The Impact of Post-9/11 Airport Security Measures on the Demand for Air Travel*

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Abstract

We examine the impact of post-9/11 airport security measures on air travel in the U.S. Using five years of data on passenger volume, we evaluate the effects of the implementation of baggage screening and the federalization of passenger screening on the demand for air travel. These two congressionally mandated measures are the most visible changes in airport security following the 9/11 attacks. Exploiting the phased introduction of security measures across airports, we find that baggage screening reduced passenger volume by about six percent on all flights, and by about nine percent on flights departing from the nation's fifty busiest airports. In contrast, federalizing passenger screening had little effect on passenger volume. We provide evidence that the reduction in demand was an unintended consequence of baggage screening and not the result of contemporaneous price changes, airport-specific shocks, schedule changes, or other factors. This decline in air travel had a substantial cost. "Back-of-the-envelope" calculations indicate that the airline industry lost about $1.1 billion, eleven percent of the loss attributed to 9/11 directly.

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1 Introduction

In response to the terrorist attacks of September 11, 2001, the federal government enacted new legislation to increase air passenger safety. On November 19, 2001, President Bush signed into law the Aviation and Transportation Security Act (ATSA). This act established a new Transportation Security Administration (TSA), which consolidated security efforts inside the Department of Transportation (DOT). In addition, the ATSA mandated several important changes in civil aviation security procedures. The two primary changes in airport security visible to passengers were the federalization of passenger security screening at all U.S. commercial airports by November 19, 2002, and the requirement to begin screening all checked baggage by December 31, 2002. The ATSA charged the TSA with overseeing security operations and implementing the mandates at all 429 commercial airports in the U.S.

While these new security regulations were enacted to ensure passenger safety and restore confidence in the U.S. aviation system, the overall effect of the new regulations on passenger demand is unclear. On one hand, greater confidence in the safety of air travel should spur demand. On the other hand, by requiring additional time and effort on the part of passengers, the inconvenience of security procedures may reduce demand for air travel. We examine the data to find the net effect.

Our identification strategy exploits variation in the timing of security implementation to estimate reduced-form models of its effect on demand. We control for unobserved airline industry conditions and time trends by including fixed effects for each carrier-segment and each time period.[1] To confirm that we are correctly attributing changes in demand to security measures instead of unobserved airport conditions, we explicitly compare a "treatment" group of originating passengers, who are affected by

[1]A segment is direct service from an originating airport to a destination airport.

the new airport security measures, with a "control" group of connecting passengers, who are not affected. Passengers and their bags are not screened on connecting flights and thus connecting passengers provide a near counterfactual—individuals flying the same segment on the same carrier from the same airport during the same calendar quarter who do not pass through security. In addition, we estimate a separate equation for return travel to discover if security implementation at destination airports in round-trip travel influences our results. Finally, we examine whether demand changes are a response to contemporaneous price changes or to changes in airline schedules rather than to security measures.

We primarily use data from two sources. Data on domestic passenger volume from 1999 to 2003 come from the Department of Transportation's Data Bank 1B (DB1B) of the Origin and Destination (O&D) survey. We use TSA press releases to establish the timing of airports' adoption of the security procedures.

Our results indicate that baggage screening reduced originating passenger volume by six percent at all airports and by nine percent at the nation's fifty busiest airports. At the same time, we find no evidence that baggage screening reduced connecting passenger volume from the same airports, on the same flights. In addition, we observe larger declines in passengers flying shorter trips, for which passengers are more likely to substitute driving for flying following the implementation of the new security procedures. Furthermore, we find that neither contemporaneous price changes nor airline schedule changes can explain our results. Prices remain stable after the implementation of baggage screening, while frequency of service is uncorrelated with the introduction of baggage screening.

In contrast to baggage screening, federalizing passenger screening had no impact on passenger volume. The effect was slightly negative in most of our models, but statistically significant in only one.

These results suggest that regulatory efforts to enhance airport security, in response to the terrorist attacks of 9/11, had the unintended consequence of reducing the convenience of air travel, which in turn caused a decline in the demand for air travel. This decline has substantial welfare implications. "Back-of-the-envelope" calculations indicate that the airline industry lost about $1.1 billion in revenues due to the reduction in demand, eleven percent of the amount that the General Accounting Office estimates the industry lost because of 9/11 itself (General Accounting Office 2001). Similar calculations suggest that substitution of driving for flying by travelers seeking to avoid security inconvenience likely led to over 100 road fatalities. These unintended consequences must be weighed relative to the intended enhancements to passenger safety and confidence in order to evaluate the effectiveness of these regulatory responses.

The rest of the paper proceeds as follows. In section 2, we provide more details about security changes since 9/11. Section 3 lays out the competing hypotheses for the effect of security measures on air travel demand. Section 4 discusses the data, section 5 details our identification strategy and estimation, and section 6 indicates our results. Section 7 considers the plausibility of the results, section 8 discusses their implications, and section 9 summarizes them.

2 Changes in Airport Security Since September 2001

Following the terrorist attacks of September 11, 2001, air travelers experienced many changes in airport security procedures. For example, airlines instructed passengers to arrive at airports as much as two hours before takeoff for domestic flights. After passing through security checkpoints, passengers were randomly selected for additional screening, including hand-searching of their carry-on bags, in the boarding area. Following an incident in December 2001, in which a passenger attempted to light a bomb in his shoe while in flight, security screeners asked passengers to remove their shoes when passing through checkpoints.

In this paper, we focus on two particular changes in airline security: the federalization of passenger screening operations, and the requirement that airports screen all checked baggage for explosives. These two changes are the most visible federal regulatory responses to the 9/11 attacks related to improving airline security.

2.1 Federalization of passenger screening operations

TSA officially took over responsibility for airport security in February 2002. Initially, TSA retained private security screeners. However, over a period of nearly seven months, starting at Baltimore-Washington International Airport (BWI) on April 30, 2002, and concluding on November 19, 2002, TSA employees began to conduct passenger-screening operations at all U.S. commercial airports.[2]

TSA made three important changes in an effort to improve the efficiency of security screening operations. First, at least initially, TSA increased staffing to help reduce waiting time in security lines. Prior to 9/11, there were about 16,200 private security screeners employed at U.S. airports, nearly all of whom were screening passengers. By the end of 2002, TSA had hired 56,000 screeners for both passenger and baggage screening (TSA, 2002). Second, TSA increased the compensation of screeners, offering higher wages and better benefits. Perhaps as a result, turnover

[2]Five airports, San Francisco (SFO), Kansas City (MCI), Tupelo (TUP), Rochester (ROC), and Jackson Hole (JAC), were included in a pilot program in which private security screeners operated under the supervision of TSA screeners. These private screeners were required to go through all TSA training, background checks, etc. We include these in our sample and treat them as airports with TSA screeners. Excluding them from the sample does not change our results.

among security screeners plummeted (General Accounting Office 2003). Third, TSA increased training for screeners. For example, prior to 9/11, x-ray machine operators at private security firms averaged about twelve hours of training (Seidenstat, 2004), while TSA requires more than a hundred hours of training for all of its passenger and baggage screeners (General Accounting Office 2003).

Despite these changes, reports of resource allocation problems have plagued TSA's passenger screening operations. Initially, TSA allocated screeners based on airport passenger volumes and screening lanes. According to the House Subcommittee on Aviation Security, this resulted in "'thousands standing around' at major connecting airports, where most passengers do not pass through screening, and shortages at origin and destination airports" (Subcommittee on Aviation 2004). Responding to concerns about imbalances and overstaffing, TSA reduced its screener workforce to 45,300 screeners by January 2004 (Subcommittee on Aviation 2004). However, the workforce reductions, combined with recent difficulties in hiring additional screeners (General Accounting Office 2003), have resulted in several major airports being understaffed, and in reports of long security lines, flight delays, and passengers missing flights.

2.2 Baggage screening procedures

Unlike passenger screening, no general system for screening checked baggage was in place prior to 9/11, and only five percent of checked bags were screened (Transportation Security Administration 2002). The TSA introduced baggage screening in two stages. Beginning on January 16, 2002, all airlines had to either adopt positive bag matching, in which they matched each piece of checked luggage to a passenger on board a flight, or to screen checked baggage for explosives using one of four methods: explosion detection systems (EDS), explosion trace detection (ETD) machines, bomb-sniffing dogs, or manual searching of bags. EDS machines are about the size of an SUV, process 150-200 bags per hour, and generate false positive identifications of explosives in almost 30 percent of bags (Butler and Poole 2004). ETD machines are much smaller and more labor-intensive (Butler and Poole 2004) since a screener must place a swab from each bag in the machine for analysis. Congress required TSA to install EDS machines at all U.S. commercial airports by the end of 2002. However, the many airports that were unable to meet this deadline were allowed to use alternative screening methods. Nonetheless, by January 1, 2003, more than 90 percent of all checked bags were being screened electronically, using either ETD or EDS machines (Transportation Security Administration 2002). The remaining checked bags were screened using either dogs or hand searches, or matched to passenger lists.

TSA has configured the baggage screening process in three different ways. In most airports, passengers first check-in at the ticket counter, and then take their baggage to a screening area, where it is screened using either ETD or EDS machines. In the majority of remaining airports, passengers first have their baggage screened and then proceed to the ticket counter to check-in. In each of these cases, if the electronic screening technology indicates the presence of explosives or other prohibited items, then additional manual screening is done, which takes time and recently has prompted many complaints of theft or destruction of passengers' property (De Lollis 2003). With either system, the baggage screening process requires additional time and effort on the part of passengers. Finally, a small number of airports (seven by the end of 2003) have installed in-line EDS, which imposes no additional time or inconvenience on passengers. With in-line EDS, passengers simply hand their checked baggage to airline agents at the ticket counter. Screening is done out of the passengers' view, while the passengers are able to proceed directly to the passenger screening area. As of January 2004, TSA had deployed more than 1,100 EDS machines and more than 7,200

ETD machines in airports around the country (Subcommittee on Aviation 2004).

3 The Effects of Security Regulations

The effects of TSA airport security measures on the demand for air travel are ambiguous. On the one hand, tighter security measures make traveling less convenient. Since 9/11, the best example of increased inconvenience is the need for passengers on domestic flights to arrive at airports as much as two hours prior to scheduled departures. Similarly, the random hand-searches of passengers and their carry-on baggage, the prohibitions regarding various seemingly non-dangerous items such as nail clippers, and the overall greater scrutiny all reduce the convenience of air travel. Survey data support the claim that increased security has made flying less convenient; 63 percent of travelers said that airport security "is becoming more of a hassle" (Woodyard and De Lollis 2003). Airlines claim that the increased inconvenience caused by security measures has cost them billions in lost ticket revenues, as potential business travelers opt to stay home (Sharkey 2002).

On the other hand, passengers likely value increased security. Several surveys conducted since 9/11 have found that passengers are willing to accept some additional inconvenience and/or higher prices in order to feel more secure (Travelocity 2002; University of Nebraska at Omaha 2003). Moreover, these surveys support TSA claims that the security measures implemented since 9/11 increase passengers' confidence in the safety of air travel (Compart 2004; University of Nebraska at Omaha 2003). Increased confidence in airline security may result in increased demand for air travel.

Looking specifically at baggage screening, the ex ante effects are ambiguous. By increasing passenger safety, baggage screening increases demand for air travel. However, by requiring additional time and effort on the part of passengers, and by increasing the likelihood of theft and/or destruction of baggage, baggage screening reduces convenience.

Moreover, the effects of baggage screening may vary with airport size. On one hand, high passenger counts may create congestion in baggage screening areas at large airports. Moreover, there have been widespread reports that baggage-screening capacity has not been able to keep up with the flow of baggage at many large airports (Higgins 2005). On the other hand, large airports may have more flexibility and resources to minimize passenger disruptions. We analyze the nation's fifty busiest airports separately to determine which effect is greater.

The impact of TSA passenger screening is also ambiguous ex ante. On one hand, the TSA screening is more rigorous than the process it replaced. These more rigorous procedures increase the time and inconvenience of passing through passenger security checkpoints (Seidenstat 2004). On the other hand, by enhancing screener quality and increasing staffing levels, TSA may have reduced the inconvenience of screening. In addition, there is some evidence that federalizing airport security may have increased travelers' confidence in that security. A Zogby poll, conducted in April 2004 found that 59 percent of respondents felt safer with federal security screeners, rather than private ones (American Federation of Government Employees 2004). More generally, as noted above, the results of several surveys indicate that travelers feel safer as a result of TSA's more stringent security procedures (Compart 2004; University of Nebraska at Omaha 2003).[3]

[3]An additional complicating factor in predicting the impact of the federalization of passenger screening is that private security firms were aware, in advance, of the date when TSA screeners would replace them. Recognizing that their contracts were facing imminent cancellation, private firms may not have invested in efficiency improvements near the end of their tenure. Instead, it seems likely that there would be a shortage of security staff, demoralized and unmotivated employees, and reduced monitoring of employees in the weeks before the handover. As a result, there might be positive benefits from TSA screening simply because of the deterioration of security procedures prior to the TSA takeover.

4 Data

We use quarterly data on passenger volume and ticket prices from the U.S. Department of Transportation's (DOT) Data Bank 1B (DB1B) of the Origin and Destination (O&D) survey. This survey covers a sample of every tenth ticket (tickets with a number ending in zero) sold by airlines for flights originating and terminating in the U.S. and includes the full itinerary for each trip, the price of the ticket, as well as the carrier (see the data appendix for a more detailed description of our dataset and how we constructed it).

We consider the time period from 1999 to 2003, placing the triggering event for our study, the terrorist attacks of 9/11, at about the midpoint of our data. Although we could have included data prior to 1999, earlier observations are unlikely to provide additional useful information for assessing the impact of security changes since 9/11.

We conduct the demand analysis at the carrier-segment level. As indicated above, a segment refers to direct service from an originating airport to a destination airport. For example, a passenger who flies round-trip from LaGuardia (LGA) to Los Angeles (LAX) with a connection in Chicago O'Hare (ORD) would fly four segments: (1) LGA-ORD; (2) ORD-LAX; (3) LAX-ORD; and (4) ORD-LGA. We estimate the demand models at the carrier-segment to allow us to distinguish between connecting and originating passengers, an important aspect of our identification strategy, which we discuss below.

We use TSA announcements of the date that TSA employees began screening checked baggage and began screening passengers at each airport to create indicator variables for security changes. In the case of passenger screening, we believe that the TSA announcements provide a precise indication of when each airport switched to federal screening. However, for the baggage screening variable, there is some uncertainty.

On November 19, 2002, the date on which it met the Congressional mandate to have TSA passenger screening in all 429 U.S. commercial airports, TSA provided a final progress report. This report lists the date that TSA began screening passengers and checked baggage at each airport. At that point, there were more than 150 airports that had not yet begun screening baggage. Because, in most cases TSA did not issue additional announcements, we do not know the exact date at which these airports began screening baggage. However, we do know that, with a small number of exceptions, all airports were screening baggage by the end of December 2002. We assume that these remaining airports began screening baggage on January 1, 2003. Stories in the print and electronic media at that time indicate that the vast majority of remaining airports did not begin screening until the very last days of December 2002, or the first day of 2003 (CNN 2002a,b). To illustrate the spread of TSA security changes, Table 1 provides the dates that each of the fifty busiest airports (using 2003 enplanements data from the DOT T-100 dataset) first began using TSA passenger screening and first began screening checked baggage.

—Insert table 1 here—

5 Identification Strategy and Estimation

5.1 Identification Strategy

To estimate the effect of the security measures on the demand for air travel, one would ideally compare passenger volume on flights with the new security procedures to the counterfactual: passenger volume on the same flights without those procedures. Although such a comparison is impossible, we exploit certain features of the airline industry to construct a very close counterfactual. Our identification comes from two factors: (1) TSA's phased introduction of security measures across different airports, which created a quasi-natural experiment and (2) the fact that only originating passengers, but not connecting passengers, should be affected by the security measures.

Empirically, we first estimate a difference-in-difference model that exploits TSA's phased introduction of the new procedures by comparing the change in the originating passenger volume on carrier-segments departing from airports adopting the new security procedures to the change in originating passenger volume on carrier-segments departing from airports that did not adopt the new security measures. Year fixed effects account for any national change in passenger volume that may have occurred at the same time as security measures were adopted, and carrier-segment fixed effects account for any time-invariant differences between airports, carriers, and/or segments. If adoption of security measures is randomly assigned, this method will produce unbiased estimates of the effect of security measures.

To illustrate our approach, consider Atlanta's Hartsfield International Airport (ATL) and New York City's John F. Kennedy Airport (JFK) from the third to fourth quarter of 2002. JFK adopted baggage screening during the fourth quarter of 2002, while ATL did not. To assess the impact of baggage screening on flights departing from JFK, the difference-in-difference estimator calculates the following:[4]

$$(OPV_{JFK,4Q2002} - OPV_{JFK,3Q2002}) - (OPV_{ATL,4Q2002} - OPV_{ATL,3Q2002}) \qquad (1)$$

[4] To be more precise, the difference-in-difference estimator subtracts the average passenger volume for all quarters before the introduction of the new security measure from the average passenger volume for all quarters after the introduction. Because the effect is identified only during the period where the treatment group has implemented the new security measure and the control group has not, we only include this period in describing the estimator.

where $OPV_{airport,quarter}$ is the originating passenger volume on carrier i's flights on segment s from the respective airports during each quarter.

A concern with this method is that we cannot separate changes in passenger volume due to security measures from changes due to airport-specific demand shocks occurring at the same time. For example, if JFK improved passenger amenities, added a runway, or made other alterations during the fourth quarter of 2002, our difference-in-difference method could falsely attribute the effect of those changes to the implementation of baggage screening.

To consider whether these factors are biasing our results, we much examine changes in the demand of a control group of passengers unaffected by security measures, but influenced by any airport-specific shocks occurring at the same airport, at the same time. Fortunately, the airline industry's hub and spoke system creates a natural control group. Connecting passengers do not undergo security when changing planes. However, these passengers are affected by unobserved changes in airport conditions.[5]

Because security measures would not affect connecting passengers, any observed relationship between security measures and connecting passenger volume would indicate a correlation between security adoption and airport demand shocks. However, the absence of a relationship between airport security measures and connecting passenger volume would allow us to exclude unobserved airport shocks as an alternative explanation for any observed relationship between security measures and originating passenger volume.

To separate changes in passenger volume due to security measures from changes due to airport-specific demand shocks, we estimate the same difference-in-difference

[5]Connecting passengers, of course, are not affected by changes at airports outside of the secure area. We cannot test for simultaneity between security measures and airport changes such as the addition of new parking or improvements in ground transportation to city centers.

in expression (1) for the control group of connecting passengers:

$$(CPV_{JFK,4Q2002} - CPV_{JFK,3Q2002}) - (CPV_{ATL,4Q2002} - CPV_{ATL,3Q2002}) \qquad (2)$$

where $CPV_{airport,quarter}$ is connecting passenger volume on carrier i's flights on segment s from the respective airports during each quarter.

To use this method to control for demand shocks, connecting passenger volume must be uncorrelated with the introduction of TSA security measures. However, connecting passengers must go through security at the feeder cities from which they originate their travel. To the extent that the introduction of security measures at these feeder airports is correlated with their introduction at the hub airports, the control group of connecting passengers becomes more similar to the treatment group of originating passengers. This potential contamination of the control group creates a conservative bias that understates the difference in the changes in demand of the two groups.[6]

An additional concern with using connecting passengers as a control group is that airlines may have changed their networks. Specifically, airlines may have eliminated segments as demand fell, causing an increase in the share of connecting passengers. If airlines eliminated segments in response to the implementation of security measures, we might incorrectly attribute a decrease in originating passengers without a decrease in connecting passengers to the introduction of the security measure rather than to

[6]To see this, consider two extremes: (1) the timing of feeder cities' security adoption is perfectly correlated with that of the connecting cities, or (2) the timing of feeder cities' security changes has no correlation at all with that of the connecting cities. In case (1), the implementation of security measures at the connecting airport would coincide with their implementation at the originating airport. In this situation, one would expect our supposed control group passengers' demand to fall in tandem with that of the treatment group, an occurrence that would obscure our findings. In case (2), our control group would be comprised of many passengers who have traveled without passing through the new security procedures. In either extreme case, and anywhere in between, our control group of connecting passengers contains some passengers who are affected by the new security measures.

the network changes.[7]

To consider this possibility, we look for for systematic changes in the size of airline networks that might affect our results. Figure 1 plots the number of carrier-segments over time. The figure shows that the number fell from 14,287 in the first quarter of 1999 to 13,466 in the first quarter of 2003. However, this decline occurred before the introduction of the new security measures. The number of carrier-segments remained fairly steady and even slightly increased in 2002 and 2003 when the security changes occurred.[8] Moreover, the percentage of passengers making a connection does not increase during the time of our study. Therefore, changes in the airline network do not seem to preclude the use of connecting passengers as an effective control group.

—Insert figure 1 here—

A related concern is the effect of security conditions at the destination airport. Our identification assumes that passengers' travel decisions are based on the security procedures at their originating airport, about which they have the most information, but one could argue that round-trip travelers also consider the security procedures at the destination airport, which they must pass through on their return. To confirm that destination airport security procedures do not influence our demand estimates, we repeat the analysis with only the population of return travelers.

Finally, if airlines changed prices or flight frequency in response to the changes in security, this could affect our results. For example, airlines might lower prices to restore passenger volume lost because of the greater inconvenience of travel. Alternatively, airlines might reduce flight frequencies, thus exacerbating the negative effect of TSA security on demand. To examine these possibilities, we test for the relationship

[7] We thank the referee for raising this issue.

[8] Regressing the number of destinations a carrier serves from an airport on security measure implementation also shows no significant relationship between the two.

between security arrangements and airfares and flight frequency.

5.2 Estimation

Our main empirical approach uses the difference-in-difference identification shown in expression 1 by estimating the following reduced form, fixed-effects models:

$$\begin{aligned} \ln PASSENGERS_{isq} = &\beta_1 BAGGAGE\_SCREENING_{sq} + \\ &\beta_2 PASSENGER\_SCREENING_{sq} + \\ &QUARTER_q + CARRIER\_SEGMENT_{is} + \epsilon_{isq} \end{aligned} \tag{3}$$

$PASSENGERS_{isq}$ is the number of passengers separately defined for originating and connecting passengers flying on airline i, on segment s, during quarter q. $BAGGAGE\_SCREENING_{sq}$ is an indicator of whether TSA had begun screening checked baggage of originating passengers flying on segment s (route r), during quarter q. Since in-line EDS baggage screening is performed outside of the passenger's presence, the indicator for the seven airports using this system is always coded zero. $PASSENGER\_SCREENING_{sq}$ indicates whether TSA had taken over the passenger screening function at the originating airport. If passenger or baggage screening began during a quarter, we coded the indicator variable with the portion of the quarter during which the security measure was in effect. For example, if the security measure was implemented one month before the quarter ended, we coded the indicator as 0.33. In each model, the next two terms are the chronological quarter, e.g., Q1 2002, and carrier-segment fixed effects. The quarter fixed effects control for changes in the airline industry during the study period. For example, these fixed effects control for changes in economic conditions, weather, technology, industry-wide labor relations, congestion, attitudes toward flying, seasonal demand, as well as many of the other changes in security procedures that were implemented simultaneously at most or all airports, including arriving early to the airport, having to remove footwear, etc. The carrier-segment fixed effects control for systematic differences in demand across airlines' segments.

One might expect correlation in the ϵ_{isq} error terms belonging to carrier-segments departing from the same airport. To correct for this correlation, we estimate standard errors clustered at the originating airport in all of our regressions.

Table 2 displays descriptive statistics for originating and connecting passengers. When looking at the number of passengers, it is important to recall that the O&D survey is a sample of every tenth ticket, which suggests that the actual mean number of originating (connecting) passengers on a carrier-segment during a quarter was about 3,749 (2,984). This number averages a distribution skewed by a small number of high-traffic carrier-segments. The combination of the low volume of passengers on many carrier-segments and the O&D's ten percent sampling rule lowers the precision of our estimates. To minimize the noise due to sampling in low volume carrier-segments, we repeat all of our analyses dropping the carrier-segments in the bottom 25% percentile by passenger volume. In most cases, this sample selection, which we refer to as the restricted sample, lowers the magnitude of the standard errors but has little effect on the estimated coefficients.

—Insert table 2 here—

Figures 2 shows the trends in each of the outcome variables during the five-year period included in the sample. The impact of the 9/11 terrorist attacks is clear; passenger volumes plummeted following September 2001. Because of the drastic effect of the attacks, we exclude the third quarter of 2001 from all of our regressions.[9]

[9]We also tried excluding the fourth quarter of 2001, rather than the third. Our results do not change.

—Insert figure 2 here—

6 Results

6.1 Effects of Security on Demand

To examine the impact of the new security measures on the number of passengers traveling by air, we first estimate difference-in-difference models which assess whether the introduction of TSA passenger screening or baggage screening affected demand in our treatment group: originating passengers. The results of these baseline models are reported in table 3.

—Insert table 3 here—

The difference-in-difference analysis shows that baggage screening is associated with about a six percent decline in originating passenger volume, an effect which is statistically significant, while TSA passenger screening has no effect on originating passenger volume. We also examine the impact of these security procedures only at the fifty busiest airports in the U.S. In each year, these airports handle more than 80 percent of total passenger volume. At these larger airports we find that the negative effect of baggage screening is greater. Baggage screening reduces the number of originating passengers traveling from large airports by over nine percent. Again, TSA passenger screening has no effect on originating passenger volume. It appears that baggage screening makes travel less attractive, while TSA passenger screening either has no effect on travelers, or that any increased inconvenience resulting from TSA screening is offset by enhanced efficiency and greater public confidence in the security of the aviation system.

In the difference-in-difference models, the fixed effects control for many unobserved factors that might be correlated with baggage or passenger screening and passenger volume. However, as discussed above, this identification strategy does not control for unobserved, airport-specific demand shocks that might occur at the same time and at the same airports as the changes in security occur. For example, TSA may have purposely introduced baggage screening at airports during periods of low traffic so as to minimize inconvenience while procedures were learned. In this case, the negative effect of baggage screening would be endogenous.

To ensure that unobserved airport-specific demand shocks are not biasing our estimates, we examine the impact of security procedures on connecting passenger volume flying on the same carrier, leaving from the same airport, for the same destination, during the same quarter. These connecting passengers provide a natural control group because passengers do not pass through security when changing planes, and they have their baggage screened only at the originating airport.

The results for the sample of connecting passengers are reported in table 4. Neither baggage screening nor TSA passenger screening has any impact on connecting passenger volume. These results provide strong evidence that the changes in security procedures are indeed causing the changes in originating passenger volume. It does not appear that unobserved airport demand shocks are biasing our estimates.[10]

—Insert table 4 here—

As a further check of airport shocks as an alternative explanation, we estimate the effect of security measures on originating passengers while conditioning on connecting passenger volume. Again, we use connecting passengers to control for any unobserved

[10]We also estimated a difference-in-difference-in-difference model by pooling the originating and connecting passenger samples and interacting the security variables with an indicator for observations from the originating passenger sample. The results for the baggage screening variable are consistent with, but stronger, than what we describe above.

airport-specific shocks. As with our baseline results, the negative effect of baggage screening is greater at the top 50 airports than at all airports. The effect of passenger screening is statistically insignificant in all models. These results, displayed in table 5, provide further evidence that the negative effect of baggage screening is not biased by unobserved airport-specific factors.

—Insert table 5 here—

Another question is whether passengers flying round-trip consider the security procedures at destination airports, from which they will depart later, when choosing whether and where to fly. To examine this possibility, we estimate our model again, this time including only passengers on the first flight of their return trip.

The results for returning passengers are reported in table 6. As with connecting passengers, we find that neither baggage screening nor TSA passenger screening has any effect on returning passengers in any of our models.[11] These results suggest that security procedures at airports other than the originating airport have little impact on travel, and that baggage screening at the originating airport is driving the estimates we report above.[12] This finding is consistent with the reasonable assumption that passengers are the most familiar with the security situation at their hometown airport.

—Insert table 6 here—

Finding that baggage screening reduces passenger volume raises the question of what the deterred passengers do instead: Do they travel less, or do they substitute a

[11]An alternative approach to removing possible bias from destination airport security is to use a sample of only one-way passengers. Doing so yields results similar to those for round-trip passengers, suggesting that destination airport security has little effect on demand.

[12]In a further attempt to isolate the impact of baggage screening at the originating airport, we also tried excluding flights to airports that had implemented baggage screening. The effect of baggage screening on demand is stronger in this analysis: baggage screening reduces originating passenger volume by seven percent at all airports and by ten percent at the fifty busiest airports.

different mode of transportation for flying? To shed light on this question, we restrict our sample to trips of less than 500 miles (roundtrips of less than 1000 miles).[13] We expect that passengers are more likely to substitute travel by car, bus, or train for flying on shorter trips, although this effect may be dampened or countered by the tendency of short-distance passengers to check less luggage. The results are reported in table 7.

—Insert table 7 here—

The salient finding is for short trips from the top 50 airports the sample of itineraries for which substitutes are the most available and airports for which the effect of baggage screening was greatest. The number of originating passengers falls by about 16 percent. The more negative coefficient supports the view that travelers are substituting other forms of transportation for flying on shorter routes.

Taken together, the above results suggest that the introduction of baggage screening at U.S. airports created inconvenience that reduced demand for air travel by about six percent, with even greater reductions in demand for flights originating at large airports and shorter flights. On the other hand, the federalization of passenger screening had little effect on passenger volume.

6.2 Effects of Price on Demand

Several alternative explanations could be responsible for the negative impact of baggage screening on passenger volume. An alternative supply-side explanation is that airlines incurred higher security costs after TSA began managing airport security. Prior to 9/11, airlines and airports paid directly for most security costs. Instead, TSA now assesses each airline a monthly security fee based on the amount it paid for

[13] That is, we exclude all observations for passengers flying on *routes* that are under 500 miles.

passenger screening in 2000 (Seidenstat 2004). In addition, Congress passed a new excise tax, the security service tax, of \$2.50 for each enplanement on flights originating at domestic airports.[14] If airlines pass these fees and taxes on to passengers in the form of higher ticket prices, then this would provide an alternative explanation for the negative relationship between baggage screening and passenger volume. However, we think the taxes and fees are unlikely to affect our results. Our identification exploits time-series variation in the implementation of security measures, whereas the TSA fees and the excise tax were applied to all flights and airports simultaneously. So, whatever their effect, the fees and taxes did not selectively affect some airports and not others and therefore should not influence our findings.

An alternative demand-side explanation is that by increasing passengers' confidence levels, the security procedures may actually have raised the demand for air travel. If airlines boosted prices in response to an upward shift in demand, then we might observe a price-induced decline in air travel from airports that screen baggage.

To consider each of these alternative explanations, we examine the impact of the security variables on airline ticket prices by estimating the following model:

$$\begin{aligned}\ln PRICE_{irq} =& \beta_1 BAGGAGE\_SCREENING_{rq} + \\ & \beta_2 PASSENGER\_SCREENING_{rq} + \\ & QUARTER_q + CARRIER\_ROUTE_{ir} + \epsilon_{irq}\end{aligned} \tag{4}$$

Consistent with prior studies, we estimate price models at the carrier-route level, where a route refers to the originating and destination airport for each leg of the trip. Because the O&D survey reports airfares at the route level, this higher level of aggregation avoids arbitrary apportionments of multi-segment fares to the segment

[14] There is a \$5 limit in each direction, i.e., a \$10 limit for round-trips.

level. Referring to the earlier example, the roundtrip from LGA to LAX (with a plane change in ORD) would comprise two routes: LGA-LAX and LAX-LGA. $PRICE_{irq}$ is half of the average round-trip fare paid by passengers flying on airline i, on route r, during quarter q. The results are presented in table 8.

—Insert table 8 here—

The results show that airlines increased fares by about two percent following the introduction of TSA passenger screening. However, baggage screening had no effect on fares, providing no support for the hypothesis that higher prices introduced a negative bias in the estimated relationship between baggage screening and passenger volume.

6.3 Effects of Flight Frequency on Demand

A second alternative demand-side explanation is that airlines reduced flight frequency, either in response to lower demand caused by the new security measures or in response to temporary operational constraints during the new security implementation. A reduction in flight frequency might have in turn caused (or exacerbated) the decline in air traffic volume. To test this possibility, we use monthly flight-level passenger data from the T-100 Domestic Segment Database to assess the effects of baggage and passenger screening on the number of monthly departures by a carrier on a segment. We estimate the following model:

$$\begin{aligned}\ln DEPARTURES_{ism} =& \beta_1 BAGGAGE\_SCREENING_{sm} + \\ & \beta_2 PASSENGER\_SCREENING_{sm} + \\ & MONTH_m + CARRIER\_ROUTE_{is} + \epsilon_{ism}\end{aligned} \tag{5}$$

$DEPARTURES_{ism}$ is the number of flights offered by carrier i, on segment s, during month m. The results are presented in table 9. Neither baggage screening nor passenger screening has a significant effect on the number of departures. If baggage screening reduces demand, it might appear puzzling that flight frequency does not decline after it introduction. Some possible explanations are the costs of changing schedules, especially in a networked system, as well as competitive pressures.[15] In any event, changes in the frequency of service do not appear to explain the decline in demand.

—Insert table 9 here—

7 Plausibility of Results

To see if the additional travel time caused by baggage screening could plausibly lead to the demand decline we estimate, we compare our results with those that would be predicted by prior studies' estimates of the airline industry's elasticity of demand.[16] Consider the outbound leg of the median round-trip in our data, which is 1,215 miles, has 2 segments, and has a price (half of the round-trip fare) of $180. The flying time for a two-segment 1,215-mile journey, say Jacksonville to Oklahoma City, with one stopover in Dallas, is about 4 hours 10 minutes (after averaging the flight times in both directions).

A Spring 2001 survey (Resource Systems Group, Inc. 2001) indicates that travelers arrived at the airport an average of 82 minutes before their scheduled departure. While no numbers are available on the time spent in exiting the destination airport after arrival, the survey indicates that 68.5 percent of travelers checked bags. There-

[15]Airlines typically set schedules as far as 330 days in advance (Chao 2006)
[16]We thank the referee for suggesting this analysis.

fore, assuming that the egress time is 15 minutes for those who do not check bags and 30 minutes for those who do check bags, the average egress time is about 25 minutes. This yields a total travel time of about 6 hours.

To calculate the time cost of travel, we used survey results (Resource Systems Group, Inc. 2003) from 2002-2003 that estimated business travelers' willingness to pay for a one-hour reduction in travel time to be \$37 for business flyers and \$17 for others. The 2001 National Household Travel Survey shows that about 40% of air travelers are business travelers. Using this proportion, one arrives at a weighted average time cost of about \$25 per hour, making the time cost of a median trip about \$150. Including the ticket price, the total cost of the trip is \$330.

We were unable to obtain measures of how much baggage screening procedures add to travel time. Taking an estimate of 30 minutes, the total cost of the trip increased by \$12.50, or 3.8 percent. Demand price elasticity estimates vary from -2.5 (Brueckner and Spiller 1994) to -1.6 (see Brander and Zhang (1990) for a review of these calculations). Taking the lowest number in this range of -1.6, the drop in demand should be about 6%. If baggage screening added 15 minutes to the travel time, the drop in demand should be half as great. If we use an intermediate value of -2.0 for the elasticity, then the expected drop in demand would be 7.6 percent (3.8 percent), assuming a 30 (15) minute increase in travel time. Although these calculations are only back-of-the-envelope approximations, it is reassuring that they predict a decline in demand of similar magnitude to the 6 percent decline that we have estimated.

8 Implications for Welfare and Transportation Safety

Taken together, we believe our results provide convincing evidence that the introduction of baggage screening reduced the demand for air travel. This finding has implications for both welfare and transportation safety. Trips not taken because of airport security constitute a reduction in consumer surplus. On the producer side, empty seats on airplanes impose a large cost in terms of reduced profits in the airline industry. Moreover, the federal government receives less tax revenue from ticket sales. Transportation safety is also compromised if the inconvenience of airport security prompts some travelers to substitute less-safe road transportation for air travel. To quantify some of these implications, we have done "back of the envelope" calculations for the effect of baggage screening on airline industry revenues and highway fatalities.

To measure the decline in airline revenues, we multiply the demand reduction estimated above by the average ticket price. Using the number of 4th quarter 2002 passengers as a base, the 6.3 percent decline in demand we estimated in our first regression indicates a realized reduction of 3.2 million trips. At the average round-trip ticket price in our data of $408, this resulted in a loss of approximately $1.3 billion of revenue for 4th quarter of 2002. Of this total, roughly $1.1 billion is lost airline industry revenue, with the remainder comprising various airline ticket taxes. In comparison, the Air Transport Association estimates that the lost revenues from the 9/11 attacks in the 4th quarter of 2001 were about $10.1 billion (General Accounting Office 2001). Because the airline industry is characterized by high fixed cost and low marginal costs, most of this lost revenue translates to lost profits in the short run. Compared to the annual domestic airline industry revenue of approximately $60 billion, our very rough estimate shows the lost revenue due to security inconvenience to be far from trivial. Of course, one must also consider the cost of baggage screening itself, which totaled \$1.5 billion in 2003 (Subcommittee on Aviation 2004). More generally, the total cost of TSA operations in its first two years (2002 and 2003) was about \$11.5 billion (Seidenstat 2004).

If the inconvenience of security deters travelers from flying, some might choose to travel by automobile instead. Consistent with our finding that the negative effect of baggage screening is greatest for trips of less than 500 miles, we expect this substitution would be especially likely on short trips, for which driving is most feasible. Because air transportation is safer than road transportation, the increase in driving could lead to more traveler fatalities. In fact, we show in a separate paper that the substitution of road for air transportation following 9/11 led to an increase in driving fatalities (Blalock, Kadiyali, and Simon 2005). As part of that analysis, we estimated the reduced-form relationship between air passenger volume and driving deaths. Using commercial vehicle fatalities to control for time trends, weather patterns, economics conditions, and unobserved highway conditions, we found that a decrease of one million enplanements leads to an increase of fifteen driving fatalities. Applying that relationship to the estimated reduction in originating passenger volume due to baggage screening, we estimate that in the 4th quarter of 2002 approximately 129 individuals died in automobile accidents which resulted from travelers substituting driving for flying in response to inconvenience associated with baggage screening.[17]

Although both our revenue and fatalities estimates are very rough approximations, the numbers are of an order of magnitude that warrant attention. These costs must

[17] To calculate the number of fatalities we multiply the estimated number of trips not taken due to baggage screening, 3.2 million, by the number of enplanements associated with each trip (2.7). This then gives us the total reduction in enplanements associated with baggage screening. Finally, we multiply this figure by the number of additional fatalities resulting from a decrease of one million enplanements, 15, to arrive at our figure of 129 additional fatalities.

be weighed against the difficult-to-measure benefits of better security.

9 Conclusion

This paper assesses the impact of post-9/11 airport security measures on demand for air travel. We find that the introduction of baggage screening at U.S. airports reduced originating passenger volume at all airports by about six percent and by about nine percent at the nation's fifty busiest airports. In contrast, baggage screening had no effect on connecting passengers, who do not need to have their baggage screened. This provides support for the view that the negative relationship between baggage screening and passenger volume is not driven by unobservable airport demand shocks. In addition, we find evidence that baggage screening reduced demand by even more on shorter flights. The federalization of passenger screening had little effect on passenger volume.

Although we find evidence that baggage screening has reduced the demand for air travel, we recognize that any assessment of the net benefits of this procedure must balance this loss in consumer welfare against the difficult-to-measure increase in the security of air travel. Nonetheless, we believe that it is important to identify these unintended consequences of regulatory efforts to enhance security against the threats of terrorism. In addition to the lost consumer welfare, rough estimates for the 4th quarter of 2002 alone suggest that baggage screening cost the airlines over a billion dollars in lost revenue and substitution from air to road travel resulted in over 100 driving fatalities. These high costs lend evidence to the argument presented in Mueller (2004) that the greatest cost of terrorism may be the unintended consequence of responses to attacks rather than the attacks themselves.

Finally, it is important to note some limitations in our analysis. First, our measures of TSA passenger and baggage screening introduction dates contain some error, with more error likely in the baggage screening measure. While our results are fairly robust to alternative specifications of these variables, this nonetheless poses a concern. In addition, our ability to precisely estimate the impact of these security changes on the demand for air travel is hampered by the fact that the O&D survey is quarterly. This poses a challenge given the short time span during which baggage screening was introduced throughout U.S. commercial airports. In particular, the short span prevents us from measuring the persistence of the demand decline. We would expect demand to slowly return to pre-intervention levels as the TSA and airports invest in infrastructure to minimize the inconvenience of baggage screening. However, absent a counterfactual of airports without baggage screening, we cannot measure effects on demand beyond the first quarter of 2003.

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A Data Appendix

Our principle dataset is the Department of Transportation's (DOT) Data Bank 1B (DB1B) of the Origin and Destination (O&D) survey. Because the DB1B is a large and complex dataset, we detail some of the procedures we used to prepare the observations for our study below.

1. We first merged DB1B Ticket quarterly files with DB1B Coupon quarterly files. Doing so allowed us to attach itinerary information to each segment of the trip. In particular, merging allowed us to calculate the distance of each trip (for the whole itinerary) and subsequently separate short from long trips.

2. We dropped all observations for itineraries with more than two markets on the itinerary. These are unusual itineraries with more than two legs to the trip. I.e., city A to city B, city B to city C, and then city C to city A.

3. To identify originating flights, we select flights for which the *seqnum* variable was encoded to one. *Seqnum* is the coupon sequence number indicating the order in which the coupons will be used and thus corresponds to the order in which segments will be traveled.

4. To identify connecting flights, used all flights with *seqnum* not equal to one except for the first return flight (for roundtrips).

5. We identified the first return flight as the first flight from the second market (i.e., the first flight on the inbound portion of the round trip).

6. The carrier is the ticketing carrier, i.e., the carrier that sells the ticket. In some instances, a carrier may sell seats on another carrier (even without a code-sharing agreement) as part of a multi-carrier itinerary. Because the majority of seats are typically issued by the operating carrier, this practice creates some small-volume carrier-segments which we have removed in some estimations as noted in the tables.

7. A segment is an origin-destination airport pair for a coupon. A coupon indicates direct service between two airports. All coupons with the same origin and destination correspond to the same segment. Some of these coupons may reflect direct service with a stop, while most of them will be nonstop. However, if passengers must change planes, then there will be a separate coupon. For example, all coupons with ORD as the origin airport and LAX as the destination airport are the ORD-LAX segment even though some of the flights may stop in DIA. However, passengers that fly from ORD to LAX, but change planes in DIA will have two coupons (ORD-DIA and DIA-LAX). The practice of some passengers staying aboard a plane that makes many hops can also create some small carrier-segments because most passengers will choose non-stop of single-connection service for those city pairs.

8. In order to identify short trips, we use the number of miles flown on the itinerary. For the case of roundtrips, we divide by two. Therefore, the analysis reported in Table 7 only includes roundtrip itineraries with less than 1,000 miles of flying distance and one-way trips with less than 500 miles of flying distance.

9. A small percentage of itineraries show an airfare of zero, likely due to frequent-flier point redemption or other forms of non-revenue travel. We remove these from our price analysis. In addition, we exclude all trips with fares greater than $10,000. Finally, the DB1B Ticket dataset contains an indicator for fares with a revenue-per-mile ratio far outside of industry norms. We drop all fares which are flagged as questionable.

10. The prices in the estimation of equation 4 are round-trip fares only. Including one-way intinerary fares does not substantively affect our results.

B Tables

Table 1: **TSA announcement dates for baggage screening and passenger screening at the 50 busiest U.S. airports.** ** indicates that the date was not announced and that baggage screening began sometime after November 19, 2002, but before January 1, 2003.

Airport	TSA Passenger Screeners	TSA Baggage Screeners
Atlanta (ATL)	9/17/2002	1/1/2003**
Chicago O'Hare (ORD)	8/6/2002	9/17/2002
Dallas-Fort Worth (DFW)	9/4/2002	9/10/2002
Los Angeles (LAX)	10/8/2002	10/29/2002
Phoenix (PHX)	9/24/2002	1/1/2003**
Denver (DEN)	9/10/2002	1/1/2003**
Las Vegas (LAS)	9/17/2002	1/1/2003**
Minneapolis-St. Paul (MSP)	9/10/2002	1/1/2003**
Detroit (DTW)	8/27/2002	10/29/2002
Houston Intercontinental (IAH)	9/17/2002	10/16/2002
Orlando (MCO)	7/30/2002	9/30/2002
Seattle (SEA)	9/17/2002	10/1/2002
San Francisco (SFO)	11/19/2002	1/1/2003**
Newark (EWR)	8/13/2002	10/29/2002
Charlotte (CLT)	9/4/2002	1/1/2003**
New York Laguardia (LGA)	8/6/2002	9/24/2002
Philadelphia (PHL)	9/10/2002	1/1/2003**
St. Louis (STL)	9/17/2002	1/1/2003**
Cincinnati (CVG)	9/4/2002	1/1/2003**
Baltimore-Washington (BWI)	4/30/2002	10/21/2002

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Airport	TSA Passenger Screeners	TSA Baggage Screeners
Boston (BOS)	8/6/2002	9/24/2002
Salt Lake City (SLC)	9/17/2002	10/1/2002
Chicago Midway (MDW)	9/4/2002	9/17/2002
New York JFK (JFK)	7/9/2002	10/29/2002
Fort Lauderdale (FLL)	9/10/2002	1/1/2003**
Tampa (TPA)	8/6/2002	1/1/2003**
San Diego (SAN)	10/16/2002	1/1/2003**
Honolulu (HNL)	10/1/2002	1/1/2003**
Miami (MIA)	10/1/2002	9/24/2002
Pittsburgh (PIT)	8/27/2002	1/1/2003**
Washington National (DCA)	9/17/2002	9/24/2002
Oakland (OAK)	10/8/2002	1/1/2003**
Washington Dulles (IAD)	10/8/2002	1/1/2003**
Portland, OR (PDX)	10/8/2002	10/22/2002
Memphis (MEM)	10/1/2002	1/1/2003**
Kansas City (MCI)	11/19/2002	1/1/2003**
San Jose (SJC)	10/1/2002	1/1/2003**
Cleveland (CLE)	8/6/2002	1/1/2003**
New Orleans (MSY)	10/16/2002	1/1/2003**
Nashville (BNA)	8/27/2002	1/1/2003**
Sacramento (SMF)	10/1/2002	1/1/2003**
Orange County, CA (SNA)	10/17/2002	1/1/2003**
Houston Hobby (HOU)	9/4/2002	1/1/2003**
Raleigh-Durham (RDU)	8/13/2002	1/1/2003**
SJU (San Juan, Puerto Rico)	10/1/2002	1/1/2003**
Indianapolis (IND)	9/17/2002	1/1/2003**
Austin (AUS)	9/4/2002	1/1/2003**

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Airport	TSA Passenger Screeners	TSA Baggage Screeners
Albuquerque (ABQ)	10/8/2002	1/1/2003**
San Antonio (SAT)	9/10/2002	1/1/2003**
Ontario, CA (ONT)	10/8/2002	1/1/2003**

Table 2: **Descriptive statistics for passenger volumes.** The unit of observation is carrier-segment-quarter. Because the O&D survey samples every tenth ticket, passenger volume should be multiplied by 10 to reflect the true number of travelers. All numbers exclude the third quarter of 2001.

Variable	Mean	Std. Dev	No. Observations
Originating passengers	374.91	(851.58)	293,489
Connecting passengers	298.46	(819.54)	261,268

Table 3: **The effect of security measures on originating passenger volume.** Carrier-segments below the 25th percentile are removed in some models as indicated. Carrier-segment and year-quarter fixed effects are included but not reported. The third quarter of 2001 is excluded.

	all airports		top 50 airports	
TSA baggage screening	−0.063	−0.063	−0.095	−0.091
	(0.025)**	(0.021)***	(0.024)***	(0.023)***
TSA passenger screening	−0.004	−0.012	−0.013	−0.021
	(0.022)	(0.018)	(0.028)	(0.024)
Adj. R^2	0.91	0.93	0.91	0.93
Carrier-segments below 25th percentile	Y	N	Y	N
No. carrier-segment observations	278,777	204,966	177,109	134,715

* Significant at 0.10; ** Significant at 0.05; *** Significant at 0.01

Table 4: **The effect of security measures on connecting passenger volume.** Carrier-segments below the 25th percentile are removed in some models as indicated. Carrier-segment and year-quarter fixed effects are included but not reported. The third quarter of 2001 is excluded.

	all airports		top 50 airports	
TSA baggage screening	0.046	0.026	0.038	0.010
	(0.044)	(0.039)	(0.062)	(0.053)
TSA passenger screening	−0.002	0.014	0.006	0.020
	(0.040)	(0.035)	(0.056)	(0.051)
Adj. R^2	0.92	0.93	0.92	0.93
Carrier-segments below 25th percentile	Y	N	Y	N
No. carrier-segment observations	248,228	172,004	177,854	125,499

* Significant at 0.10; ** Significant at 0.05; *** Significant at 0.01

Table 5: **The effect of security measures on originating passenger volume, conditional on connecting passenger volume.** Carrier-segments below the 25th percentile are removed in some models as indicated. Carrier-segment and year-quarter fixed effects are included but not reported. The third quarter of 2001 is excluded.

Dep. variable: passengers	all airports		top 50 airports	
TSA baggage screening	−0.081	−0.085	−0.112	−0.111
	(0.029)***	(0.026)***	(0.034)***	(0.031)***
TSA passenger screening	0.008	−0.010	−0.018	−0.022
	(0.023)	(0.017)	(0.027)	(0.023)
log(connecting passengers)	0.579	0.466	0.618	0.501
	(0.016)***	(0.013)***	(0.018)***	(0.015)***
Adj. R^2	0.94	0.95	0.94	0.95
Carrier-segments below 25th percentile	Y	N	Y	N
No. carrier-segment observations	186,838	167,584	127,904	114,806

* Significant at 0.10; ** Significant at 0.05; *** Significant at 0.01

Table 6: **The effect of security measures on returning passenger volume.** Carrier-segments below the 25th percentile are removed in some models as indicated. Carrier-segment and year-quarter fixed effects are included but not reported. The third quarter of 2001 is excluded.

Dep. variable: passengers	all airports		top 50 airports	
TSA baggage screening	−0.026	−0.021	−0.037	−0.013
	(0.032)	(0.031)	(0.036)	(0.036)
TSA passenger screening	−0.011	−0.019	−0.019	−0.012
	(0.023)	(0.022)	(0.031)	(0.030)
Adj. R^2	0.91	0.93	0.91	0.93
Carrier-segments below 25th percentile	Y	N	Y	N
No. carrier-segment observations	274,876	195,639	248,228	130,681

* Significant at 0.10; ** Significant at 0.05; *** Significant at 0.01

Table 7: **The effect of security measures on originating passenger volume for passengers traveling less than 500 miles.** Carrier-segments below the 25th percentile are removed in some models as indicated. Carrier-segment and year-quarter fixed effects are included but not reported. The third quarter of 2001 is excluded.

Dep. variable: passengers	all airports		top 50 airports	
TSA baggage screening	−0.116	−0.101	−0.157	−0.142
	(0.036)***	(0.033)***	(0.049)***	(0.042)***
TSA passenger screening	−0.035	−0.056	0.015	−0.014
	(0.040)	(0.033)*	(0.064)	(0.056)
Adj. R^2	0.93	0.93	0.93	0.94
Carrier-segments below 25th percentile	Y	N	Y	N
No. carrier-segment observations	79,112	58,186	44,306	33,138

* Significant at 0.10; ** Significant at 0.05; *** Significant at 0.01

Table 8: **The effect of security measures on round-trip ticket prices.** Carrier-route and year-quarter fixed effects are included but not reported. The third quarter of 2001 is excluded.

Dep. variable: ticket prices ($)	all airports	top 50 airports
TSA baggage screening	−0.004	0.006
	(0.011)	(0.016)
TSA passenger screening	0.020***	0.019**
	(0.007)	(0.008)
Adj. R^2	0.42	0.47
Carrier-routes below 25th percentile	Y	Y
No. carrier-route observations	2,480,034	968,516

* Significant at 0.10; ** Significant at 0.05; *** Significant at 0.01

Table 9: **The effect of security measures on flight frequency.** Carrier-segment and year-quarter fixed effects are included but not reported. The third quarter of 2001 is excluded.

Dep. variable: flights per day	all airports	top 50 airports
TSA baggage screening	0.010	0.014
	(0.016)	(0.020)
TSA passenger screening	0.012	0.017
	(0.013)	(0.013)
Adj. R^2	0.93	0.92
Carrier-segments below 25th percentile	Y	Y
No. carrier-segment observations	438,691	276,786

* Significant at 0.10; ** Significant at 0.05; *** Significant at 0.01

C Figures

Figure 1: **Carrier-segments by quarter.**

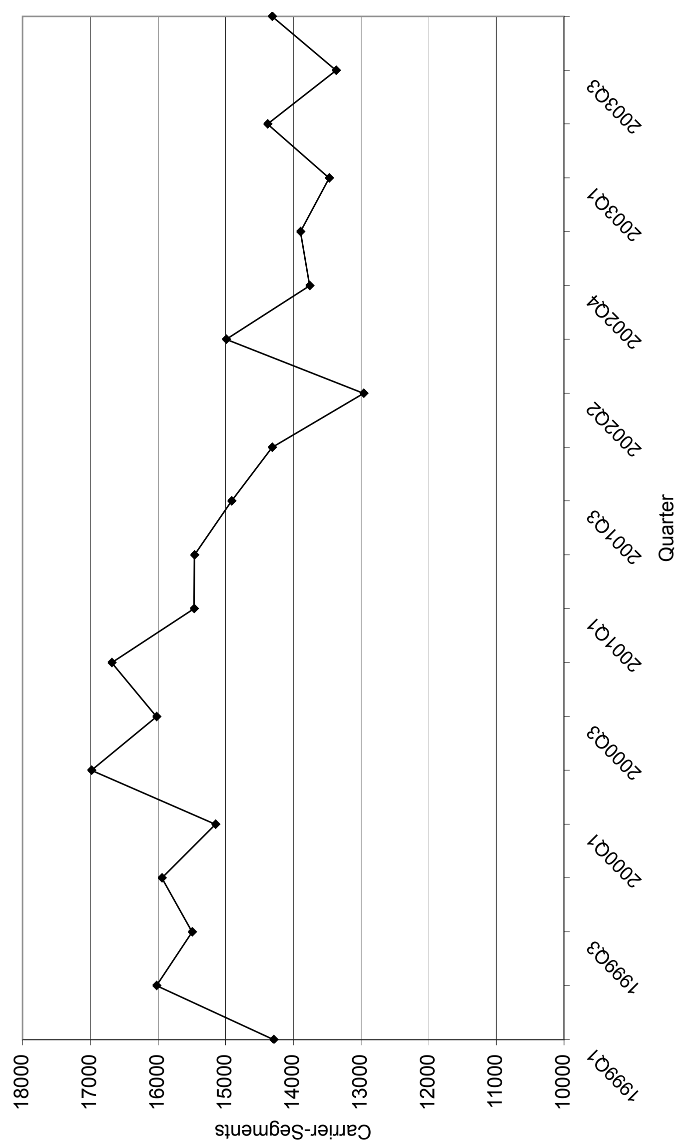

Figure 2: **Mean passenger volume on a carrier-segment by quarter.** Because the O&D survey samples every tenth ticket, passenger volume should be multiplied by 10 to reflect the true number of travelers.