	(6) lnEmppart
Merger02	-0.156***	-0.144***	-0.262***	-0.051***	-0.053***	0.022
	(0.021)	(0.022)	(0.052)	(0.014)	(0.014)	(0.039)
Merger35	-0.012	-0.002	-0.107**	-0.033**	-0.034**	-0.045
	(0.017)	(0.018)	(0.048)	(0.016)	(0.016)	(0.047)
Capital Material Costs (KMP)	0.507***	0.519***	0.472***	0.500***	0.483***	0.775***
	(0.040)	(0.041)	(0.056)	(0.104)	(0.112)	(0.126)
Fuel Costs (FC)	0.000	0.001	0.048**	0.384***	0.424***	0.067
	(0.008)	(0.008)	(0.019)	(0.094)	(0.103)	(0.090)
Revenue Passenger Miles (RPM)	-0.484***	-0.506***	-0.263***	-0.974***	-1.132***	-0.098
	(0.040)	(0.039)	(0.078)	(0.198)	(0.192)	(0.524)
Average Stage Length (ASL)	-0.441***	-0.383***	-0.882***	-0.499***	-0.427***	-1.601***
	(0.043)	(0.044)	(0.094)	(0.111)	(0.111)	(0.273)
Average Seat Miles (ASM)	0.136***	0.204***	-0.353***	0.079	0.262	-1.407**
	(0.046)	(0.044)	(0.104)	(0.193)	(0.184)	(0.572)
Observations	4,082	4,082	3,876	1,462	1,462	1,459
Number of Carriers	108	108	108	25	25	25
R-squared	0.984	0.983	0.936	0.993	0.994	0.943
Carrier FE	y	y	y	y	y	y
Time FE	y	y	y	y	y	y

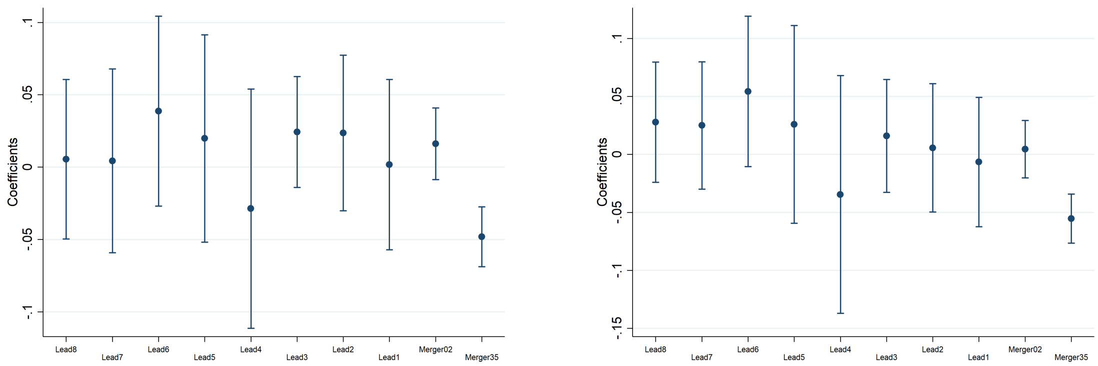
Note: (i) All models are estimated using DID. (ii) The Majors and LCCs subsample is based on six large-scale mergers involving major carriers and LCCs with the control group being non-merging major carriers and LCCs. (iii) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (iv) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (v) Other time-varying carrier characteristics such as *N_enplanements*, *Nroutes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (vi) All models include carrier and year-quarter fixed effects. (vii) Robust standard errors are in parenthesis. (viii) *** p<0.01, ** p<0.05, * p<0.1.

2.4.2 Robustness Checks

We perform a number of sensitivity tests to check the robustness of our baseline findings.[18] We first assess the validity of the parallel trends assumption behind our baseline DID specification by estimating a generalized DID as specified in Equation 2.2. In Figure 2.2, we plot the estimated coefficients on the eight pre-merger quarterly dummies, *Merger02*, and *Merger35* based on the full sample. In addition, we also plot in Figure 2.3 the estimated coefficients on the five post-merger year-specific lag indicators

[18] Our robustness checks will focus on the wage impact specifications since the dependent variables there already account for carriers' quarterly total employment.

Figure 2.2: Pre-Merger Common Trends



(a) Wage Per Worker

(b) Wage Plus Fringe Benefits Per Worker

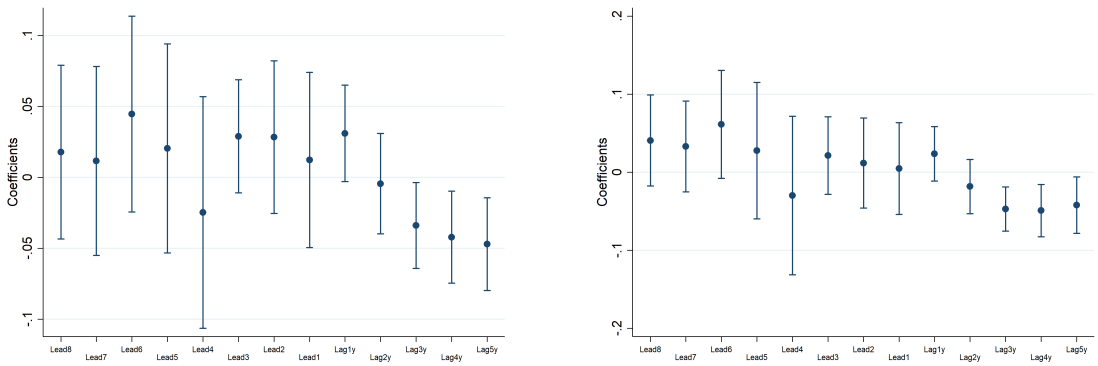
Note: The figure plots the estimated coefficients on the eight pre-merger quarterly dummies, *Merger02*, and *Merger35* in Equation 2 based on the full sample. The dependent variables in Panels (a) and (b) are wage per worker and wage plus fringe benefits per worker, respectively. The presented confidence intervals are at 95% level.

instead of the *Merger02* and *Merger35* dummies.[19] In both figures, we observe that in the eight quarters leading to the merger announcement, there are no statistically significant differences in salary patterns between the merging and non-merging airlines. This implies that the merging and non-merging airlines follow similar pre-merger common trends, which gives suggestive evidence supporting the parallel trends assumption. Moreover, the estimated coefficients on the post-merger yearly dummies also corroborate our baseline findings that the merger impact on wage and benefits tend to exist in the longer-term rather than short term.

Next, despite the suggestive evidence that our DID specifications conform to the parallel trends assumption, one might still be concerned about the pre-treatment characteristics being correlated with the dynamics of the dependent variables and unbalanced between groups. Thus, as a further robustness check on our baseline DID specifications, we also implement an inverse probability of treatment weighting method

[19]The corresponding point estimates are presented in Tables 2.A4 and 2.A5 in the Appendix that also report the estimates based on the subsample of large-scale mergers involving major carriers and LCCs. These estimates are in line with our baseline findings presented in Table 2.4.

Figure 2.3: Pre-Merger Common Trends with Lag Indicators



(a) Wage Per Worker

(b) Wage Plus Fringe Benefits Per Worker

Note: The figure plots the estimated coefficients on the eight pre-merger quarterly dummies as well as five post-merger yearly dummies in Equation 2 based on the full sample. The dependent variables in Panels (a) and (b) are wage per worker and wage plus fringe benefits per worker, respectively. The presented confidence intervals are at 95% level.

using propensity scores. Specifically, we divide our sample into the following four groups: Group 1 – merging carriers before; Group 2 – merging carriers after; Group 3 – non-merging carriers before; and Group 4 – non-merging carriers after. Each group is then weighted using inverse probability of treatment weighting $\frac{1}{propensity\ score_g}$ for group $g = 1$ to 4. To estimate the propensity scores, we fit a multinomial logistic regression predicting the selection into a given group as a function of the set of covariates that we include in our DID specifications. It is important to only balance the characteristics that are not likely affected by the program of interest in order to avoid post-treatment biases (Rosenbaum, 1984). While some of our variables may be affected after the mergers, especially those related to costs and output proxies (e.g., variables such as *KMP*, *FC*, *RPM*, *ASM*, *N_enplanements*), we seek to balance variables that are less likely to be affected after the mergers (e.g., variables such as *Networksize*, *N_hub*, *Distance* as well as major airline and LCC status). Balancing and overlap tests are performed using standardized mean differences as presented in Table 2.A3 and box plots of propensity scores as shown in Figures 2.A2 and 2.A3 in the Appendix, respectively. Overall, these tests point to strong evidence toward the reduction in biases after matching by propensity scores.[20] It thus appears reasonable to consider the covariates' distributions balanced between the different matched groups. Table 2.6 presents the estimates of our baseline wage specifications upon applying IPTW, and they again confirm the long-term reductions in post-merger salary and fringe benefits with the estimated magnitudes of the impact smaller than those in Table 2.4.

Additionally, our baseline findings are based on merger announcement dates rather than completion dates because it typically takes years for airlines to fully integrate and the merger completion dates tend to be less reliably documented. However, in order to ensure that our findings are not affected by the choice of merger dates, we redefine the merger dummies, *Merger02* and *Merger35*, based on merger completion dates and re-estimate Equation 2.1. The results, as presented in Table 2.7, indicate that the short-term wage effect based on merger completion dates in fact captures much of the longer-term effect in our baseline findings based on merger announcement dates, which in turn corroborates the robustness of our baseline findings.[21]

Since the acquiring airlines may strategically target underperforming airlines as merger targets, we also conduct a robustness check by focusing on a subsample of mergers that do not involve bankrupt airlines. Specifically, we exclude four bankruptcy related mergers, including American (AA)-Trans World (TW), US Airways (US)-American West (HP), Southwest (SW)-ATA (TZ), and American (AA)-US Airways (US) as outlined in Table 2.1. We then estimate the same set of specifications as those in Table 2.4 and present the estimates in Table 2.8.[22] We conclude that the results

[20] With no established standards for determining substantial overlap of propensity scores, we use a combination of balancing and overlap tests to assess whether the groups are similar enough to support causal inference, e.g., standardized mean differences of less than 0.2, 0.4 and 0.6 are considered small, moderate, and large, respectively (Cohen, 2013).

[21] We also perform a corresponding check on the pre-merger common trends, similar to that presented in Figure 2.2, and we again find no statistically significant differences in salary patterns between merging and non-merging airlines in the quarters leading to the merger completion.

[22] In addition, we also re-estimate our wage specifications for a subsample that exclude any bankrupt

Table 2.6: Wage Impact (IPTW)

	(1) lnWage	(2) lnWageBene
Merger02	0.012	0.006
	(0.016)	(0.016)
Merger35	-0.029**	-0.035***
	(0.012)	(0.013)
Capital Material Costs (KMP)	0.067***	0.074***
	(0.025)	(0.024)
Fuel Costs (FC)	0.008*	0.002
	(0.004)	(0.004)
Revenue Passenger Miles (RPM)	-0.120**	-0.142***
	(0.054)	(0.050)
Average Stage Length (ASL)	0.157***	0.067*
	(0.043)	(0.041)
Average Seat Miles (ASM)	0.166***	0.099**
	(0.053)	(0.047)
Observations	4,082	4,082
Number of Carriers	108	108
R-squared	0.759	0.800
Carrier FE	y	y
Time FE	y	y

Note: (i) The sample is divided into 4 groups (Group 1 – merging carrier before; Group 2 – merging carrier after; Group 3 – non-merging carrier before; Group 4 – non-merging carrier after) with each weighted using inverse probability of treatment weighting $\frac{1}{propensity\ score_g}$ for each group $g = 1$ to 4. To estimate the propensity scores, we fit a multinomial logistic regression predicting the selection into a given group as a function of the set of covariates used in our DID specifications. Balancing and overlap test are performed using standardized mean differences in Table 2.A3 and box plots of propensity scores in Figures 2.A2 and 2.A3 in the Appendix, respectively. (ii) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (iii) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (iv) Other time-varying carrier characteristics *N_enplanements*, *Nroutes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (v) All models include carrier and year-quarter fixed effects. (vi) Robust standard errors are in parenthesis. (vii) *** p<0.01, ** p<0.05, * p<0.1.

continue to be in line with our baseline findings after excluding the merger episodes involving bankrupt airlines, which alleviates the concern regarding the acquiring airlines' strategically choosing underperforming merger targets.

Lastly, carriers that are in financial distress but not yet in bankruptcy could also

carrier from the entire sample period. The results, as presented in Table 2.A6, are qualitatively similar to those in Table 2.8.

Table 2.7: Wage Impact (DID) Based on Merger Completion

	All Carriers		Majors and LCCs	
	(1) lnWage	(2) lnWageBene	(3) lnWage	(4) lnWageBene
Merger02	-0.036**	-0.051***	-0.118***	-0.117***
	(0.014)	(0.014)	(0.017)	(0.016)
Merger35	-0.035***	-0.033***	-0.111***	-0.103***
	(0.012)	(0.012)	(0.014)	(0.014)
Capital Material Costs (KMP)	0.104**	0.107**	0.020	0.001
	(0.045)	(0.045)	(0.050)	(0.058)
Fuel Costs (FC)	0.003	0.005	0.073	0.072
	(0.005)	(0.005)	(0.045)	(0.053)
Revenue Passenger Miles (RPM)	-0.092***	-0.074***	0.063	0.165
	(0.026)	(0.027)	(0.122)	(0.120)
Average Stage Length (ASL)	0.016	-0.061	0.245***	0.191**
	(0.045)	(0.045)	(0.076)	(0.077)
Average Seat Miles (ASM)	0.131***	0.045	0.130	-0.041
	(0.032)	(0.035)	(0.121)	(0.122)
Observations	4,082	4,082	1,462	1,462
Number of Carriers	108	108	25	25
R-squared	0.695	0.746	0.812	0.857
Carrier FE	y	y	y	y
Time FE	y	y	y	y

Note: (i) All models are estimated using DID. (ii) The Majors and LCCs subsample is based on six large-scale mergers involving major carriers and LCCs with the control group being non-merging major carriers and LCCs. (iii) *Merger02* is a dummy variable indicating whether the carrier merged during the two years (eight quarters) after the completion of merger. Similarly, *Merger35* indicates whether the carrier merged within the three to five years (9-20 quarters) after the completion of merger. (iv) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (v) Other time-varying carrier characteristics such as *N_enplanements*, *Nroutes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (vi) All models include carrier and year-quarter fixed effects. (vii) Robust standard errors are in parenthesis. (viii) *** p<0.01, ** p<0.05, * p<0.1.

be likely candidates to merge with carriers that are in a better financial position. Following Ma (2019) that shows that capital structure can affect airlines' decisions when responding to Southwest Airlines entry threat and actual entry, we augment our main wage specification by including airlines' debt-to-asset ratio as a proxy for their financial leverage.[23] As a continuous variable, the debt-to-asset ratio would potentially

[23]The debt-to-asset ratio is defined as total debts divided by total assets. We derive the variable from Schedule B-1 of the Air Carrier Financial Reports from the BTS that contains quarterly operating

Table 2.8: Wage Impact (DID) Excluding Mergers Involving Bankrupt Carriers

	All Carriers		Majors and LCCs	
	(1)	(2)	(3)	(4)
	lnWage	lnWageBene	lnWage	lnWageBene
Merger02	0.010	0.027*	0.021	0.039*
	(0.016)	(0.016)	(0.022)	(0.022)
Merger35	-0.061***	-0.039**	-0.100***	-0.062***
	(0.016)	(0.016)	(0.020)	(0.018)
Capital Material Costs (KMP)	0.107**	0.108**	0.015	-0.008
	(0.046)	(0.046)	(0.052)	(0.061)
Fuel Costs (FC)	0.002	0.005	0.071	0.071
	(0.005)	(0.005)	(0.047)	(0.055)
Revenue Passenger Miles (RPM)	-0.094***	-0.076***	0.076	0.213*
	(0.027)	(0.027)	(0.127)	(0.123)
Average Stage Length (ASL)	0.009	-0.066	0.250***	0.195**
	(0.046)	(0.046)	(0.078)	(0.078)
Average Seat Miles (ASM)	0.131***	0.045	0.128	-0.077
	(0.032)	(0.035)	(0.127)	(0.127)
Observations	3,933	3,933	1,378	1,378
Number of Carriers	108	108	25	25
R-squared	0.694	0.745	0.811	0.861
Carrier FE	y	y	y	y
Time FE	y	y	y	y

Note: (i) All models are estimated using DID. (ii) Four bankruptcy related mergers, including AA-TW, US-HP, SW-TZ, and AA-US as outlined in Table 2.1, are excluded in this analysis. (iii) The Majors and LCCs subsample is based on the three mergers involving non-bankrupt major carriers and LCCs with the control group being non-merging major carriers and LCCs. (iv) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (v) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (vi) Other time-varying carrier characteristics *N_enplanements*, *N_routes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (vii) All models include carrier and year-quarter fixed effects. (viii) The numbers of carriers remain the same since we only only exclude merging carriers that became bankrupt during the post-merger period in this analysis. (ix) Robust standard errors are in parenthesis. (x) *** p<0.01, ** p<0.05, * p<0.1.

provide a more accurate depiction of the airline's financial condition than a dummy variable on the carrier's bankruptcy status. In addition, we also include a dummy variable that indicates whether the workforce of a major airline (or LCC) is significantly unionized.[24] Due to the lack of reliable data sources, the unionization dummy does

balance sheet statements for large certificated U.S. air carriers including items as cash, short-term investments, accounts receivable, long-term debt, accounts payable, and salaries and wages.

[24]The unionization dummy is constructed utilizing various data sources including the Association

not include regional carriers. Table 2.9 presents the results from our augmented wage specification that includes the debt-to-asset ratio variable and the unionization dummy. We again observe that the estimates on the post-merger dummies are consistent with our baseline findings in Table 2.4.

2.5 Discussion and Conclusions

In line with the impact of merger-induced monopsony power discussed in Marinescu and Hovenkamp (2019) and Naidu et al. (2018), our findings point to a consistently negative labor market impact of airline mergers with a significant reduction in merging airlines' short-term employment level and long-term per capita salaries and fringe benefits following the mergers, particularly among large-scale mergers involving major airlines and low cost carriers. The short-term employment impact could reflect adjustment costs due to the merger, while the long-term post-merger wage suppression is consistent with the inverse relation between labor market concentration and wage levels that Benmelech et al. (2018) find to become more pronounced over time.[25] Since the within-airline seniority ranks help impose barriers to cross-airline mobility (Hirsch, 2007), which can effectively reduce the elasticity of labor supply, our findings further imply that while the seniority integration process can hinder the efficient integration of the merging airlines, it may potentially reinforce the integrated airlines' monopsony power and suppress salaries and benefits over time despite the heavy union presence in the industry. Although data limitations prevent us from directly testing this potential

of Flight Attendants (AFA) website, Communications Workers of America (CWA) website, carriers' official websites, Form 10-K from the Securities and Exchange Commission, and extensive news article searches.

[25]It is worth noting that mergers could entail less competition for labor and thus strengthened monoposony power even for the non-merging carriers. This could bias our estimates toward zero, which implies that if anything, our estimates provide a lower bound of the negative wage and employment impact of airline mergers.

Table 2.9: Wage Impact (DID) with Unionization and Debt-to-Asset Ratio Controls

	All Carriers		Majors and LCCs	
	(1) lnWage	(2) lnWageBene	(3) lnWage	(4) lnWageBene
Merger02	0.013	0.001	0.008	-0.012
	(0.012)	(0.012)	(0.015)	(0.016)
Merger35	-0.051***	-0.058***	-0.091***	-0.095***
	(0.010)	(0.011)	(0.014)	(0.014)
Capital Material Costs (KMP)	0.058***	0.060***	0.008	-0.016
	(0.020)	(0.020)	(0.051)	(0.059)
Fuel Costs (FC)	0.004	0.006	0.075	0.080
	(0.005)	(0.005)	(0.047)	(0.054)
Revenue Passenger Miles (RPM)	-0.084**	-0.080**	0.078	0.187
	(0.034)	(0.037)	(0.122)	(0.121)
Average Stage Length (ASL)	0.057*	-0.020	0.198**	0.180**
	(0.031)	(0.031)	(0.080)	(0.081)
Average Seat Miles (ASM)	0.141***	0.071	0.069	-0.078
	(0.038)	(0.043)	(0.121)	(0.123)
Unionization	0.062***	0.038*	0.106***	0.053**
	(0.022)	(0.022)	(0.021)	(0.021)
Debt-to-Asset	-0.002	-0.003	0.013**	0.017*
	(0.011)	(0.012)	(0.006)	(0.009)
Observations	3,996	3,996	1,461	1,461
Number of Carriers	107	107	25	25
R-squared	0.718	0.761	0.812	0.856
Carrier FE	y	y	y	y
Time FE	y	y	y	y

Note: (i) All models are estimated using DID. (ii) The Majors and LCCs subsample is based on six large-scale mergers involving major carriers and LCCs with the control group being non-merging major carriers and LCCs. (iii) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (iv) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (v) Other time-varying carrier characteristics such as *N_enplanements*, *Nroutes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (vi) All models include carrier and year-quarter fixed effects. (vii) Robust standard errors are in parenthesis. (viii) *** p<0.01, ** p<0.05, * p<0.1.

mechanism, our finding regarding the differential short-term and long-term wage effects lends support toward the explanation.

Naidu et al. (2018) point out that, while the Department of Justice and the Federal Trade Commission's Horizontal Merger Guidelines provide specific analytical frameworks for evaluating the product market consequences of mergers, they do not offer guidance on how to weigh in on the potential adverse labor market impact of mergers. Our findings confirm the negative labor market impact of mergers in the context of the airline industry and thus offer important policy implications toward the ongoing discussions regarding how to account for employer monopsony power during mergers and acquisitions. For instance, the short-term downsizing, potentially centered around flight attendants and ground crew, and long-term expansion in managerial positions, coupled with an overall long-term post-merger wage suppression, may impose considerable equity and welfare concerns.

While our paper aims to provide first evidence of the labor market impact of airline mergers, its scope is admittedly curbed by our data limitations, which then paves the way for future research. For example, the labor market outcome data in our study are at airline level rather than airline-occupation level. This prevents us from exploring more detailed occupation-specific wage impact of mergers, e.g., wage impact may depend on the composition of airlines' workforce, or certain types of airline workers could be more likely to be subject to the exercise of airline market power than others. Administrative data on airline employment that capture geographical categorization of the relevant labor market as well as information on employee seniority ranks would also help one exploit local variations in employer monopsony power and further parse the role of employer monopsony versus seniority integration in explaining the observed labor market impact of airline mergers. Lastly, one could also follow studies such as Ma et al. (2020) that perform comparative analysis of airline mergers involving overlapping versus complementary network structures and study their potentially differential labor market impact.

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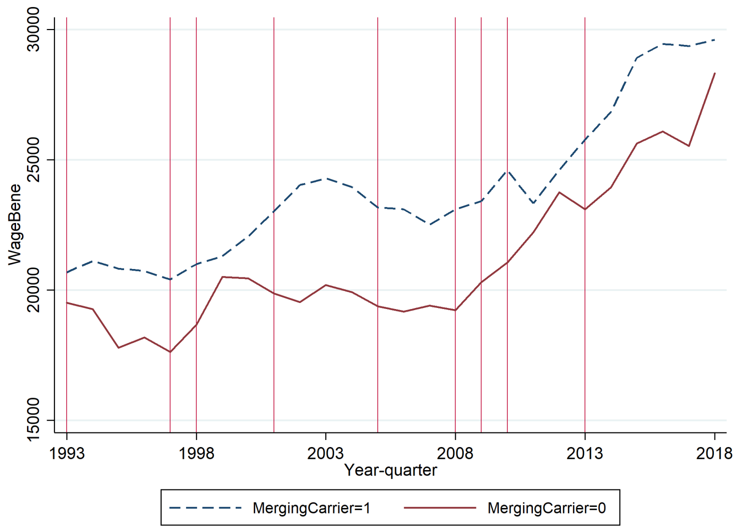
Appendix: Additional Figures and Tables

Figure 2.A1: Merging and Non-Merging Carrier's Quarterly Wage Plus Benefit Time Trend

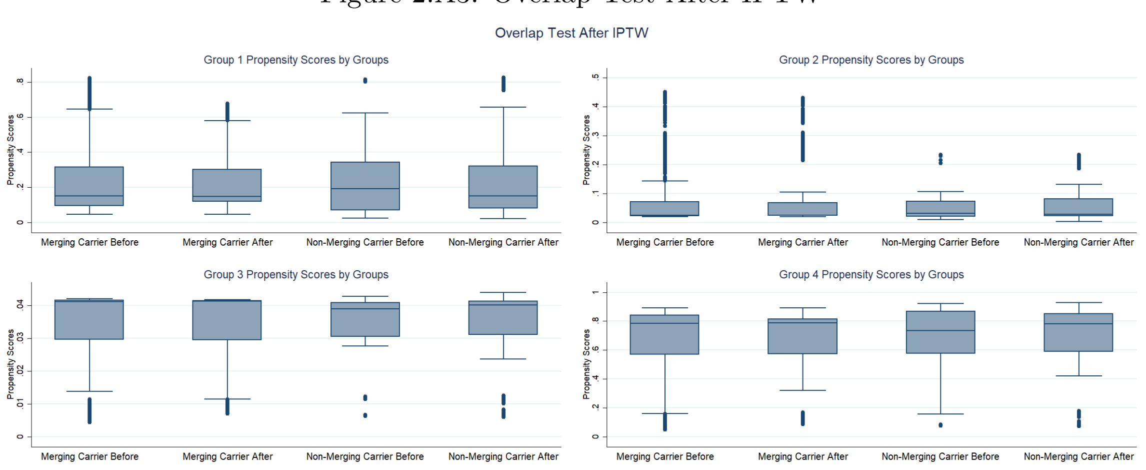


Note: Vertical lines indicate merger announcement years. Merging carriers are those that were involved in mergers at any point during our sample period. Non-merging carriers are those that were never involved into any merger.

Figure 2.A2: Overlap Test Before IPTW

Note: (i) The figure presents the overlap test of the distributions of propensity scores for each of the four groups prior to applying the IPTW. The box marks the first and third quartiles of the propensity scores with a line drawn at the median.

Figure 2.A3: Overlap Test After IPTW



(i) The figure presents the overlap test of the distributions of propensity scores for each of the four groups after applying the IPTW. The box marks the first and third quartiles of the propensity scores with a line drawn at the median.

Table 2.A1: Summary Statistics for Majors and LCCs Subsample

	Mean	S.D	Min	Max
Wage	16,935	4,208	3,300	56,273
Wage+Benefits	23,354	6,339	3,552	70,266
Total Employment	28,666	30,148	67.30	108,767
Merger02	0.061	0.239	0	1
Merger35	0.052	0.222	0	1
Capital Material Costs (KMP)	45,435	343,144	2,960	7,083,547
Fuel Costs (FC)	234	481	13	16,489
Revenue Passenger Miles (RPM)	3,754,994	2,400,004	65,926	20,083,750
Average Stage Length (ASL)	41.40	86.20	3.73	1,389
Average Seat Miles (ASM)	4,980,549	2,971,033	118,234	23,272,188
Debt-to-Asset	0.899	0.592	-0.769	10.70
Unionization	0.404	0.491	0	1
N_enplanements	8,994,261	8,857,388	662	42,872,537
N_routes	469	398	1	1,896
Load Factor	0.704	0.097	0.186	0.945
Network Size	0.277	0.121	0.024	0.598
Bankruptcy	0.007	0.083	0	1
N_hub	201	256	0	1,055
Distance	902	252	348	2,258

Note: (i) The Majors and LCCs subsample is based on six large-scale mergers involving major carriers and LCCs with the control group being non-merging major carriers and LCCs. (ii) Observations are at carrier-year-quarter level, and sample size is 1,462 for all variables. (iii) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (iv) Carrier-level time-varying controls are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (v) All nominal variables are converted into real values in 2018 prices using the CPI from the BLS.

Table 2.A2: Employment Subcategories (DID, Full Sample)

	(1)	(2)	(3)	(4)	(5)	(6)
	lnGeneralManage	lnPilotsCopilots	lnFlightAttendents	lnMaintenance	lnTraffic	lnOthersEmp
Merger02	0.097	-0.066***	-0.165***	-0.211***	-0.249***	-0.204***
	(0.084)	(0.025)	(0.058)	(0.050)	(0.047)	(0.054)
Merger35	0.442***	0.020	0.010	0.027	-0.146***	-0.016
	(0.073)	(0.021)	(0.036)	(0.034)	(0.048)	(0.044)
Capital Material Costs (KMP)	0.214***	0.567***	0.514***	0.581***	0.525***	0.470***
	(0.053)	(0.052)	(0.060)	(0.055)	(0.059)	(0.058)
Fuel Costs (FC)	0.092***	-0.015	-0.021	-0.035***	0.057***	-0.070***
	(0.019)	(0.010)	(0.013)	(0.013)	(0.018)	(0.015)
Revenue Passenger Miles (RPM)	-0.494***	-0.681***	-0.268***	-0.517***	-0.591***	-0.269***
	(0.099)	(0.057)	(0.102)	(0.072)	(0.077)	(0.096)
Average Stage Length (ASL)	-0.438***	-0.513***	-0.568***	-0.384***	-0.695***	-0.572***
	(0.094)	(0.071)	(0.102)	(0.067)	(0.083)	(0.091)
Average Seat Miles (ASM)	0.190*	0.218***	-0.003	0.079	-0.052	-0.074
	(0.104)	(0.064)	(0.131)	(0.078)	(0.092)	(0.112)
Observations	3,494	3,719	3,518	3,737	3,694	3,679
Number of Carriers	108	108	108	108	108	108
R-squared	0.803	0.951	0.908	0.935	0.955	0.928
Carrier FE	y	y	y	y	y	y
Time FE	y	y	y	y	y	y

Note: (i) All models are estimated using DID. (ii) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (iii) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (iv) Other time-varying carrier characteristics *N_enplanements*, *N_routes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (v) All models include carrier and year-quarter fixed effects. (vi) Robust standard errors are in parenthesis. (vii) *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A3: Balancing Test Before and After IPTW

	Mean				Unweighted Standardized Mean Differences						Weighted Standardized Mean Differences					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Group1	Group2	Group3	Group4	1 vs. 2	1 vs. 3	1 vs. 4	2 vs. 3	2 vs. 4	3 vs. 4	1 vs. 2	1 vs. 3	1 vs. 4	2 vs. 3	2 vs. 4	3 vs. 4
Networksize	0.33	0.29	0.25	0.24	0.368	0.528	0.657	0.321	0.438	0.060	-0.026	0.061	0.131	0.081	0.157	0.049
lnN_hub	2.54	3.57	0.26	0.18	-0.340	0.997	1.056	1.442	1.511	0.081	0.056	0.037	0.049	-0.037	-0.026	0.024
lnDistance	6.53	6.62	6.47	6.49	-0.249	0.084	0.064	0.230	0.231	-0.023	0.040	-0.108	-0.105	-0.136	-0.137	0.013
Major	0.45	0.46	0.05	0.04	-0.020	1.041	1.107	1.065	1.131	0.074	-0.024	-0.047	-0.044	-0.015	-0.011	0.008
LCC	0.24	0.32	0.16	0.16	-0.188	0.189	0.195	0.378	0.384	0.006	0.042	0.034	0.038	-0.013	-0.009	0.004
Observations	983	296	136	2,669												

Note: (i) Standardized mean difference is defined as the difference in means divided by the standard deviation.

Table 2.A4: Pre-Merger Common Trends

	All Carriers				Majors and LCCs			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	lnWage	lnWageBene	lnWage	lnWageBene	lnWage	lnWageBene	lnWage	lnWageBene
MergingCarrier	-0.047**	-0.032			0.142***	0.094***		
	(0.020)	(0.020)			(0.034)	(0.035)		
TimeTrend	0.004***	0.004***			0.004***	0.004***		
	(0.000)	(0.000)			(0.001)	(0.001)		
MergingCarrierTimeTrend	0.000	0.000			-0.002***	-0.001***		
	(0.000)	(0.000)			(0.000)	(0.000)		
Lead8			0.016	0.039			-0.004	0.018
			(0.031)	(0.030)			(0.040)	(0.035)
Lead7			0.010	0.031			-0.024	-0.002
			(0.034)	(0.030)			(0.037)	(0.032)
Lead6			0.044	0.060*			-0.010	0.012
			(0.035)	(0.035)			(0.037)	(0.037)
Lead5			0.020	0.027			-0.031	-0.032
			(0.037)	(0.044)			(0.034)	(0.036)
Lead4			-0.026	-0.032			-0.009	-0.019
			(0.042)	(0.052)			(0.029)	(0.030)
Lead3			0.027	0.019			-0.000	-0.011
			(0.020)	(0.025)			(0.020)	(0.022)
Lead2			0.027	0.010			-0.010	-0.013
			(0.028)	(0.030)			(0.031)	(0.030)
Lead1			0.011	0.003			-0.004	-0.006
			(0.032)	(0.030)			(0.037)	(0.035)
Merger02			0.019	0.009			-0.004	-0.018
			(0.013)	(0.013)			(0.016)	(0.016)
Merger35			-0.047***	-0.054***			-0.087***	-0.095***
			(0.010)	(0.011)			(0.014)	(0.014)
Capital Material Costs (KMP)	0.127***	0.147***	0.104**	0.107**	-0.003	0.008	0.019	0.000
	(0.011)	(0.011)	(0.045)	(0.045)	(0.026)	(0.028)	(0.051)	(0.059)
Fuel Costs (FC)	0.007*	0.007*	0.004	0.005	0.042**	0.041**	0.071	0.070
	(0.004)	(0.004)	(0.005)	(0.005)	(0.019)	(0.020)	(0.046)	(0.054)
Revenue Passenger Miles (RPM)	0.027	-0.003	-0.093***	-0.075***	0.183	0.342***	0.076	0.185
	(0.034)	(0.034)	(0.026)	(0.027)	(0.125)	(0.125)	(0.125)	(0.122)
Average Stage Length (ASL)	-0.077***	-0.065***	0.016	-0.060	0.369***	0.291***	0.237***	0.182**
	(0.019)	(0.017)	(0.045)	(0.045)	(0.070)	(0.075)	(0.076)	(0.077)
Average Seat Miles (ASM)	-0.101***	-0.031	0.131***	0.045	0.123	-0.121	0.109	-0.066
	(0.036)	(0.037)	(0.032)	(0.035)	(0.139)	(0.141)	(0.123)	(0.124)
Observations	4,084	4,084	4,082	4,082	1,462	1,462	1,462	1,462
Number of Carriers	108	108	108	108	25	25	25	25
R-squared	0.395	0.460	0.696	0.746	0.677	0.741	0.808	0.855
Carrier FE	n	n	y	y	n	n	y	y
Time FE	n	n	y	y	n	n	y	y

Note: (i) We apply extended DID (Columns (1), (2), (5), and (6)) and generalized DID models (Columns (3), (4), (7), and (8)) to check pre-merger common trends. (ii) The Majors and LCCs subsample is based on six large-scale mergers involving major carriers and LCCs with the control group being non-merging major carriers and LCCs. (iii) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (iv) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (v) Other time-varying carrier characteristics *N_enplanements*, *Nroutes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (vi) Generalized DID includes carrier and year-quarter fixed effects. (vii) Robust standard errors are in parenthesis. (viii) *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.A5: Pre-Merger Common Trends with Lag Terms

	All Carriers		Majors and LCCs	
	(1)	(2)	(3)	(4)
	lnWage	lnWageBene	lnWage	lnWageBene
Lead8	0.018	0.041	-0.001	0.021
	(0.031)	(0.030)	(0.040)	(0.035)
Lead7	0.012	0.033	-0.022	0.001
	(0.034)	(0.030)	(0.037)	(0.032)
Lead6	0.045	0.061*	-0.009	0.014
	(0.035)	(0.035)	(0.037)	(0.037)
Lead5	0.020	0.028	-0.032	-0.031
	(0.038)	(0.045)	(0.034)	(0.037)
Lead4	-0.025	-0.030	-0.008	-0.017
	(0.042)	(0.052)	(0.029)	(0.030)
Lead3	0.029	0.021	0.003	-0.006
	(0.020)	(0.025)	(0.021)	(0.023)
Lead2	0.028	0.012	-0.006	-0.008
	(0.027)	(0.029)	(0.030)	(0.030)
Lead1	0.012	0.005	0.000	-0.002
	(0.032)	(0.030)	(0.036)	(0.034)
Lag1y	0.031*	0.024	0.008	-0.008
	(0.017)	(0.018)	(0.019)	(0.021)
Lag2y	-0.005	-0.018	-0.021	-0.032
	(0.018)	(0.018)	(0.022)	(0.022)
Lag3y	-0.034**	-0.047***	-0.075***	-0.087***
	(0.015)	(0.014)	(0.021)	(0.018)
Lag4y	-0.042**	-0.049***	-0.097***	-0.099***
	(0.017)	(0.017)	(0.018)	(0.018)
Lag5y	-0.047***	-0.042**	-0.085***	-0.084***
	(0.017)	(0.018)	(0.022)	(0.026)
Capital Material Costs (KMP)	0.104**	0.107**	0.018	-0.001
	(0.045)	(0.045)	(0.050)	(0.059)
Fuel Costs (FC)	0.004	0.005	0.072	0.071
	(0.005)	(0.005)	(0.046)	(0.054)
Revenue Passenger Miles (RPM)	-0.092***	-0.074***	0.079	0.187
	(0.026)	(0.027)	(0.125)	(0.122)
Average Stage Length (ASL)	0.016	-0.061	0.234***	0.178**
	(0.045)	(0.045)	(0.077)	(0.077)
Average Seat Miles (ASM)	0.130***	0.045	0.102	-0.073
	(0.032)	(0.035)	(0.123)	(0.124)
Observations	4,082	4,082	1,462	1,462
Number of Carriers	108	108	25	25
R-squared	0.696	0.746	0.807	0.854
Carrier FE	y	y	y	y
Time FE	y	y	y	y

Note: (i) We apply generalized DID models to check pre-merger common trends. (ii) The Majors and LCCs subsample is based on six large-scale mergers involving major carriers and LCCs with the control group being non-merging major carriers and LCCs. (iii) The *Lag* variables are time dummies indicating j years ($1 \leq j \leq 5$) post the merger announcement. (iv) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (v) Other time-varying carrier characteristics *N_enplanements*, *Nroutes*, *Loadfactor*, *Networksize*, *Bankruptcy*, *N_hub*, and *Distance* are controlled in all columns. (vi) Generalized DID includes carrier and year-quarter fixed effects. (vii) Robust standard errors are in parenthesis. (viii) *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A6: Wage Impact (DID) Excluding All Bankrupt Carriers

	All Carriers		Majors and LCCs	
	(1)	(2)	(3)	(4)
	lnWage	lnWageBene	lnWage	lnWageBene
Merger02	-0.027	0.005	-0.032	-0.008
	(0.020)	(0.021)	(0.022)	(0.022)
Merger35	-0.084***	-0.045**	-0.129***	-0.091***
	(0.022)	(0.021)	(0.022)	(0.018)
Capital Material Costs (KMP)	0.116**	0.116**	-0.007	-0.028
	(0.049)	(0.049)	(0.054)	(0.062)
Fuel Costs (FC)	0.000	0.004	0.083*	0.082
	(0.006)	(0.006)	(0.048)	(0.056)
Revenue Passenger Miles (RPM)	-0.087***	-0.065**	0.030	0.154
	(0.027)	(0.027)	(0.133)	(0.129)
Average Stage Length (ASL)	-0.003	-0.073	0.260***	0.210***
	(0.048)	(0.048)	(0.080)	(0.080)
Average Seat Miles (ASM)	0.123***	0.036	0.203	0.020
	(0.033)	(0.035)	(0.132)	(0.132)
Observations	3,440	3,440	1,173	1,173
Number of Carriers	98	98	21	21
R-squared	0.691	0.741	0.820	0.870
Carrier FE	y	y	y	y
Time FE	y	y	y	y

Note: (i) All models are estimated using DID. (ii) Carriers that became bankrupt at any point during our entire sample period are excluded in this analysis. (iii) The Majors and LCCs subsample is based on three mergers involving non-bankrupt major carriers and LCCs with the control group being non-merging and non-bankrupt major carriers and LCCs. (iv) *Merger02* is a dummy variable indicating whether the carrier merged during the previous two years (eight quarters). Similarly, *Merger35* indicates whether the carrier merged within the past three to five years (9-20 quarters). (v) Carrier-level time-varying controls such as *KMP*, *FC*, *RPM*, *ASL*, and *ASM* are in natural logs and are derived from Schedule P-6 and Form T-100 and described in Table 2.2. (vi) Other time-varying carrier characteristics *N_enplanements*, *N_routes*, *Loadfactor*, *Networksize*, *N_hub*, and *Distance* are controlled in all columns. (vii) All models include carrier and year-quarter fixed effects. (viii) The number of carriers are different since we exclude mergers involving bankrupt carriers across the sample periods. Given that ATA (TZ) is missing in our data and that American (AA) is involved in 2 bankruptcy-related mergers with Trans Words (TW) and US Airways (US), respectively, the number of carriers thus decreases by 10 compared to Table 2.8. (ix) Robust standard errors are in parenthesis. (x) *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

Retail Marijuana Deregulation and Housing Prices

3.1 Introduction

The rapid legalization of recreational marijuana has created a new industry in the United States. Despite the quick succession of states passing these legalization measures, there is little evidence of how the local economy responds. Immediately upon passage of legalization laws, states increase revenues with marijuana sales taxes and decrease costs by reducing the burden of marijuana-related arrest and incarcerations. Both of these examples create second-order effects on markets which have yet to be considered. This research contributes to the growing marijuana legalization literature by studying the cross-state effect of recreational marijuana legalization (RML) on the housing market.

Other studies has considered the impact of marijuana legalization on residential home prices, many of which are concerned with the effect of marijuana dispensaries. Thomas and Tian (2020), Conklin et al. (2020), Tyndall (2019), and Burkhardt and

Flyr (2019) all estimate the housing market response to new dispensary openings in nearby neighborhoods. Among these papers, the evidence is decidedly mixed with negative, positive, null, and positive results respectively. Cheng et al. (2018) use the staggered adoption of city-level marijuana regulations within Colorado to estimate a difference-in-differences model, finding a six percent price increase in the housing market. Our key contribution to the literature is the estimation of a cross-state model, which is made possible with a rich national level housing data set from the online real estate database Zillow.com. We also provide new evidence of the effect of dispensaries on nearby home values in both Colorado and Washington.

Twelve states and Washington D.C. have passed initiatives legalizing the use of marijuana for recreational purposes since 2012. Additionally, 33 states and Washington D.C. have passed medical marijuana laws since 1996. This quick shift in policy puts the states at odds with the federal government, which still classifies marijuana as a Schedule 1 narcotic on par with cocaine, heroin, and lysergic acid diethylamide (LSD).[1] The disconnect between the public and the federal government reflects the evolution of the perceived benefits of marijuana. Large majorities of American adults believe that marijuana has medical benefits (Keyhani et al. (2018)), and adolescents have low risk perceptions of the drug (Roditis and Halpern-Felsher (2015)) even though medical professionals are unsure of its efficacy (Kondrad and Reid (2013); Carlini et al. (2017); Fitzcharles et al. (2014); Braun et al. (2018)). Despite the public's beliefs, most states have been reluctant to legalize marijuana for recreational use. Concerns about the potential effect on crime rates and the difficulty in policing impaired driving have been cited as reasons to slow-walk the path to full recreational legalization.

Legalization could increase crime rates, as the drug's effect can make users act more erratically, and easy access to marijuana creates a low-risk trafficking network across

[1] From the Drug Enforcement Agency (DEA), https://www.dea.gov/drug-scheduling

state lines. It is well established that crime and the perception of crime negatively impact home prices (Pope (2008); Buonanno et al. (2013)), so legalization might put downward pressure on the housing markets of states with successful ballot measures. Counter to the crime narrative however, early research suggests that there are no negative effects. Brinkman and Mok-Lamme (2019) find a 19 percent decrease in crime rate in Denver neighborhoods with dispensaries relative to the average crime rate in the sample period. Similarly, Morris et al. (2014) and Huber et al. (2016) find decreases in violent and property crimes following the passage of medical marijuana legalization. There is also evidence that RML increased crime clearance rates by police in Colorado and Washington (Makin et al. (2019)). Research on traffic incidents suggest similar null or negative results in states with legal recreational (Hansen et al. (2020)) and medical marijuana (Bartos et al. (2018)).

An emerging literature studies the impact of medical marijuana legalization on labor market outcomes. Sabia and Nguyen (2018) find no effect on adult wages, employment or hours worked and a small decrease in wages among young men with access to marijuana dispensaries. Nicholas and Maclean (2019) focus on older adults, finding an increase in the labor supply of those over the age of 51 with the largest effect coming for adults with health conditions which qualify them for legal medical marijuana use. If there are positive labor supply effects, then it is possible that the housing market could be impacted directly through in-migration as individuals from non-legalization states seek to enjoy the perceived benefits. Zambiasi and Stillman (2020) use a synthetic control approach to estimate Colorado's in and out-migration following its passage of RML. Their results suggest that Colorado experienced a large positive inflow of migrants as a result of legalization and no change in out-migration.

Immigration inflows have been shown to increase single family home prices in Switzerland (Degen and Fischer (2017)), but decrease in the United Kingdom as wealthy native homeowners leave the newly immigrant-populated neighborhoods (Sá (2015)). Despite these mixed results, the combination of reduced crime rates (and arrests), migrant inflows, and a new source of sales tax revenue could increase demand for housing in states that pass RML. Some of these states have used the new tax revenue specifically for school funding, which is a mechanism through which home prices might increase. There is a long literature on school resources and student outcomes (Card and Krueger (1998); Jackson et al. (2016); Martorell et al. (2016)) and school capital investment's impact on the value of nearby homes (Cellini et al. (2010); Neilson and Zimmerman (2014)). The combined effect of RML – increased revenue for public goods, decreased crime, little or no change in traffic incidents, and positive labor supply and migration inflow effects – lead naturally to the question of the real estate market. This paper contributes to the literature by estimating the cross and within-state impacts of RML on housing.

First we estimate the cross-state impact using Zillow housing data. The Zillow data is at the individual property transaction level. The treatment group consists of home transactions in states which have legalized the recreational use of marijuana and the control group consists of home transactions in states which have have not legalized it. We find consistent positive effects in the RML case of around 8 percent across a number of specifications which include time and location fixed effects ranging from the county level to the ZIP code level. The estimates are most pronounced when we consider the date that the sale of recreational marijuana is made legal, suggesting that housing demand responds primarily once the drug is being sold, not when the law is victorious at the ballot.

We then extend the cross-state analysis by estimating an unconditional quantile regression (UQR) as in Firpo et al. (2009) with city level fixed effects. Using city level fixed effects controls for unobserved local property taxes which have long been recognized to influence the housing market (Oates (1969); Anderson (1986)). Doing so provides additional insight into the forces driving our treatment effect. Due to the large heterogeneity in housing markets across the country, the UQR estimates are more robust against extreme value observations than our fixed effects models and provide a more complete understanding of central tendency and dispersion measures. The results of the UQR show positive effects in the top of the distribution following the success of the ballot measure legalizing recreational marijuana, but no effect in the lower half. The greatest impact occurs once it becomes legal to sell marijuana, with large positive effects across the price distribution, especially in the middle three deciles. Heterogeneous responses to a policy shock have not been well-researched in the housing literature, making the findings here one of our major contributions.

Finally, we estimate a spatial model within Colorado and Washington using the Zillow housing data and dispensary location information from the Marijuana Enforcement Division of the Colorado Department of Revenue and the Washington State Department of Health. Our identification strategy follows that of Dronyk-Trosper (2017), who use the staggered construction of municipal buildings such as fire stations to estimate their impact on home prices. In our application, homes which are within two miles of a dispensary at time t and have a second dispensary open within a half mile of the home at time $t + 1$ increase in value by over 6 percent. The price appreciates the closer to the new dispensary a home is, suggesting that the dispensary itself is a neighborhood amenity which has some positive value among home buyers.

This paper contributes to the existing literature by providing robust evidence that marijuana legalization has beneficial spillover effects at both the state and local levels. Taken together, our three sets of results show that states which pass RML ballot measures benefit relative to other states and that marijuana dispensaries provide a boost to the home values in the immediate vicinity. Marijuana's liberalization provides a novel source of tax revenue which states have used to fund capital expenditures, especially in education and it acts as an amenity via the dispensaries that distribute it. The creation of a new legal market has direct implications for the local economy, as it establishes new dispensary jobs and reduces arrest rates. All of these factors have well-established impacts on housing markets. Indeed our results show that the spillover effects of marijuana legalization on the housing market are both statistically and economically significant.

The paper proceeds as follows. Section 3.2 discusses the history of medical and recreational marijuana legalization in the United States, as well as potential mechanisms through which legalization could impact the housing market. Section 3.3 details three data sources used for estimation and presents summary statistics. Section 3.4 describes the empirical strategy and Section 3.5 presents the impact of marijuana legalization on housing markets.

3.2 Background

3.2.1 Medical and Recreational Marijuana Legalization

Beginning in 1937, the federal government prohibited the use of marijuana for recreational consumption and sale with The Marijuana Tax Act of 1937 (Pub. L. No. 75-238, 50 Stat. 551). The law went into effect on October 1, 1937 and two days later a Mexican-American man named Moses Baca was arrested by Denver police for marijuana possession, the first such arrest in the country.[2] In 1968 Richard Nixon won the U.S. presidency on a platform of law and order, quickly establishing drug abuse as "public enemy number one in the United States." The Controlled Substance Act

[2]For a brief history of the first marijuana arrests, see: https://www.leafly.com/news/politics/drug-war-prisoners-1-2-true-story-moses-sam-two-denver-drifters-became-cannabis-pioneers

(Pub. L. 91-513, 84 Stat. 1236) of 1970 created tiers of illegal drugs indicating the severity of negative health effects and the level of addictiveness. Marijuana is included in the Schedule 1 tier, indicating that its severity is at the highest possible level alongside addictive narcotics such as heroin. In 1973 the federal government established the Drug Enforcement Agency, which was the primary entity responsible for policing drug use in the country.

Some states introduced marijuana decriminalization proposals in response to the federal government's aggressive stance on marijuana, but that effort ultimately fell out of favor and the intensity of the War on Drugs escalated in the 1980s and early 90s (Pacula et al. (2003)). In 1996 California became the first state to legalize recreational marijuana, marking the beginning of the end of punitive escalation that began with the Marijuana Tax Act in 1937 and was amplified through the 70s, 80s, and 90s. Once California passed the Compassionate Use Act in 1996, the floodgates were opened and in the ensuing years states across the country legalized marijuana for medicinal purposes. Table 3.2 shows this progress. As of May 2020, 33 states and Washington DC have or are in the process of legalizing medical marijuana consumption.

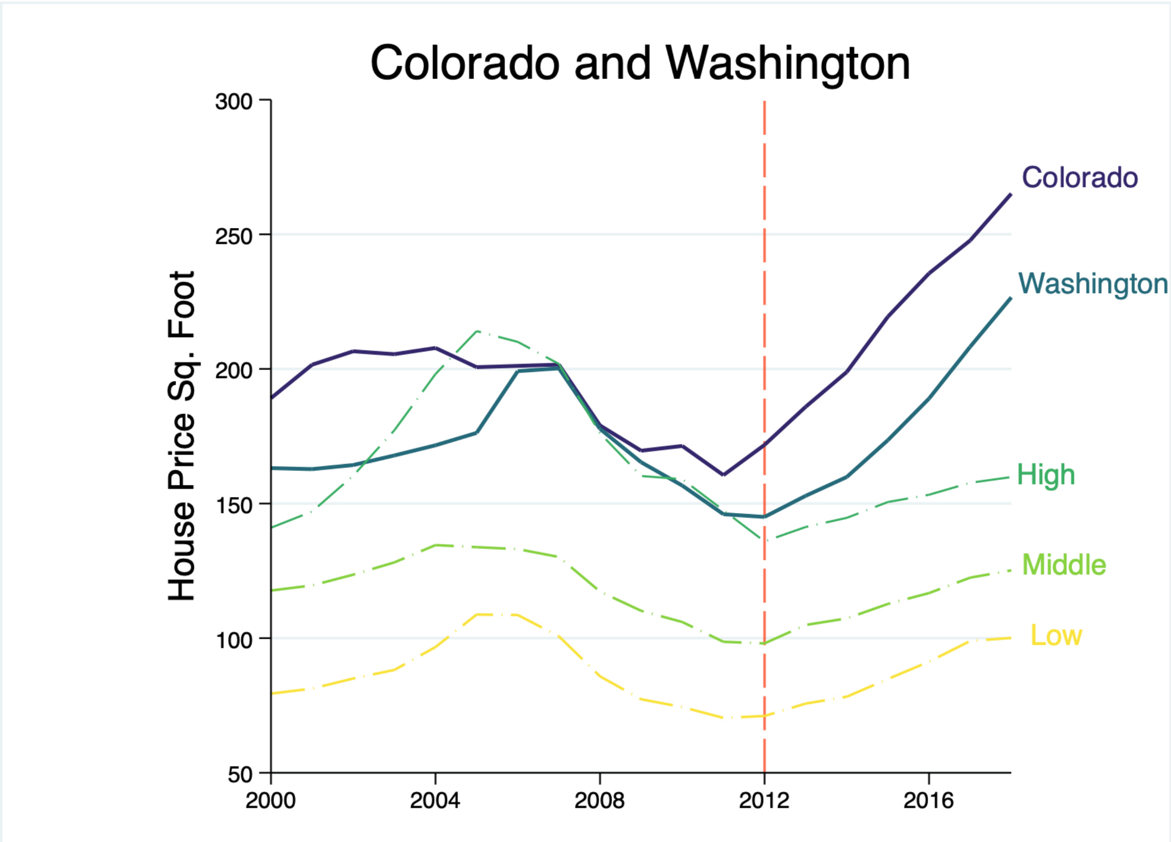
Despite the progress in MML over the last 20 years, it has been a much slower path to full recreational marijuana legalization. Colorado and Washington were the first two states to approve RML on the ballot in 2012, 16 years after California passed its MML law and after 18 other states had done the same. In the years since, Colorado and Washington have been joined by Alaska, California, Maine, Massachusetts, Michigan, Nevada, Oregon, Vermont, and Washington D.C. Some states have had significant lags between their legalization measures passing a vote and the practical implementation of the law. Massachusetts, for example, voted in favor of RML in November 2016 but it was not until November 2018 that dispensaries selling marijuana opened. It is widely expected that this march of progress will continue in the 2020 election cycle and beyond. This paper contributes another data point to the debate over marijuana legalization, demonstrating that those early adopter states have experienced significant appreciations in home values since legalization has been implemented.

3.2.2 The Housing Market Connection

Marijuana legalization comes with a number of trade-offs that make its connection to the housing market ambiguous. The expected direction of legalization's effect depends on a number of forces pushing in opposite directions. Increased public capital expenditures and in-migration would increase demand for housing in the short run and, assuming housing supply is fixed in the short run, raise prices. On the other hand, out-migration, negative health impacts, and increases in crime rates could deflate home values.

To establish the direction of the effect on home prices following marijuana legalization, Figure 3.1, 3.2, and 3.3 show the trend in the national housing market since 2000, divided by when each state adopted RML. There are three cohorts of states. Figure 3.1 includes Colorado and Washington, the first two states to legalize recreational marijuana in 2012. Figure 3.2 includes Oregon, which legalized in 2014, and Figure 3.3 includes California, Massachusetts, and Nevada, all of which legalized recreational marijuana in 2016. The four other states and Washington D.C. which have legalized recreational marijuana are not included because they are outside the sample for reasons discussed in Section 3.1. Solid lines are treatment states across the three figures, and dotted lines reflect states which did not legalize recreational marijuana. To verify that this divergence is a feature of marijuana legalization and not a few wealthy states outpacing the national trend, we divide non-RML states into three groups based on average house price per square foot levels. The six treatment states would all fall into the High average price per square foot grouping with the exception of Nevada, which would be classified in the Middle group if it were not a treatment state. By by comparing the trend in those states to other wealthy and middle income states, we can get a better idea of the impact legalization has had on the housing market.

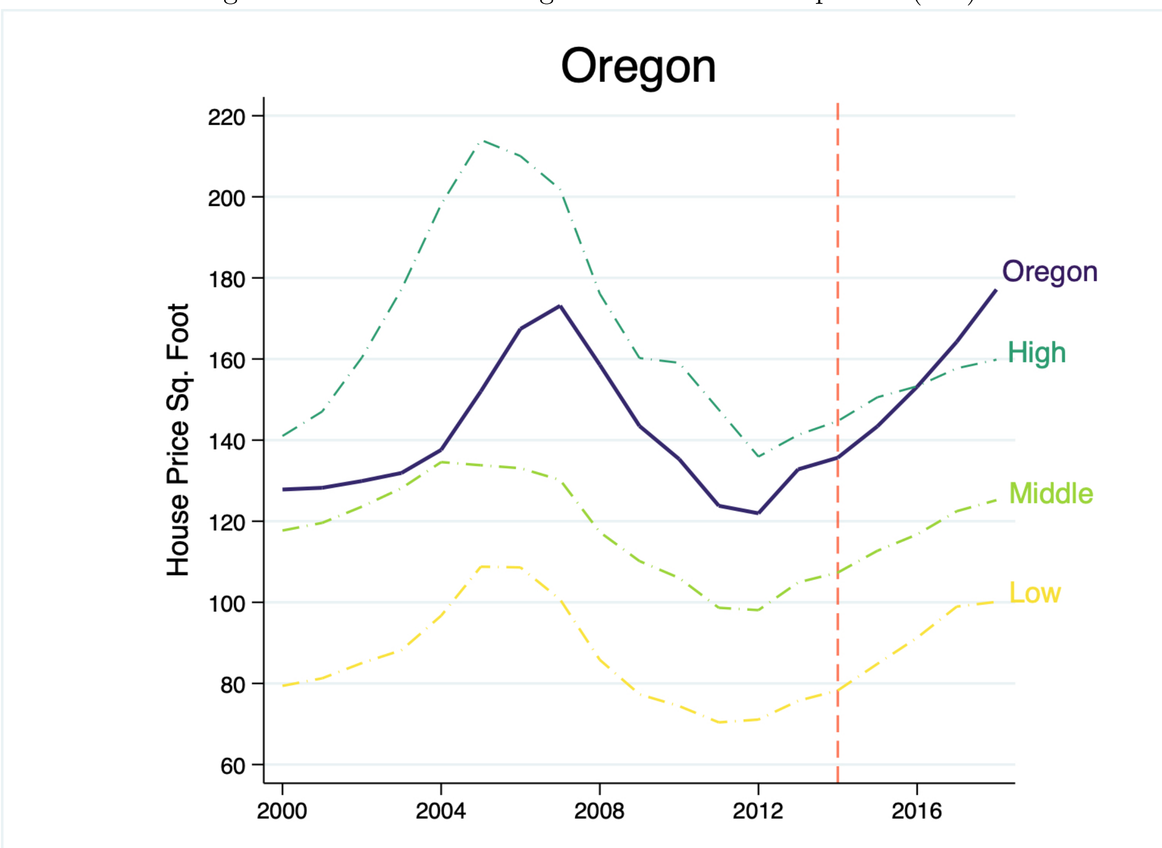
Figure 3.1: Trend of Average House Price Per Sq. Foot (CO, WA)



Note: (i) Control states are divided into three groups – high, middle, and low – based on their average home price per square foot. The low group is composed of Alabama, Florida, New Hampshire, Rhode Island, South Carolina, Tennessee, Texas, and West Virginia. The middle group consists of Georgia, Iowa, Kentucky, Mississippi, Montana, Nebraska, North Carolina, and Pennsylvania. The high group is made of Connecticut, Washington D.C., Delaware, Illinois, Minnesota, New Jersey, Virginia, and Wisconsin. (ii) The vertical line reflects the recreational marijuana legalization date for Colorado and Washington, 2012.

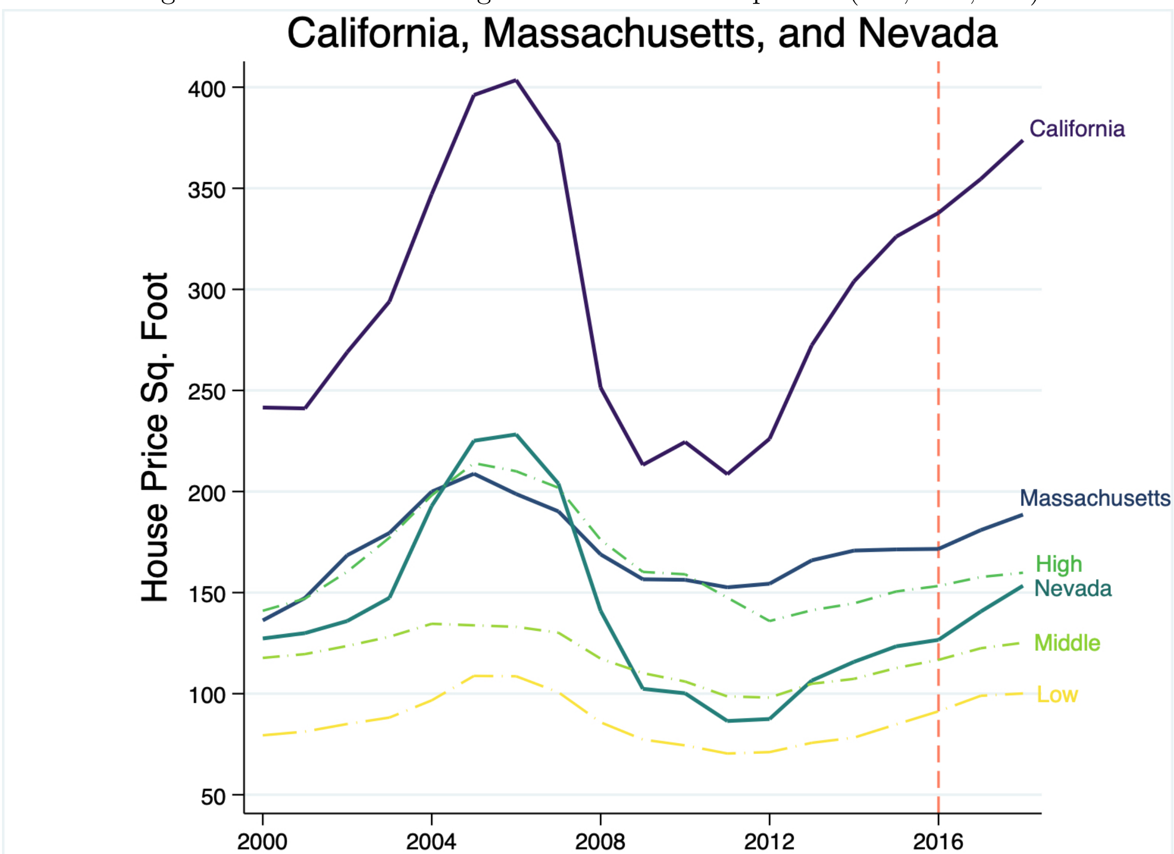
Figure 3.1, 3.2, and 3.3 demonstrate that all three control groups show similar housing market trends since 2000. The RML states meanwhile consistently diverge from the control trends upon their respective cohorts' legalization dates. Across the three graphs, the price trend was similar across RML and non-RML states until 2012.

Figure 3.2: Trend of Average House Price Per Sq. Foot (OR)



Note: The control grouping is the same as in Figure 3.1. The vertical line reflecting the RML treatment date is 2014 for Oregon.

Figure 3.3: Trend of Average House Price Per Sq. Foot (CA, MA, NV)



Note: The control grouping is the same as in Figure 3.1 and 3.2. The vertical line reflecting the RML treatment date is 2016 for California, Massachusetts, and Nevada.

Colorado and Washington display a clear divergence in their housing markets following legalization at the end of 2012. A similar divergence can be seen in Figure 3.2 when Oregon voted in favor of RML in 2014. At the end of the time trend, the 2016 legalization cohort also see distinct jumps in the housing markets relative to the non-RML states.

The housing markets of RML states have recovered faster and stronger than those of non-RML states. The effect in Figure 3.1, 3.2, and 3.3 are all despite the period spanning the Great Recession. Volatility in the housing market can be seen clearly in each figure; the market begins accelerating in 2002, peaks in 2006, and reaches its nadir in 2011. The difference in recovery between RML and non-RML states can be seen most dramatically in the first cohort of Colorado and Washington. This could reflect slack in housing as the market over-corrected during the recession, but there can be no doubt that those two states recovered at a faster rate than their economic peers. It appears that the implementation of RML raised house prices despite the burden of the housing market recovery.

3.2.3 Mechanisms

Having established that states which enacted RML laws received a positive boost during the recovery period following the Great Recession, we now turn our attention to the mechanisms responsible. We consider two possible avenues, which we will refer to as the the "economic development" effect and the "amenity" effect. The economic development effect considers long-run changes to the community which legalization induces; increased tax revenue and spending on public goods that results is an example. The amenity effect captures the role dispensaries have on nearby home values. This reflects the local brick-and-mortar changes that occur due to RML. Our cross-state models estimate the economic development effect and our spatial model estimates the amenity effect.

First consider the economic development effect. The illegal marijuana market prior to legalization is necessarily un-taxed. In the political debate over legalization, supporters often advocate for a mandate that marijuana sales taxes fund public goods investment, including infrastructure improvements and education funding. For example the disposition of Colorado marijuana tax revenue is first distributed to the Public School Capital Construction Assistance Fund, and any revenue over $40 million is transferred to the Public School Fund.[3] There is a long literature on school resources and student outcomes (Card and Krueger (1998); Jackson et al. (2016)). The physical condition of school capital and government investment as a vehicle for student achievement is also of interest in the existing literature (Martorell et al. (2016)). There is further evidence that school capital investment increases the value of local homes. Cellini et al. (2010) use a regression discontinuity design method, exploiting local referenda on bond issuances for capital expenditures to identify the causal effect of referenda passage on the local housing market. Their results suggest a sizable and immediate positive impact on local home values. Neilson and Zimmerman (2014) study the staggered implementation of a school construction project in New Haven, Connecticut, finding that home prices increase in the local neighborhood by approximately 10%. We contribute to this literature by examining whether the passage of recreational marijuana legalization laws – and therefore new sources of tax revenue – affect local home prices.

Another potential mechanism of the economic development effect is migration. By legalizing the use of marijuana, Colorado and other RML states become an attractive option for residents of other states who value the ability to consume marijuana without fear of legal repercussions. Zambiasi and Stillman (2020) find large migration inflows

[3] `https://www.colorado.gov/pacific/revenue/disposition-marijuana-tax-revenue`

following Colorado's passage of RML, supporting this hypothesis. For individuals who migrate to a state with legal recreational marijuana, the cost of moving is less than the consumption cost. Those who use marijuana for medicinal purposes could fall into this category, as easy access to legal marijuana decreases the cost of obtaining and consuming an ameliorative drug.

Assuming that housing supply does not increase in response to the success of RML, in-migration of these individuals could affect local housing markets. The effect of inter-country migration on housing markets is ambiguous in the existing literature (Degen and Fischer (2017); Sá (2015)). However, there is substantial evidence that the number of people migrating within the United States is shrinking and local labor markets conditions and home equity have explain much of the decision to migrate (Henley (1998); Foote (2016); Zabel (2012); Koşar et al. (2019)). Despite this downturn in internal migration, young educated households frequently move to areas with high quality business environments (Chen and Rosenthal (2008)). Recreational marijuana legalization liberalizes the criminal code, but it also creates a new industry in the states that enact it. Business creation increases employment opportunities and growth (Baptista and Preto (2011); Andersson and Noseleit (2011)), which in turn puts upward pressure on housing markets (Liu et al. (2016); Reichert (1990)). Benefits (and potential costs) of industry job creation and demand for marijuana from non-locals could be capitalized into housing values (Cheng et al. (2018)).

We estimate the effect of marijuana legalization at different points of the process (i.e. at the time of the vote to legalize, when the law goes into effect, and when the first dispensaries open), which provides insight into the magnitude of the economic development effect. Since the two-way fixed effects and UQR models define treatment as all homes in a state, the coefficients should reflect the broad treatment inside each state. Homes without nearby dispensaries therefore are likely not experiencing the positive shock through an amenity effect, but through secondary mechanisms such as increased school funding and capital investment. We estimate the UQR model to capture the sensitivity of the price distribution to the economic development effect. The hedonic price function frequently estimated in the housing literature can be highly non-linear. For this reason, the UQR model is our preferred model specification and the primary contribution of this research's estimates of RML on the economic development effect in housing.

The amenity effect will be captured in our Spatial Difference-in-Differences model (see Section 4). By restricting our sample to just homes near dispensaries in Colorado and Washington, we recover the dispensaries' effect on the nearby housing market. This approach is in line with previous research, as prices exhibit localized variation based on a number of amenity factors, including public school quality (Bogart (2000); Cheshire and Sheppard (2004)), public transit options (Bajic (1983); Dewees (1976)), water quality (Epp and Al-Ani (1979); Young and Teti (1984); Leggett and Bockstael (2000)), rail lines (Bowes and Ihlanfeldt (2001); Gibbons and Machin (2005); McMillen and McDonald (2004)), and crime (Hellman and Naroff (1979)). Home prices vary significantly as households are heterogeneous in their amenity preferences and income (Gibbons and Machin (2008)). If dispensaries are an amenity – either positive or negative – then we should be able to recover an effect with the Spatial Difference-in-Differences model. Indeed other research has estimated the dispensary-housing market connection (Thomas and Tian (2020); Conklin et al. (2020); Tyndall (2019); Burkhardt and Flyr (2019)), but either did not use a spatial model as part of their identification strategy or are limited to particular cities which might raise external validity concerns. Recovering the amenity effect of dispensaries in Colorado and Washington using a novel estimation method is the second major contribution of this research.

3.3 Data

This research relies on three primary sources of data. First is a national housing data set from the online real estate database company Zillow (Zillow (2017)). The second is a hand-compiled data set identifying each states' laws regarding the liberalization of marijuana use. Finally, we have yearly data on the construction of marijuana dispensaries in Colorado and Washington.

3.3.1 Housing Data

Zillow is a popular tool used by the public to search for properties available for sale in the United States. The company provides a centralized source of property transactions through its Zillow Transaction and Assessment Dataset (ZTRAX).[4] This dataset compiles multiple listing services (MLS) from all fifty states, Washington D.C., and other U.S. territories to provide a comprehensive resource for real estate transactions.

The information includes not only details of a given housing market transaction, such as the sales price and date, but also information about the house itself. The ZTRAX repository provides access to a large number of home characteristics, such as the number of rooms, square foot area of the property, and any structures on it. Table 3.1 shows the summary statistics for all homes in our sample, as well as annual state-level economic variables, such as GDP. The differences among both the home characteristic and local economic variables suggest that local fixed effects will be an important factor in our model specifications.

We consider all homes in each state, conditional on the data being representative of a state's housing market. This is not the case for every state, as some do not have MLS

[4] Data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX). More information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the authors and do not reflect the position of Zillow Group.

Table 3.1: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Transaction Prices ($)					
House Price	330,342	364,989	10,838	9,999,181	38,145,054
log(House Price)	12.35	0.86	9.29	16.12	38,145,054
Price per Sq. Foot	180	179	1.24	23,088	38,145,054
log(Price per Sq. Foot)	4.90	0.80	0.21	10.05	38,145,054
Property Characteristics					
Bedrooms	3.1	0.9	1.0	7.0	38,145,054
Bathrooms	2.1	0.8	0.25	7.0	38,145,054
Sq. Feet	1,948	1,065	420	10,228	38,145,054
log(Sq. Feet)	7.6	0.4	6.0	9.2	38,145,054
Year Built	1976	29	0.00	2018	38,145,054
State Characteristics					
GDP (Millions $)	787,941	706,135	36,281	2,968,117	38,145,054
Population	15,535,151	12,272,350	567,136	39,557,045	38,145,054
Land (Acres)	77,264	50,066	61	261,797	38,145,054
Density	2.83	6.04	0.19	114.41	38,145,054
log(GDP)	13.18	0.92	10.50	14.90	38,145,054
log(Population)	16.21	0.89	13.25	17.49	38,145,054
log(Land)	10.96	0.96	4.12	12.48	38,145,054
Treatment Indicators					
Recreational Vote	0.07	0.25	0	1	38,145,054
Recreational Possession	0.06	0.25	0	1	38,145,054
Dispensary Date	0.04	0.20	0	1	38,145,054
First Dispensary	0.04	0.20	0	1	38,145,054
Medical	0.45	0.50	0	1	38,145,054

Housing variables are at the individual property transaction level ist, where i is a single property in state s. t reflects the date of transaction. The Price and Price per Sq. Foot variables represent unique transaction prices and are deflated using the 2018 Consumer Price Survey. The home characteristics Bedrooms, Bathrooms, Sq. Feet, and Year Built are unique to a given property but not necessarily unique to the dataset if a given property was sold more than once during the sample period. State characteristic variables are yearly at the state level s. GDP is the gross domestic product in a given year, Population is the state's total population, Land is the total land area of state s in acres, and Density is Population divided by Land which represents how concentrated a state's population is geographically. Treatment indicators are those indicators described in Section 3.3.2.

public reporting requirements across all counties. For example, North Dakota has only one county which consistently reports transactions to the state's MLS, so we exclude it from our sample. Additionally, since this research is interested in the spillover effect of marijuana legalization of the housing market, we only consider homes which Zillow documents as residential properties. The richness of the data means that some states report business, government, and other non-residential properties. We exclude these observations.

The data is also filtered for observations that are likely non-market transactions. All included observations are categorized as a deed transfer, which signifies the exchange of a property's title from one party to another. Despite this, there are observations where a non-market transfer occurs between, for example, family members in the case of inheritance. These types of observations are often indicated as such, but in order to further exclude cases where reporting standards differ, we also filter for transactions which have a listed sales price below \$10,000 and above \$10,000,000. Doing so substantially reduces the sample size, but it is unlikely that homes below that price are actual market transactions given the price distribution. Additionally, states that have fewer than 100,000 transaction across the sample period are excluded in order to reinforce that a state's housing market sample is properly represented. We provide a more comprehensive examination of our data cleaning process for the Zillow data in Appendix.

3.3.2 Marijuana Laws

In addition to the housing and dispensary data, we used the legalization dates as determined by each state to identify our treatment conditions. As mentioned in the introduction, there are three possible legal states that marijuana can be classified as: legal to use recreationally, legal to use medicinally, and illegal. We used successful laws and ballot measures to indicate the relative legality of marijuana in each state. The information in this data is presented in Table 3.2. The second column reflects the date that a given state votes for and passes recreational legalization. The third column is the "effective date" for recreational legalization when either the result of a popular vote is approved or a law goes into effect. This is the date when it is no longer illegal to possess or grow marijuana for recreational purposes.

It is not until the date in Column (4) that there is a way to legally purchase recreational marijuana. An important distinction to note is the difference between the "Dispensary Date" and "First Dispensary" columns. In some cases, the ballot question outlines a specific date on which dispensaries are allowed to open. This is not always the case, however, as some states leave the decision when to open dispensaries up to local municipalities. This distinction is why Dispensary Date and First Dispensary are considered two separate treatments. Some states, such as California and Colorado, specify the Dispensary Date in their ballot questions, and as a result have dispensaries open on that date. In that case, the Dispensary Date and First Dispensary column dates are identical. Other states such as Massachusetts and Maine have large time gaps between the two dates due to local governing bodies having discretion over dispensary permit approvals. The preferred treatment and what is presented in our primary models is the Dispensary Date. We provide separate estimates for both variables, and consider the First Dispensary treatment as a robustness check.

We use a similar logic for cases of medical marijuana legalization. This process is significantly more complicated, however, as the regulations enacted by each state vary widely. A state may vote via a ballot measure or through the state legislature to legalize the use of marijuana for medicinal purposes, but the process following that approval has many additional steps. Similar to the recreational case, the law becomes effective as soon as it is passed, but the possession of marijuana is not necessarily legal due to the method through which the state distributes licenses. California, which was one of the first states to enact medical marijuana legalization, distributed medical

Table 3.2: Marijuana Legalization Laws

State	Vote	Possession	Dispensary Date	First Dispensary	Medical
Alaska	Nov 4, 2014	Feb 24, 2015	Feb 24, 2015	Oct 31, 2016	Mar 4, 1999
Arizona					Nov 2, 2010
Arkansas					Nov 9, 2016
California	Nov 8, 2016	Nov 9, 2016	Jan 1, 2018	Jan 1, 2018	Nov 6, 1996
Colorado	Nov 6, 2012	Dec 6, 2012	Jan 1, 2014	Jan 1, 2014	Jun 1, 2001
Connecticut					May 31, 2012
Delaware					Jul 1, 2011
Florida					Jan 3, 2017
Hawaii					Jun 14, 2000
Illinois	Jun 25, 2019	Jan 1, 2020	Jan 1, 2020	Jan 1, 2020	Jan 1, 2014
Louisiana					1978
Maine	Nov 8, 2016	Jan 30, 2017	May 2, 2018	Spring 2020 (Expected)	Dec 22, 1999
Maryland					Jun 1, 2014
Massachusetts	Nov 8, 2016	Dec 15, 2016	Jul 1, 2018	Nov 20, 2018	Jan 1, 2013
Michigan	Nov 6, 2018	Dec 6, 2018	Dec 1, 2019	Dec. 1, 2019	Dec 4, 2008
Minnesota					May 30, 2014
Missouri					Dec 6, 2018
Montana					Nov 2, 2004
Nevada	Nov 8, 2016	Jan 1, 2017	Jan 1, 2017	Jul 1, 2017	Oct 1, 2001
New Hampshire					Jul 23, 2013
New Jersey					Jul 1, 2010
New Mexico					Jul 1, 2007
New York					Jul 5, 2014
North Dakota					Apr 18, 2017
Ohio					Sep 8, 2016
Oklahoma					Jul 26, 2018
Oregon	Nov 4, 2014	Jul 1, 2015	Oct 1, 2015	Oct 1, 2015	Dec 3, 1998
Pennsylvania					May 17, 2016
Rhode Island					Jan 3, 2006
Utah					Dec 1, 2018
Vermont	Jan 22, 2018	Jul 1, 2018			Jul 1, 2004
Washington	Nov 6, 2012	Dec 6, 2012	Jul 8, 2014	Jul 8, 2014	Nov 3, 1998
Washington DC	Nov 4, 2014	Feb 26, 2015			Jun 20, 2010
West Virginia					Jul 1, 2018
Total	12	12	10	10	34

Note: Vermont and Washington D.C. have passed laws allowing for the possession and cultivation of recreational marijuana, but have yet to allow for sales at retail locations as of this writing in February 2020. The data was derived from legislative and ballot acts, which are compiled nationally at the Marijuana Policy Project – https://www.mpp.org/

license cards similar to a driver's license for those eligible for marijuana possession. Additionally, there are complications with prescriptions that vary by state which add a layer of complexity to identifying the timing of our effective date. It is also not always clear whether dispensaries that can sell medical marijuana to users with a valid prescription have opened, or if there is some other distribution mechanism that the state has adopted. As a result, we use a similar logic to the recreational case and consider the effective medical marijuana legalization date to be the date that a ballot measure is ratified or a state legislative measure is signed by the governor.

3.3.3 Dispensary Data

For our spatial analysis we use data from the Marijuana Enforcement Division of the Colorado Department of Revenue and the Washington State Liquor and Cannabis Board, which detail every dispensary location in the two states since their legalization of recreational marijuana. These data include the spatial coordinates of a given dispensary and the year it opened. Our estimation focuses on the opening of new dispensaries, so the data begins in 2014 when the first strictly recreational dispensaries opened in Colorado and Washington. It is worth noting however that there existed dispensaries in both states prior to recreational legalization due to the previous passage of medical legalization. Those dispensaries are taken as given and exist at the start of the data. The spatial identification strategy depends on the opening of new dispensaries, so whether a dispensary was an already-existing medical dispensary should have no bearing on the validity of the estimation. We combine the dispensary data with the Zillow housing data to estimate the effect of new dispensaries opening on the housing market in the immediate vicinity. This represents the within-state amenity effect of legalization.

3.4 Empirical Strategy

Our empirical strategy involves three primary specifications. First is a linear model, which we test with varying fixed effect levels to establish a baseline relationship between marijuana legalization (both MML and RML) and home prices. We estimate the following:

$$\begin{aligned}\log(\text{Price}_{ijst}) = {} & \alpha_1 \text{Recreational Vote}_{st} + \alpha_2 \text{Recreational Possession}_{st} \\ & + \alpha_3 \text{Dispensary Date}_{st} + \alpha_4 \text{Medical}_{st} + \beta X'_{ijst} + \delta_j + \rho_q + \epsilon_{ijst}\end{aligned} \tag{3.1}$$

Since the Zillow housing data is at the transaction level, our primary dependent variable Price_{ist} is the price of home i in county/city/ZIP j and state s at time t. In this simple model the variables of interest are $\text{Recreational Vote}_{st}$, $\text{Recreational Possession}_{st}$, $\text{Dispensary Date}_{st}$, and Medical_{st}, which are all binary variables indicating whether state s has adopted RML (for Recreational Vote, Recreational Possession, and Dispensary Dates) or MML (for Medical) at time t. $\text{Recreational Vote}_{st} = 1$ if the state has approved RML by ballot vote or a legislative statute by the transaction date, $\text{Recreational Possession}_{st} = 1$ if the RML law has gone into effect and it is legal to possess marijuana, $\text{Dispensary Date}_{st} = 1$ if dispensaries can apply for permits to sell recreational marijuana, and $\text{Medical}_{st} = 1$ if MML has been approved by state voters or legislators. In addition to these indicators, X'_{ijst} is a vector of housing characteristics and local economic measures including the number of bedrooms, bathrooms, the age of the home, state GDP, state population, and state land area. Finally we include location and time fixed effects, δ_j and ρ_q, respectively. We use year-quarter fixed effects for ρ_q, but the legalization dummies are defined by the exact date of RML voting, possession, and dispensary openings. This makes our models traditional hedonic estimations.

The second model employed is an unconditional quantile regression (UQR), as specified by Firpo et al. (2009) (FFL). Table 3.1 demonstrates the large amount of variation across the data, especially with regard to our outcome variable of choice, home price. The observed prices and house characteristics exhibit significant heterogeneity, which makes a UQR an attractive estimation strategy. As we demonstrated in Figure 3.1, 3.2, and 3.3, response to the housing recovery varied widely between RML states and non-RML states. Extending this idea to the distribution of prices, a UQR model accounts for systematic differences across states that may influence their decision to pass legalization measures. The UQR model is evaluated on the distribution of independent variables marginally. Because of this, the model does not depend on the covariates conditioned on as in a traditional conditional model.

The UQR model evaluates the impact of RML and MML on house prices across the price distribution using a recentered influence function (RIF) (Hampel et al. (2005)). Although the RIF can be applied to any distributional statistic, FFL use it to estimate quantiles along the distribution. The marginal effect of any quantile on the home price can be represented by:

$$\begin{aligned} E[\mathrm{RIF}(\mathrm{Price}_{ijst}; q_\tau)|\mathrm{RML}, \mathrm{MML}, X, \delta, \rho] &= \alpha_1 \mathrm{Recreational\ Vote}_{st} \\ + \alpha_2 \mathrm{Recreational\ Possession}_{st} &+ \alpha_3 \mathrm{Dispensary\ Date}_{st} \\ + \alpha_4 \mathrm{Medical}_{st} + \beta X'_{ijst} &+ \delta_j + \rho_q + \epsilon_{ijst} \end{aligned} \tag{3.2}$$

Model 3.2 is the same equation as in Model 3.1, with the only difference being the estimation of the RIF. q_τ in the RIF reflects each quantile being estimated. In our case we will derive estimates for each decile along the price distribution (i.e. $q_\tau = (0.1, 0.2, ..., 0.9)$). By estimating each decile, the RIF allows us to interpret the effect of RML across the distribution which may provide additional insight into the mechanisms behind legalization's impact on the housing market.

Like the fixed effects Model 3.1, the UQR estimates the difference in home prices along the distribution across states. It could be the case that there are differences within states that legalized marijuana use as well. To test this we use data from the

Marijuana Enforcement Division of the Colorado Department of Revenue, the state agency in Colorado tasked with regulating the sale of marijuana, and the Washington State Liquor and Cannabis Board. The agencies' data provide the location of marijuana dispensaries opened in the states between 2014-2018. By combining this data with the Zillow housing data, we are able to estimate the effect of a dispensary opening on neighborhood home values.

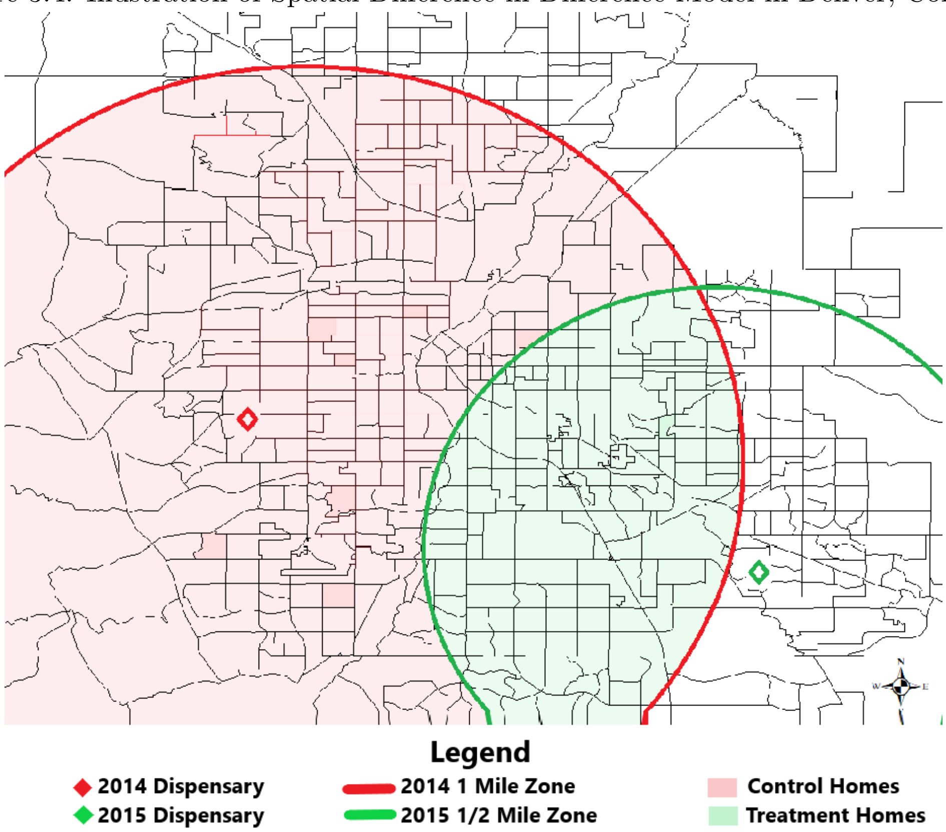
A clear source of endogeneity in a standard difference-in-differences (DiD) approach is that the location of a dispensary is not random; a firm chooses what it believes to be the most profitable location for its dispensary and finds suitable properties to rent or purchase. The firm may rent property in a business district or near transit, which could bias the housing market in the immediate area upward. On the other hand if these are new or inexperienced businesses that have capital constraints, they might locate where property is relatively inexpensive. This would have the opposite effect, as homes in less dense areas are generally on the lower tail of the price distribution.

To account for the endogeneity concern, we use a DiD approach developed in Dronyk-Trosper (2017). The authors use the local government's construction of public service facilities, such as fire departments and police stations, to identify changes in the local housing market. Control homes are those which maintain their distance from the closest facility throughout the sample period. Treatment homes are those which – at period t_0 – have the same distance as the control group but at some future period t_s, where $s > 0$, a new facility is constructed that reduces the distance to the nearest option. We modify this approach by substituting the public facilities for marijuana dispensaries. The spatial DiD model is represented by:

$$\log(\text{Price}_i) = \beta_1 \text{Treatment}_i + \beta_2 \text{State}_i + \beta_3 (\text{Treatment}_i \times \text{State}_i) + \gamma X_i + \epsilon_i \quad (3.3)$$

with Treatment_i is an indicator variable which reflects whether a home is in our treatment group – whether a new dispensary has opened closer to home i since period t_0. State_i is a dummy for whether a home sale occurred before or after the construction of a new closer dispensary, and X_i is a vector of home characteristic controls. β_3 is our variable of interest, which represents the change in home values for treated units following the opening of a new dispensary. Figure 3.4 demonstrates the buffer zones around marijuana dispensaries in the Denver metropolitan area and the homes that fall within the buffer zone. For the purpose of Model 3.3, only a subset of the homes that appear in Figure 3.4 will be included in our treatment group.

Figure 3.4: Illustration of Spatial Difference in Difference Model in Denver, Colorado



3.5 Results

3.5.1 Housing Prices Following Statewide Marijuana Legalization

Tables 3.3 and 3.4 estimate the effect of recreational marijuana legalization on housing prices using a simple linear model and a fixed effects model, respectively. In these tables and in the rest of the main specifications, the dependent variable is the logged value of home prices. Each column in the two tables includes a single treatment variable with the exception of Column (5), which includes three treatment variables. The treatment variable indicating the date recreational marijuana possession is legalized is excluded in Column (5) because, as indicated in Table 3.2, the gap between the vote and possession dates are typically no longer than a month. If this gap is longer than a month, then the possession date is typically very close to the first legal sales date. We estimate the coefficient for possession separately in Column (2) of Tables 3.3 and 3.4, and as expected its point estimate falls between the vote and sales points estimates.

In Table 3.3, as in the rest of the tables that follow, each estimation includes variables which control for house characteristics and state economic indicators. Table 3.3 includes city-level clustered standard errors to account for potential correlations of error terms, but does not include any fixed effects indicators. In this simple linear model the estimated coefficients of interest are large and significant, with each point estimate reflecting greater than a eighteen percent appreciation in home prices for the RML variables of interest. Table 3.4 includes city and year-quarter fixed effects for the same five estimations as Table 3.3. This table represents the primary linear cross-state results. Similar to the previous table, we find large and positive estimates for the three RML treatment indicators, again exceeding ten percent when considered individually. A noteworthy difference between the fixed effects and OLS models is the magnitude of the coefficients. Including fixed effects greatly reduced the estimated effect, which is to be expected considering the data is a national sample which features large amounts of heterogeneity in housing and economic characteristics.

Table 3.3: Effect of Marijuana Legalization on House Price per Sq. Foot (OLS)

	log (House Price)				
	(1)	(2)	(3)	(4)	(5)
Medical	0.414***				0.409***
	(0.035)				(0.034)
Recreational Vote		0.180***			0.110***
		(0.029)			(0.020))
Recreational Possession			0.186***		
			(0.029)		
Dispensary Date				0.152***	-0.024
				(0.035)	(0.029)
R-squared	0.322	0.281	0.281	0.280	0.323
Observations			38,145,054		

Note: (i) The Possession dummy is excluded in the main column (5) since the time gap between Recreational Vote and Possession or Possession and the Dispensary Date are typically quite small. (ii) Both house characteristics – which includes bedrooms, bathrooms, the year built – and state characteristics such as state per capita GDP and density are controlled for in each model. (iii) City level clustered standard errors in parenthesis to take into account potential correlation in the error terms. (iv) As a robustness check we use house price per square foot as the dependent variable, which can be seen in Table 3.A1 in Appendix B. $*** : p < 0.01$, $** : p < 0.05$, $* : p < 0.1$

The model is designed to identify the effect of RML specifically, but we include the medical coefficient in order to address the potential endogeneity issue of states voting in favor of recreational legalization. Policy treatment represents a selection issue as voters choose whether to vote in favor of marijuana legalization. As seen in Table 3.2, however, there are a large number of states which have legalized medical marijuana but only ten which have legalized recreational marijuana. Due to the limitations of the Zillow housing data discussed in Section 3.3.1, the only states which are in the RML treatment group are California, Colorado, Massachusetts, Nevada, Oregon, and Washington. RML treatment states make up less than a quarter of the MML states as a result. Every state that has enacted RML has enacted MML, but the inverse is not true. By including the medical treatment in our primary model specification, we cannot guarantee the consistency of the medical coefficient but we should recover the marginal effect for the two RML treatment variables.

Column (5) of Table 3.4 demonstrates that once we include city and year-quarter fixed effects into our primary linear model, both Recreational Vote and Dispensaries Date's coefficients retain large, positive, and significant point estimates. The larger effect happens at the Dispensary Date, when the first dispensary *could* open. This estimate reflects an eleven percent appreciation in home prices. As explained in Section 3.3.2, this is not necessarily the date that the first dispensary opens since each municipality in a given treatment state has different permitting rules for new businesses. As a robustness check, we use the opening date of the first dispensary in a state as the dispensary treatment and find qualitatively similar results. The estimated coefficient for the Recreational Vote treatment meanwhile reflects 5.4 percent price appreciation. Taken together, the two linear models support the hypothesis that RML induces large positive effects in the housing market.

Table 3.4: Effect of Marijuana Legalization on Home Price (Fixed Effects)

	log (Price)				
	(1)	(2)	(3)	(4)	(5)
Medical	0.039**				0.061***
	(0.019)				(0.020)
Recreational Vote		0.106***			0.054***
		(0.014)			(0.013)
Recreational Possession			0.107***		
			(0.014)		
Dispensary Date				0.138***	0.111***
				(0.015)	(0.010)
City FE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
R-squared	0.610	0.610	0.610	0.610	0.611
Observations	38,144,444				

Note: All models include city and year-quarter fixed effects. Beside our typical house characteristic controls (number of bedrooms, bathrooms, age), we also include local economic indicators at the state level. These include per capita GDP and population density. City level clustered standard errors are in parentheses to account for potential correlation in the error terms. As a robustness check we use house price per square foot as the dependent variable, which can be seen in Table 3.A1 in Appendix B. $*** : p < 0.01$, $** : p < 0.05$, $* : p < 0.1$

To further test the state-level effect of marijuana legalization on housing prices, we estimate an unconditional quantile regression (UQR) as specified by Firpo et al.

(2009). A UQR has three principle advantages over a traditional linear model despite the fact that it simply recovers the marginal effect of the treatment indicators. First, it is less sensitive to extreme values in the dependent variable. This is unlikely to be an issue in the data used for this paper as the number of observations is substantial, but it is nonetheless a strength of the model. Second, a UQR model accounts for differences across states that could affect the likelihood of a given state passing a marijuana legalization bill, which is a significant concern. Finally it marginalizes the treatment effect across the price distribution, which provides a more complete understanding of the impact of RML on the housing market.

With those advantages in mind, Figure 3.5 and 3.6 plot the UQR coefficients for each decile along the distribution. For a more precise view of the estimated coefficients, Appendix Tables 3.A3 and 3.A4 in Appendix display the point estimates. Again we have estimated two model specifications, one with the Dispensary Date treatment and one with First Dispensary due to the close time proximity of those two variables. A pattern emerges in both cases: there appears to be some significant effect in the Medical Vote or Recreational Vote treatments and a significant, positive, and increasing effect across the Dispensary Date/First Dispensary distributions. The Recreational Vote treatment show some significant appreciation in the top four deciles, but as in the linear models the Medical coefficients should be interpreted conservatively.

The positive effect in the upper deciles for the two Vote treatments range between a three and twelve percent increase in home price. The concentration, especially in $Q_\tau = .80, .90$ could point to the level of liquidity available to those purchasing the most expensive properties. For example, if those wealthy buyers have greater access to credit than buyers lower in the distribution, then their demand for marijuana and in turn housing in RML or MML states could shift immediately upon the success of a ballot measure. This interpretation would be consistent with the economic development

Figure 3.5: Unconditional Quantile Regression (Dispensary Date)

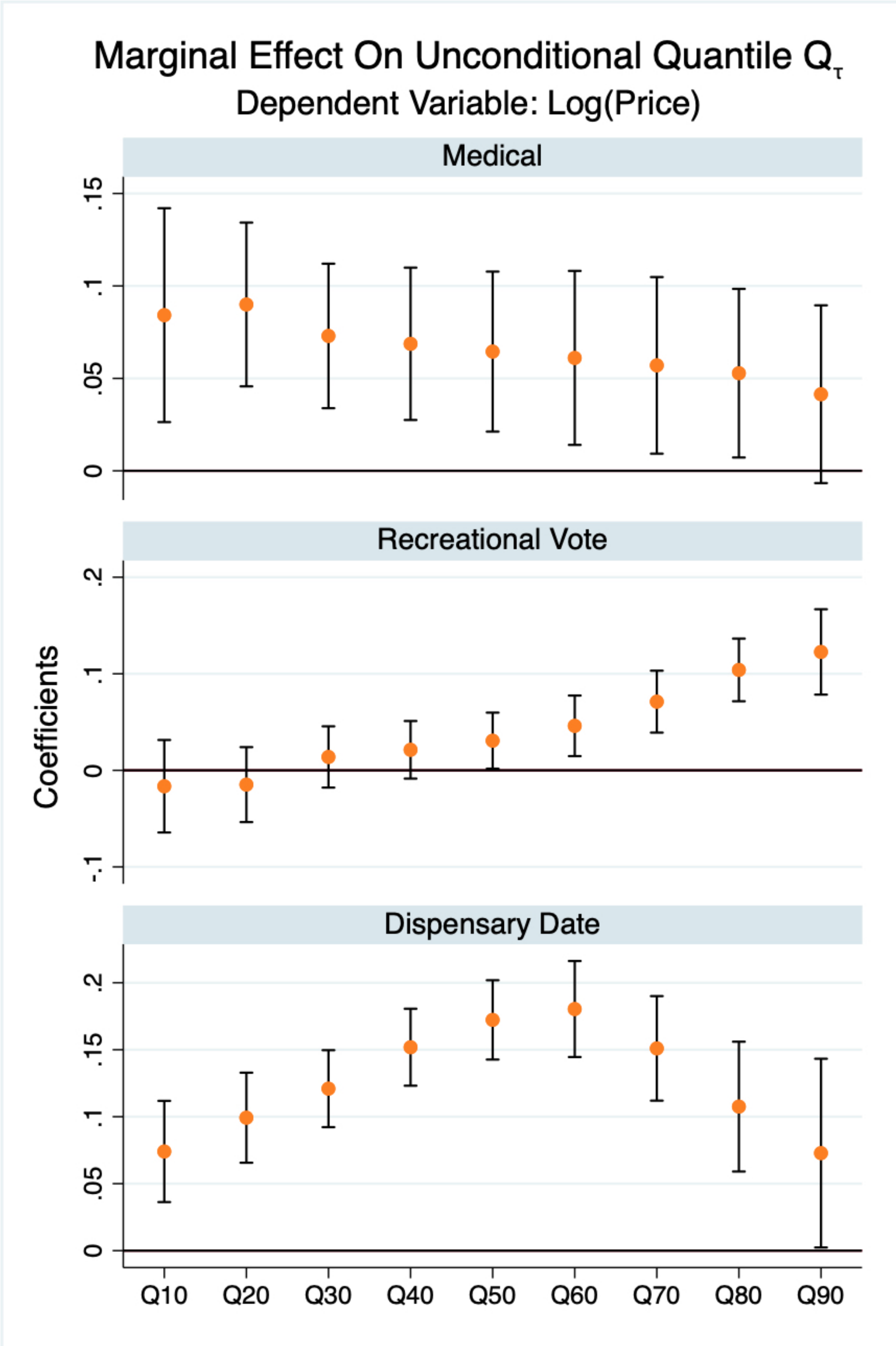
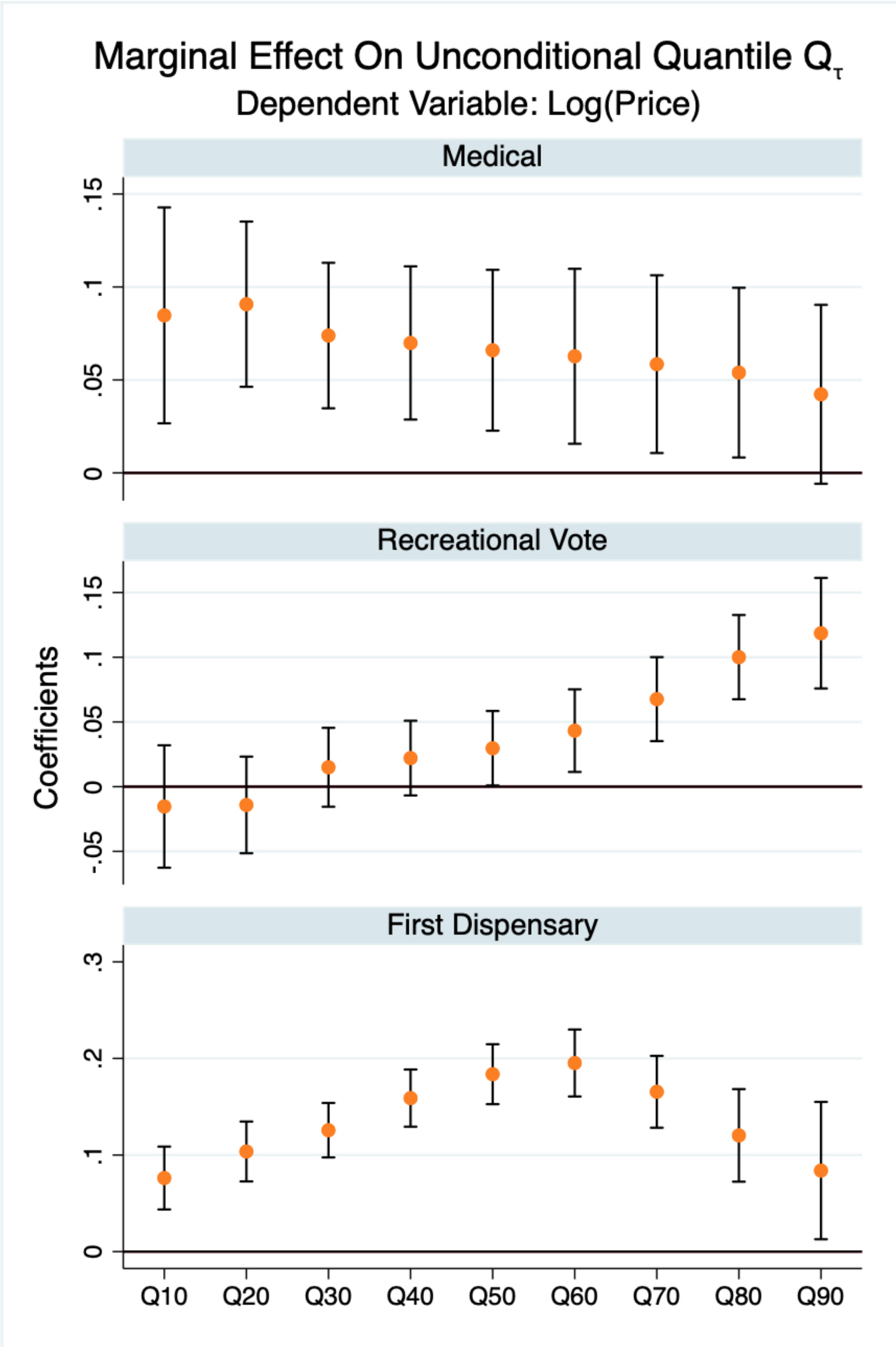


Figure 3.6: Unconditional Quantile Regression (First Dispensary)



hypothesis presented in Section 3.2.3; demand for housing is responsive to employment gains, which itself is a natural byproduct of new business creation, and potential in-migration. The results support the those from the linear fixed effects model estimated in Table 3.4, with the top two deciles dominating the average effect,

The Dispensary Date and First Dispensary treatments differ from the two Vote treatments in that they have large, positive, and significant effects across the price per square foot distribution. These values range from approximately seven percent to nineteen percent, with the point estimates increasing in magnitude until beginning to decrease at the 7th decile. It should be noted that the values in the 8th and 9th deciles have very large confidence intervals and so the point estimates may be overstating the effect. Regardless of the estimated confidence intervals, we can say with some certainty that the two dispensary treatment dates reflect a shift in housing demand in RML and MML states. This large effect again supports the hypothesis that the economic development effect drives the change in the housing market. Once recreational marijuana becomes available to buy easily at a dispensary and tax revenue is generated, there is significant home price appreciation.

3.5.2 Spatial Model

To further test whether it is open dispensaries that are driving the increased demand for housing, we estimate the results from a spatial model which identifies the effect of new dispensaries on the value of nearby homes. The model, which is described in Section 3.4 and follows the empirical strategy developed in Dronyk-Trosper (2017), estimates the within-state effect, as opposed to the cross-state effect of the linear and UQR models presented in the previous section. The various treated groups in this model represent homes which have already been "exposed" to a dispensary by having a dispensary open within a two-mile radius of the property. They are then considered

Table 3.5: Summary Statistics by Spatial Difference-in-Difference Treatment

	Inside 0.5 Miles		**Between 0.5 and 1 Mile**		**Between 1 and 2 Miles**		**Outside 2 Miles**	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
House Price ($)	413,997	373,542	364,451	338,805	378,964	355,227	368,097	296,083
Price per Sq. Foot ($)	255	193	214	149	198	132	186	128
Sq. Feet	1,711	873	1,723	846	1,941	963	2,046	964
Bedrooms	2.9	0.9	3.0	0.9	3.1	0.9	3.1	0.9
Bathrooms	2.0	0.9	2.1	0.8	2.3	0.8	2.4	0.9
Age of House at Sale (Years)	41.5	31.3	43.3	28.9	33.3	24.7	27.1	22.8
Observations	382,937		134,337		150,123		218,436	

The sample for the spatial difference-in-differences (SDD) model includes all home transactions in Colorado and Washington from 2014-2018. Each grouping represents the distance a home is from a dispensary, so for example homes in the first group are less than a half mile away from the nearest dispensary.

treated when a second dispensary opens geographically closer at a later date. Figure 3.4 demonstrates this idea graphically.

In order for this empirical strategy to be valid, homes in the treatment groups must not differ from each other in price and house characteristics. Table 3.5 presents the mean and standard deviation values for the four groups. The group “Inside 0.5 Miles” includes all homes sold which were within a half mile of a dispensary at any point in the sample period of 2014-2018 in Colorado and Washington; “Between 0.5 and 1 Mile” includes homes sold which were between a half and one mile of a dispensary at any point in the sample period; “Between 1 and 2 Miles” contains homes sold which were between one and two miles of a dispensary at any point in the sample period; and the “Outside 2 Miles” group includes homes which are outside a two-mile radius of any dispensary.

Table 3.6 presents the results for the spatial difference-in-differences models. Like the linear and UQR estimates in the previous section, each of the models have the logged value of price as the dependent variable. Column (1) is a simple fixed effects model, where the point estimates for 1/2 Mile Zone, 1 Mile Zone, and 2 Mile Zone reflect the premium for homes within a two mile radius of a dispensary in Colorado and Washington during our sample period. This model in this column has no causal mechanism and simply estimates the mean difference between homes near (i.e. within two miles) of a dispensary and those outside that bound. Homes within 0.5 miles have a slight premium of 4.5 percent, but homes between 0.5 miles and one mile and homes between one and two miles have a slight discount.

Table 3.6: Spatial Difference-in-Differences

	log (Price)		
	(1)	(2)	(3)
1/2 Mile Zone	0.045***	0.072***	0.082***
	(0.002)	(0.002)	(0.002)
1 Mile Zone	-0.028***	0.009***	
	(0.002)	(0.002)	
2 Mile Zone	-0.034***		
	(0.002)		
Control Group	Outside 2 Miles	Within 2 Miles	Within 1 Mile
Observations	885,833	650,437	565,923
R-squared	0.406	0.425	0.431

The sample includes transactions in the period between 2014 and 2018 in Colorado and Washington. Logged county level data such as county number of employees, wage, and the county employment ratio (county employees/state total employees), as well as home characteristics including the number of bedrooms, the square value of bedrooms, the age of the home, the number of bathrooms, and the square footage of the home, are used in the regression to control for differences across the states. Column 1 is an OLS model where treatment homes are homes that fall within 2 miles or closer of a dispensary and control homes are home that are not within 2 miles of a dispensary. Column 2 is the spatial difference in difference model where the control group becomes all homes that fall within 2 miles of a dispensary and the treatment group are homes that start off within 2 miles of a location and move within .5 or 1 mile of a dispensary. Column 3 is the same but now control are home starting off 1 mile and moving within .5 miles of a dispensary. Robust standard errors in parenthesis. $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$

The primary spatial model specifications appear in Columns (2) and (3) of Table 3.6. Both columns follow the identification strategy in Dronyk-Trosper (2017), and so can be interpreted as the causal effect of a marijuana dispensary opening on the local housing market. Column (2) uses homes within two miles of a dispensary as the control group. The two treatment variables – 1/2 Mile Zone and 1 Mile Zone – are indicators for homes which previously were within two miles of a dispensary and were subsequently sold after a new dispensary opens. The sold homes are newly situated within a half mile or between a half mile and a mile of a dispensary, respectively. The coefficients for 1/2 Mile Zone and 1 Mile Zone represent the premium for these homes. Both treatment zones experience an appreciation in price after the construction of a new dispensary. The 1 Mile Zone homes increase in value by slightly under one percent and the 1/2 Mile Zone homes increase by slightly over seven percent. Column (3) is the same specification, except now the only treated homes are those within a half mile of a new dispensary. The homes in 1 Mile Zone that were previously considered part of our treatment group in Column (2) are now included in the control group. Again the estimated coefficient for the half mile group is significant and positive with an eight percent appreciation. In order to guarantee that the results are not being driven by one of the two state's effect dominating the other, we separate the sample into tables for Colorado and Washington as a robustness check. Appendix tables 3.A5 and 3.A6 appear in Appendix. The results are similar between the two states and between the individual state estimates and the combined estimates, suggesting that this effect is not due to one state's influence.

Dronyk-Trosper (2017) find that the effect of municipal government service buildings, such as police stations and firehouses, increases the value of homes at a decreasing rate. Those homes closest to the government buildings actually decrease in value, likely as a response to the increased traffic and noise associated with those services. Our results imply the opposite; when a dispensary opens nearby, homes closest to it appreciate in price the most. This is consistent with our interpretation that new dispensaries act as amenities in the local housing market. Since the spatial model is restricted to Washington and Colorado – the first two states to legalize recreational marijuana – we cannot guarantee that these results generalize to each subsequent state that legalizes. However, together with the cross-state models presented in the previous section, it is clear that recreational marijuana legalization has large positive effects on the housing market of states that legalize and municipalities which allow dispensaries to open in their communities.

3.5.3 Robustness Checks

There are two primary robustness check categories we employ. First, we use the home price per square foot as the dependent variable rather than home price. Geographic heterogeneity in our sample suggests that simply using house price as the dependent variable could bias the results since treatment homes are in high-price states. By using house price per square foot as the dependent variable, we can ensure that this potential source of bias is accounted for. Second, we include the First Dispensary treatment in place of the Dispensary Date variable for the reasons outline in Section 3.3.2. If the primary mechanism in our cross-state models is the economic development effect, then it is possible that the impact is only felt once the first dispensaries open and a large volume of marijuana sales take place, thereby generating tax revenue.

Appendix Table 3.A1 uses the log value of house price per square foot as the dependent variable in the two linear cross-state models. In this table, Dispensary Date is still the right-hand side treatment variable of choice. As in the price per square foot results, the OLS model in the first five columns shows large positive results for all four treatment variables, including the Medical Vote treatment. Again, these results should be interpreted carefully as the Medical Vote treatment is likely absorbing a large amount of the effect due to the lack of time fixed effects. That being said, the point estimates are very similar to those presented in Table 3.3. The same can be said for the fixed effects results in columns (6) through (10). The Recreational Vote variable is still significant and positive, as is the Dispensary Date. The point estimates are large and positive, as in the original specification.

Next, we check our results using First Dispensary as our treatment variable of interest rather than Dispensary Date. For some states these dates are the same, so we would expect the results to be very similar. Appendix table 3.A2 presents the estimates, and indeed that is what we find. The results are consistent with the Dispensary Date results. Once again, there are positive effects for each of the two RML variables, Recreational Vote and First Dispensary, just as in our primary results. The magnitude of the First Dispensary estimates are similar to those for Dispensary Date presented in Table 3.4. Appendix table 3.A2 also presents the original model specification with various levels of controls. Excluding house characteristic and local economic variables do no affect the magnitude or significance of the estimated models.

3.6 Conclusion

Uncertainty regarding the costs and benefits of marijuana legalization, along with marijuana's status on the federal level as a Schedule 1 drug, have made the public reluctant to support policies which liberalize its use and distribution. To help fill this information gap, this research demonstrates that there is a large positive spillover effect on the housing market following legalization. We further support these findings with a spatial approach which shows that within states that legalize recreational marijuana use, homes experience a positive valuation shock when a dispensary opens nearby. The results are robust to a number of of specifications, including a different (but temporally similar) date for the actual sale of marijuana at dispensaries. Taken together, the inter and intra-state results suggest that preferences for public services – derived from a new source of tax revenue – and dispensaries as a commercial amenity create largely positive effects following the legalization of recreational marijuana.

The impact of legalization on the housing market is supported by two models. First, a fixed effects model demonstrates a five percent appreciation in home prices following the passage of RML and an eleven percent appreciation once sales of marijuana products begin. Extending this logic to an unconditional quantile regression approach, we find positive effects across the home price distribution following the date that dispensaries are allowed to open. Differences across the price distribution can likely be thought of as heterogeneous preferences among different levels of wealth. The promise of future funding to schools and other public infrastructure as a result of legalization supports a long literature showing a positive relationship between home prices and local economic development.

To approximate the effect of dispensaries we estimate a spatial model in Colorado and Washington. The results again show price appreciations for homes as the distance to the nearest dispensary decreases. This demonstrates that is it not simply the benefits of increased tax revenue, but also the existence of the dispensaries themselves, that is driving the price increases. The dispensaries act as commercial amenities that the public puts a premium on being nearby.

Without the benefit of foresight, our research is not able to determine whether the positive effect will persist. For example if immigration inflows are the primary cause of our results, then we would expect that states would experience diminishing returns to legalization. The first cohort of states which legalized recreational marijuana would draw those that valued legalization most, and each successive state should not expect a similar inflow. Additionally, more research on marijuana legalization is required to fill in the remaining knowledge gaps. We do not estimate some of the other second-order effects, such as the impact on policing and the outcomes for minority communities that were previously convicted for marijuana possession at a disproportionate rate. Future research would be well served to approach these questions, as it will better inform the public and policy makers with respect to the reclassification of recreational drugs.

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Appendix: Data Cleaning Description

Zillow Housing Data

Considering the size and scope of the Zillow ZTRAX repository, it is necessary to document the data cleaning process used for this research. However, in order to create a dataset that is both national and representative, some adjustments were made to the import process. In general, the effort follows Zillow's own script which creates a hedonic dataset.[5] The end product results in a dataframe in which each row is a home transaction and each column reflects home and transaction characteristics. The files are initially imported state-by-state and then appended together to make a master file.

The process goes as follows. First, three tables are imported from the Assessment repository: Main, Building, and BuildingAreas. These three tables combine to provide house characteristics, as well as information about the type of property exchanged in a given transaction. For example, the variable "PropertyLandUseStndCode" in the Building table details whether a property is a single-family residence, used in industry, is a farm, et cetera. We erred on the side of inclusivity when filtering for these variables during import, as reporting standards across counties and states vary widely. The properties included are described as follows in Zillow's documentation:

1. Residential General
2. Single-Family Residences
3. Rural Residences
4. Mobile Home

[5] The original file is publicly available on the firm's ZTRAX GitHub repository: `https://github.com/zillow-research/ztrax/blob/master/ExampleRcode_UsingZTRAXtoCreateHedonicDataset.R`

5. Townhouse
6. Cluster Home
7. Condominium
8. Cooperative
9. Row House
10. Planned Unit Development
11. Residential Common Area
12. Seasonal, Cabin, Vacation Residence
13. Bungalow
14. Zero Lot Line
15. Manufactured, Modular, Prefabricated Homes
16. Patio Home
17. Garden Home
18. Landominium
19. Inferred Single-Family Residential

Also, following the logic described by Zillow, we filter the "BuildingAreaStndCode" from the BuildingAreas table in order to get as accurate a measure of total square footage as possible. Again, different counties have different reporting standards as to what is included in their square footage calculations, so to ensure consistency we have included only those options which enumerate the buildings on the property, not the land itself. These two filters – for "PropertyLandUseStndCode" and "BuildingAreaStndCode" – are the only two at this point in the process. Once this is complete, the three assessment tables are merged to create a single assessment file with all the necessary housing characteristic variables to be used in analysis.

The second set of data comes from the Transaction repository. Included are the PropertyInfo and Main tables. All the information provided here reflects the transaction itself, not any characteristics of the home. This includes variables like the price of the transaction, the date of transfer, and the type of transfer. The only filtering that occurs in this step is in regard to the variable "DataClassStndCode," which details the type of transaction occurring. Since the subject of study are property transactions, only deed transfers and deed transfers with concurrent mortgages are included. This excludes other types of transactions, including foreclosures and inter-family transfers as in the case of inheritances. These two tables are appended together to make a single transaction file. Finally, the transaction and assessment files are combined to make a single master file for a given state. The states files are then appended together to make a national-level dataset which is then used for analysis.

The master file is filtered to exclude extreme observations, as well as define the period of study. To ensure that results are not being driven but incorrect or implausible observations, we drop transactions which had sales prices below \$10,000 and above \$10,000,000, similar to Cheng et al. (2018). On the lower end it is unlikely that transactions with prices below \$10,000 occurred on the market, and may have slipped through the "DataClassStndCode" filter. Prices above \$10,000,000 are extraordinary and in some cases are likely the result of data entry errors. Similarly, house characteristics are filtered to exclude observations that are in the top thousandth or top ten-thousandth percentile. Doing so, for example, eliminated an observation with over 1000 bedrooms. This process removed a large number of observations in states which do not require counties to report the home characteristics, leaving small states like Maine with just 11,000 transaction observations. To guarantee a representative sample, we then dropped states which did not have at least 100,000 observations. That is an arbitrary standard, but by doing so we can more confidently argue that each states' market is properly represented. Finally, prices were adjusted to reflect 2018 prices using the Federal Reserve's Consumer Price Index.

Appendix: Additional Model Specifications

Table 3.A1: Effect of Recreational Marijuana Legalization on House Price per Sq. Foot

	log (Price per Sq. Foot)									
	OLS					Fixed Effects				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Medical	0.466***				0.461***	0.046**				0.069***
	(0.035)				(0.035)	(0.020)				(0.021)
Recreational Vote		0.187***			0.088***		0.108***			0.055***
		(0.030)			(0.020)		(0.014)			(0.014)
Recreational Possession			0.193***					0.109***		
			(0.031)					(0.015)		
Dispensary Date				0.169***	0.001				0.141***	0.116***
				(0.037)	(0.030)				(0.015)	(0.011)
Bedrooms	-0.434***	-0.458***	-0.458***	-0.459***	-0.433***	-0.102***	-0.102***	-0.102***	-0.102***	-0.102***
	(0.035)	(0.036)	(0.036)	(0.036)	(0.036)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Bedrooms^2	0.044***	0.047***	0.047***	0.047***	0.044***	0.004*	0.004*	0.004*	0.004*	0.004*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Bathrooms	0.188***	0.192***	0.192***	0.192***	0.187***	0.044***	0.044***	0.044***	0.045***	0.045***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Age	0.001**	0.001	0.001	0.001	0.001**	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age^2	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
GDP Per Capita	0.356***	0.828***	0.826***	0.870***	0.314***	1.323***	1.273***	1.273***	1.302***	1.252***
	(0.053)	(0.062)	(0.062)	(0.061)	(0.059)	(0.109)	(0.114)	(0.114)	(0.113)	(0.111)
Density	0.004***	-0.004	-0.003	-0.004	0.005***	-0.003	-0.002	-0.002	-0.002	-0.001
	(0.001)	(0.003)	(0.003)	(0.003)	(0.001)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
City FE	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
City Clustered S.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.161	0.099	0.099	0.098	0.162	0.528	0.529	0.529	0.529	0.530
Observations	38,145,054	38,145,054	38,145,054	38,145,054	38,145,054	38,144,444	38,144,444	38,144,444	38,144,444	38,144,444

Note: (i) The dependent variable is the log of house price per square foot while the first half columns are OLS results and the latter half are FE results. (ii) Possession dummy is excluded in our main columns (5) and (10) since the time gap between vote and possession, or sale and possession are too small to capture significantly valuable variations. (iii) City level clustered standard errors in parenthesis to take into account the correlations of error terms. $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$

Table 3.A2: Effect of Recreational Marijuana Legalization on House Price (Robustness)

	log (Price)				
	(1)	(2)	(3)	(4)	(5)
Medical	0.094***	0.101***	0.095***	0.102***	0.062***
	(0.023)	(0.023)	(0.023)	(0.023)	(0.020)
Recreational Vote	0.168***	0.154***	0.161***	0.146***	0.052***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Dispensary Date	0.063***	0.074***			
	(0.013)	(0.013)			
First Dispensary			0.078***	0.091***	0.120***
			(0.011)	(0.011)	(0.011)
Bedrooms		0.023		0.023	0.035*
		(0.021)		(0.021)	(0.020)
Bedrooms^2		-0.005**		-0.005**	-0.007***
		(0.002)		(0.002)	(0.002)
Bathrooms		0.136***		0.136***	0.135***
		(0.007)		(0.007)	(0.007)
log(Sq. Feet)		0.654***		0.654***	0.645***
		(0.017)		(0.017)	(0.016)
Age		-0.002***		-0.002***	-0.002***
		(0.000)		(0.000)	(0.000)
Age^2		0.000*		0.000*	0.000**
		(0.000)		(0.000)	(0.000)
GDP Per Capita					1.282***
					(0.104)
Density					-0.001
					(0.002)
R-squared	0.427	0.601	0.428	0.601	0.611
Observations			38,144,444		

Note: (i) Various levels of controls are used to ensure that the models are not misspecified. (ii) City level clustered standard errors in parenthesis to take into account the correlations of error terms. $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$

Table 3.A3: Heterogeneous Effect of Marijuana Legalization on House Price across Q_τ

	Log(Price per Sq. Foot)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Q10	Q20	Q30	Q40	Q50	Q60	Q70	Q80	Q90
Recreational Vote	-0.016	-0.015	0.014	0.021	0.031**	0.046***	0.071***	0.104***	0.123***
	(0.024)	(0.020)	(0.016)	(0.015)	(0.015)	(0.016)	(0.016)	(0.017)	(0.023)
Dispensary Date	0.074***	0.099***	0.121***	0.152***	0.172***	0.180***	0.151***	0.108***	0.073**
	(0.019)	(0.017)	(0.015)	(0.015)	(0.015)	(0.018)	(0.020)	(0.025)	(0.036)
Medical	0.084***	0.090***	0.073***	0.069***	0.064***	0.061**	0.057**	0.053**	0.041*
	(0.030)	(0.023)	(0.020)	(0.021)	(0.022)	(0.024)	(0.024)	(0.023)	(0.025)
R-squared	0.060	0.123	0.179	0.218	0.243	0.254	0.247	0.220	0.163
Number of Cities					10,640				
Observations					38,145,054				

Note: (i) Possession dummy is excluded since the time gap between vote and possession, or sale and possession are quite small. (ii) House characteristics such as the number of bedrooms, bathrooms, year built and state characteristics such as state GDP, population, land area, and density are controlled in the regressions. (iii) City level clustered standard errors in parenthesis to take into account the correlations of error terms. $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$

Table 3.A4: Heterogeneous Effect of Marijuana Legalization on House Price across Q_τ

	Log(Price per Sq. Foot)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Q10	Q20	Q30	Q40	Q50	Q60	Q70	Q80	Q90
Recreational Vote	-0.015	-0.014	0.015	0.022	0.030**	0.043***	0.068***	0.100***	0.119***
	(0.024)	(0.019)	(0.016)	(0.015)	(0.015)	(0.016)	(0.017)	(0.017)	(0.022)
First Dispensary	0.076***	0.104***	0.126***	0.159***	0.184***	0.195***	0.165***	0.120***	0.084**
	(0.017)	(0.016)	(0.014)	(0.015)	(0.016)	(0.018)	(0.019)	(0.024)	(0.036)
Medical	0.085***	0.091***	0.074***	0.070***	0.066***	0.063***	0.058**	0.054**	0.042*
	(0.030)	(0.023)	(0.020)	(0.021)	(0.022)	(0.024)	(0.024)	(0.023)	(0.025)
R-squared	0.060	0.123	0.179	0.218	0.243	0.254	0.247	0.220	0.164
Number of Cities					10,640				
Observations					38,145,054				

Note: (i) First Dispensary is used in place of Dispensary Date for the purpose of a robustness check. (ii) The Possession dummy is excluded since the time gap between Recreational Vote and Recreational Possession, or First Dispensary and Recreational Possession are quite small. (iii) House characteristics such as the number of bedrooms, bathrooms, year built and state characteristics such as state per capita GDP, and density are controlled in the regressions. (iv) City level clustered standard errors in parenthesis to take into account the correlations of error terms. $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$

Table 3.A5: Spatial Difference-in-Differences: Colorado Subsample

	log (Price)		
	(1)	(2)	(3)
1/2 Mile Zone	0.059***	0.114***	0.123***
	(0.003)	(0.003)	(0.003)
1 Mile Zone	-0.036***	0.041***	
	(0.003)	(0.003)	
2 Mile Zone	-0.067***		
	(0.003)		
Control Group	Outside 2 Miles	Within 2 Miles	Within 1 Mile
Observations	447,501	256,699	218,605
R-squared	0.411	0.414	0.413

The results in this table are from the same model specification as in the Spatial Difference-in-Differences Table 6, but limited to the observations in the Colorado subsample. House characteristics and county-level economic data are used as controls with robust standard errors. $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$

Table 3.A6: Spatial Difference-in-Differences: Washington Subsample

	log (Price)		
	(1)	(2)	(3)
1/2 Mile Zone	0.061***	0.065***	0.061***
	(0.002)	(0.002)	(0.002)
1 Mile Zone	-0.015***	-0.006***	
	(0.003)	(0.002)	
2 Mile Zone	-0.013***		
	(0.003)		
Control Group	Outside 2 Miles	Within 2 Miles	Within 1 Mile
Observations	438,332	393,738	347,318
R-squared	0.491	0.510	0.519

The results in this table are from the same model specification as in the Spatial Difference-in-Differences Table 6, but limited to the observations in the Washington subsample. House characteristics and county-level economic data are used as controls with robust standard errors. $***: p < 0.01$, $**: p < 0.05$, $*: p < 0.1$