

CHAPTER 8: TRAJECTORY ANALYSIS

This chapter describes the first of two major analyses of child outcomes, comparing results for children in the original demonstration counties with those in the original comparison group. The analysis examines child safety in terms of child welfare service trajectory, i.e. the way cases progress through the child welfare system from the initial point of investigation.

As is described in more detail below, Ohio’s Title IV-E Waiver allows participating counties to pay for non-placement services with funding that was formerly reserved for placement services, provided there are real reductions in out-of-home care. The motivation for this change was the assumption that some of the children who were placed in care could, in fact, be more appropriately served in home if funding for those services was available. The primary challenge in shifting resources to in-home services is that the risk of maltreatment may increase among children who otherwise might have been placed in out-of-home care. In other words, the successful implementation of the waiver would be evidenced by an increase in in-home service utilization and lower placement utilization, without a commensurate increase in the risk of maltreatment.

To examine these issues, we compare patterns of service utilization for cohorts of children experiencing an initial maltreatment investigation during fiscal years 1994 through 2006. Specifically, the study team used state child welfare administrative data to show when a given child was the subject of his or her first maltreatment investigation. Then, using these same data, we examine patterns of service utilization and subsequent maltreatment investigation over a period of time that includes both ProtectOhio Waiver periods.

The presentation is in two parts. In the first part, we outline the theory of the waiver and the projected impact on service decisions made by workers, under the assumption that the waiver creates an incentive for counties to develop and use non-placement services. In the second part, we describe the results of multiple analyses. The discussion starts with a simple count of initial maltreatment investigations across waiver groups and fiscal years. We then examine three groups of children who are differentiated from one another based on their service experience during the first six months following the date of an initial investigation: children who are not served (i.e. the case is closed in intake), children who are initially served in-home, and children who are placed.¹ For each group of children, we examine their subsequent service history (e.g., investigation, placement) in order to determine whether there were increased safety risks associated with the demonstration counties relative to the comparison counties during the waiver period(s).

¹ Note that the definition of service utilization used in this chapter, which is based on case status and placement, differs somewhat from those used in prior chapters, which have been based on the receipt of a particular type of service (e.g., mental health and substance abuse services or kinship support services).

8.1 THE THEORY OF THE WAIVER, SERVICE AVAILABILITY AND WORKER DECISIONS

The Title IV-E Waiver program seeks to change the fundamental dynamic of child welfare service interventions. Under standard Title IV-E funding, providers of out-of-home care are reimbursed for each day of care they provide. The mechanism is a classic per diem system – provide a day of care and reimbursement follows. Specific details of the federal/state, state/local, and local/private sector relationships are important, but the essence of the system is largely the same. Revenue for a large part of the system depends on having children in out-of-home care.

The problem with the per diem system is that it is self-perpetuating. First and foremost, it often leads to an over reliance on the services funded in that way. Moreover, once investments in out-of-home care have been made, it is hard to back out of those investments because categorical restrictions on the use of placement dollars prohibit shifting the resources to service alternatives. Among other effects, the per diem system tends to finance inefficiency in that, absent other controls, an inefficient system is reimbursed in the same way that an efficient system is. Having the money tied up in placement means that it is very difficult to bring non-placement service alternatives on line. As a consequence, placement becomes supply-driven, used because the beds exist and other services do not.

The federal Title IV-E Waiver offers an opportunity to alter this core dynamic. Theoretically, by making placement dollars fungible (i.e., usable on services other than out-of-home care), favorable changes in the utilization of placement will occur, and will result in the reinvestment of unused placement dollars in those services that allow counties to forego placement in the first instance.

The theory of the waiver hinges on the availability of clinically viable alternatives to out-of-home care. Under the pre-waiver system, workers were taught to make a placement decision because service alternatives did not exist. With the waiver, the child welfare system increases the supply of services so that placement can be avoided. At this point, the question of clinically viable alternatives enters the picture. At one level, the assumption behind the waiver is that the out-of-home care system is fundamentally inefficient – more out-of-home care is provided (too many admissions or too much time is spent in placement) than the needs of children dictate. In other words, the safety of children can be preserved short of placing them in out-of-home care but for the absence of an alternative. The assumption rests on whether the alternatives assure safety commensurate with what would have been true if out-of-home care had been used in the amount used previously. If the same outcomes (or better) can be achieved and out-of-home placement is reduced, then the plan works.

From the perspective of the worker who must decide on a course of services for a child, the non-waiver situation offers limited choices. A child becomes known to the system and a service decision has to be made. Because the supply of non-placement services is limited, the likelihood of being served in home is lower than it would otherwise be. Over a group of 100 children who are referred for services, hypothetically speaking, the worker may elect to place 16 children because six really need placement and 10 need protection, but the necessary level of protection cannot be provided within the available supply of non-placement services. Rather than expose a child to the risk, the worker elects to over-serve the child by placing him or her in out-of-home care. The child is protected from the risk of maltreatment but the service level exceeds what is necessary.

With the waiver program in place, the under-supply of in-home services can be remedied. New capacity makes it possible to serve more children in-home. The worker may make several different choices. Ideally, in-home services are allocated to the 10 children whose needs can be addressed in the context of their home. Alternatively, the worker may allocate the in-home services to some but not all of the 10 children who could have been served at home. If this happens, the system becomes only somewhat more efficient. Or, the worker could elect to offer in-home services to some of the 10 children whose needs can be met with in-home services and some of the six children who really need placement. The worker may do this because he/she believes that the in-home services are equally effective, or perhaps he/she misses the diagnosis – a false positive on the ability of in-home services to meet the safety needs of the child. If this happens, the risk to the child increases because the service is not matched to the need. The impact on the child is an increased risk of maltreatment given that the service is not adequate. The data will not tell us why maltreatment followed the course of action taken – the use of ineffective services or a missed diagnosis – only that the course of action did not produce the desired result.

8.2 METHODOLOGY

The sample for these analyses includes children 13 years old and younger who experienced an initial maltreatment investigation during fiscal years 1994 through 2006 ($N = 286,485$).^{2, 3, 4, 5} Data were obtained from FACSIS, which contain records of service utilization and maltreatment investigations through fiscal year 2006. FACSIS data were used in lieu of SACWIS data because, as noted above, the study team found inconsistencies in how non-placement service events (i.e., in-home case openings and closings) are captured in the Ohio's SACWIS system.⁶

The focus of the analyses described below is on the impact that waiver-induced changes in service utilization may have had on child safety. To examine this issue, we conduct four sets of analysis. First, we compare the percentage of children in the demonstration and comparison counties for whom a subsequent maltreatment investigation occurred within 1 year of the date of their initial investigation, regardless of any services that may have been provided. This comparison informs our general understanding of whether there were any differences in safety between the two county groups as a whole.

We then consider three different groups of children differentiated from one another based on their service experiences. First, we identify a group of children we call **not served**. Children in this group are children for whom neither an in-home nor a placement case was opened within 6 months of an initial substantiated maltreatment investigation; we limit our sample to children whose first or second

² An important focus of these analyses is the potential impact on child safety of the expected increase in in-home service utilization among demonstration counties. Because older adolescents are at a lower risk of a subsequent maltreatment investigation than younger children, they were excluded from the sample.

³ A maltreatment investigation was indicated by FACSIS Event 100.

⁴ Because of inconsistencies in how non-placement service events (i.e., in-home case openings and closings) for children in Hamilton county are captured in the FACSIS data, these children are excluded from these analyses

⁵ Based on federal fiscal years.

⁶ See Chapter 1, section 1.2.2.1.

investigations were substantiated.⁷ For this group of children, we examine whether a subsequent investigation occurred within 18 months of the initial investigation.

Second, we identify a group of children we refer to as **served at home**. Children in this group had an in-home services case opening that followed their initial maltreatment investigation. To define this group of children, we limit our sample to children with in-home case openings that occur within 6 months of the date of the initial investigation. Further, in order to differentiate this group of children from those whose in-home service case opening was associated with a subsequent placement, we exclude from this set of analyses children who were placed within 2 weeks of the date of their in-home services case opening. For this analysis, we examine the percentage of children who were subject to a subsequent maltreatment investigation, and the percentage placed within 12 months of the in-home services case opening.

Finally, we identify children who were placed within 6 months of an initial maltreatment investigation in order to examine **post-placement** safety. Specifically, we are interested in whether, within the group of children discharged from care after short placements, the likelihood of maltreatment increased. The analysis includes two steps. First we assess the time to discharge from care by calculating the percentage of children exiting care earlier than the pre-waiver (1994 – 1997) median length of stay (91 days); larger percentages exiting earlier than the pre-waiver median indicate shorter lengths of stay, while smaller percentages indicate longer lengths of stay.. Then, to assess whether changing lengths of stay affected child safety, we examine the percentages of children subject to a subsequent maltreatment investigation among those children who exited care earlier than the pre-waiver median.

For all of the analyses described below, the between-demonstration/comparison county differences, relative to the timing of the two waiver periods, are used to assess the impact of the waiver.

8.3 FINDINGS

This section presents the five sets of findings: first, a simple count of initial maltreatment investigations, as a context for what follows; second, a comparison of the percentage of children in demonstration and comparison counties for whom a subsequent maltreatment investigation occurred within 1 year of the date of their initial investigation; and then three separate analyses of the likelihood of subsequent maltreatment investigations among the three groups of children -- children who are not served, children who are initially served in home, and children who are placed.⁸

8.3.1 Children's Initial Maltreatment Investigation

Between fiscal years 1994 and 2006, approximately 286,500 children were the subject of an initial maltreatment investigation across the two county groups. During the pre-waiver period (1994-1997), the number of children with initial investigations declined in both demonstration and comparison counties (Chart 8.1 and Table 8.1). During the first waiver period (1998-2002), the two county groups

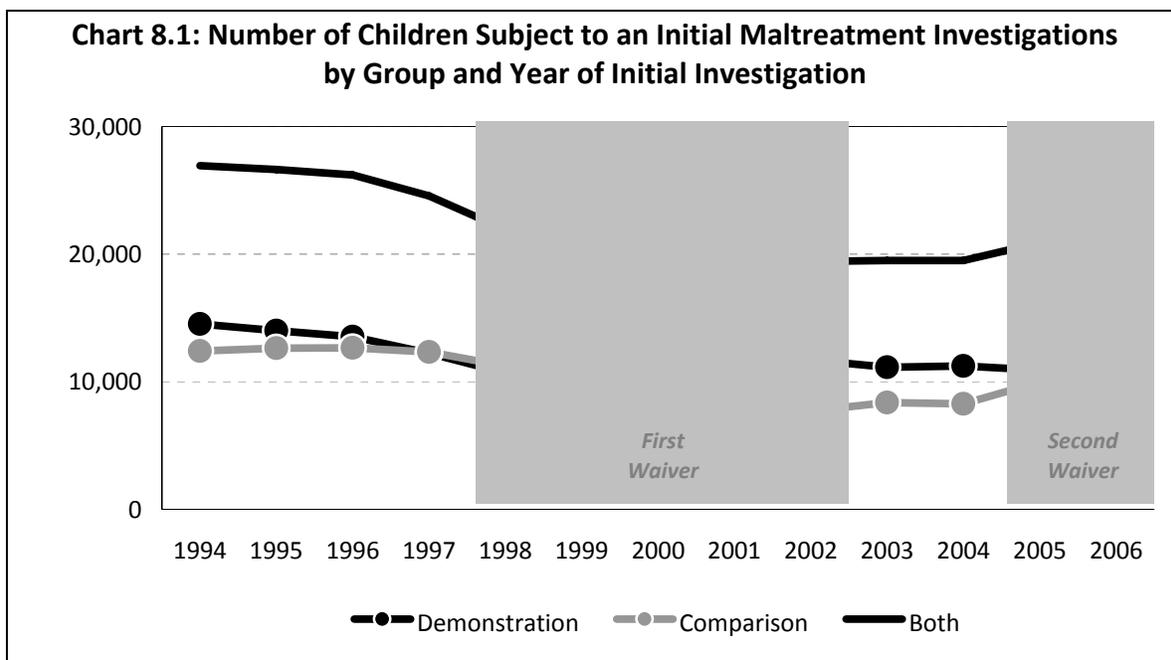
⁷ Case opening and placement are, respectively, indicated by FACSIS events 172 and 060.

⁸ For the children in the 'not served' group, the 6-month eligibility window was initiated on the date of the first substantiated investigation. See Section 8.2 Methodology for further details.

diverged: the number of children with initial investigations remained steady in demonstration counties, while continuing to decline in comparison counties. Starting at the end of the first waiver period (2001/02), the number of investigations in the comparison counties began a gradual increase, which continued through fiscal year 2006, and brought the comparison counties back to a level that was comparable to the demonstration counties.

Table 8.1: Number of Children with Initial Maltreatment Investigations by County Group and Fiscal Year (in thousands)

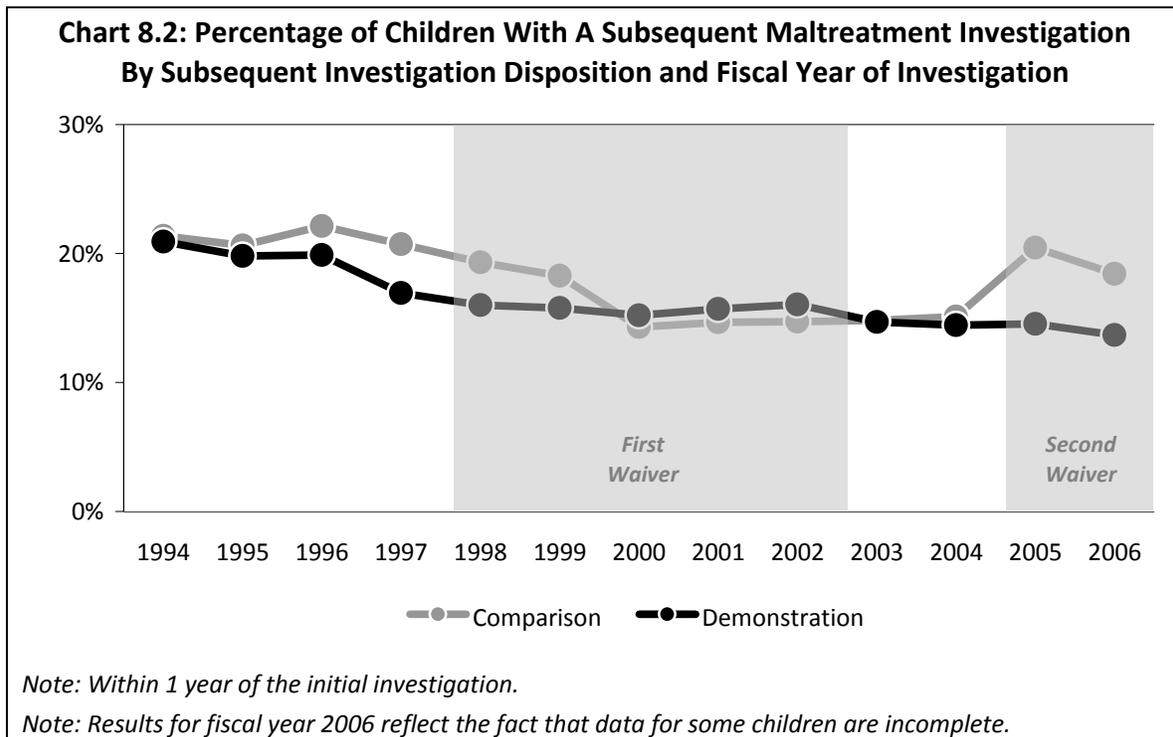
Fiscal Year	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	Total
Comparison	12.4	12.6	12.7	12.3	11.2	10.0	8.2	7.1	7.7	8.4	8.3	10.0	10.9	131.8
Demonstration	14.5	14.0	13.5	12.2	10.7	11.1	11.3	11.7	11.7	11.1	11.2	10.9	10.7	154.7
Total	26.9	26.6	26.2	24.6	21.9	21.1	19.5	18.8	19.4	19.5	19.5	20.9	21.6	286.5



8.3.2 Subsequent Maltreatment Investigation by Group and Fiscal Year

We next examine the incidence of a subsequent maltreatment investigation following an initial investigation. Through the middle of the first waiver period (fiscal year 2000), the percentage of children with a subsequent maltreatment investigation within 1 year of the date of an initial maltreatment investigation decreased among both demonstration and comparison counties (Chart 8.2). Between fiscal years 2000 and 2004, the percentage of children with a subsequent investigation remained flat in both groups. Finally, during the first year of the second waiver period, the percentage subject to a subsequent investigation increased somewhat among the comparison counties. When we limit our consideration to subsequent *substantiated* investigations (not shown), the same patterns hold true.

In general, the observed changes over time in the percentage of children with a subsequent maltreatment investigation do not suggest a significant relationship between the waiver and child safety. Since the beginning of the first waiver period (fiscal year 1998), there has been very little change among the demonstration counties, and among the comparison counties the changes that occurred appear to transcend the boundaries of the waiver period.



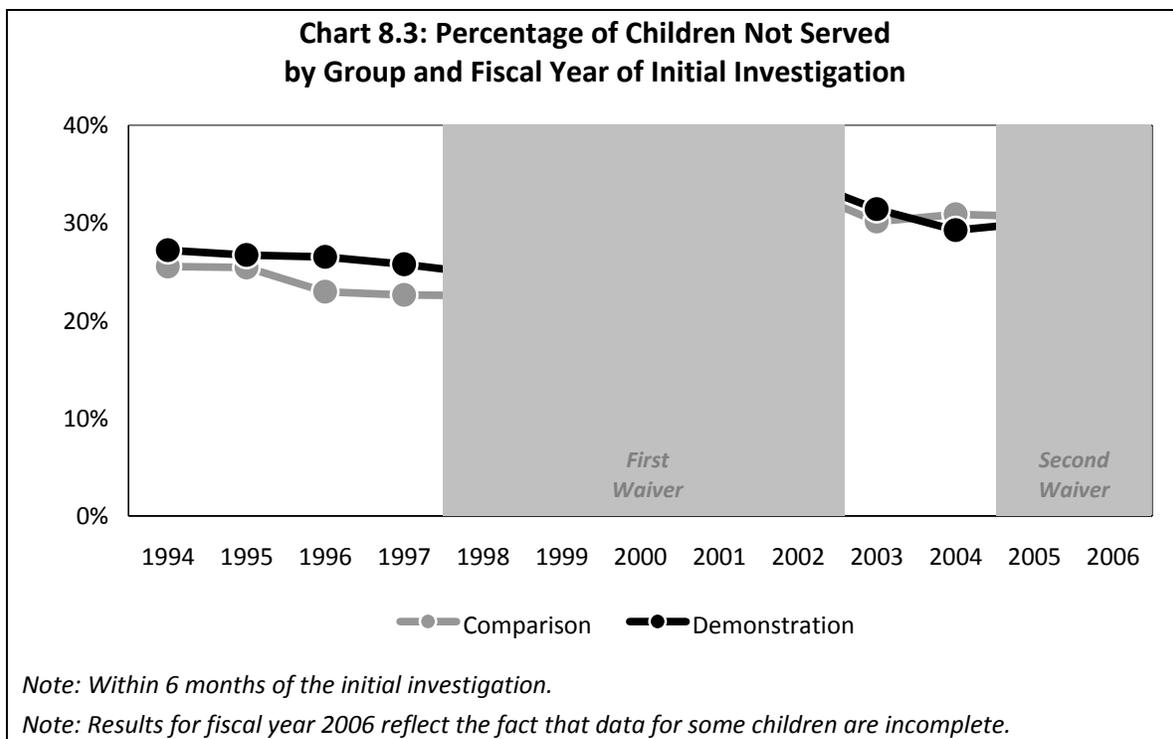
8.3.3 Children Not Served

Given the lack of relationship between waiver status and subsequent maltreatment shown above, the immediate question becomes: does child safety differ depending on how a child is served? In theory, the waiver provides county workers with an opportunity to change their service choices: children who would have been placed might now be served in home instead; children who are placed might now spend less time in care because post-permanency services are more readily accessible. As children (and their families) are directed to services thought to best meet their needs, the overriding concern with those choices is whether child safety is affected. In this section, and the ones that follow, we examine a subset of the pathways (trajectories) through the system to see whether we can detect changes in safety associated with those pathways. As noted above, we start with children **not served**. These are children for whom there is no service event within 6 months of their initial investigation. Because this is the largest group of children, we are interested in whether the decision to not serve is connected to an increase in maltreatment.

Chart 8.3 presents the percentage of children not served by group and fiscal year. Through the middle of the first waiver period (fiscal year 1999), the percentage of children who were not served

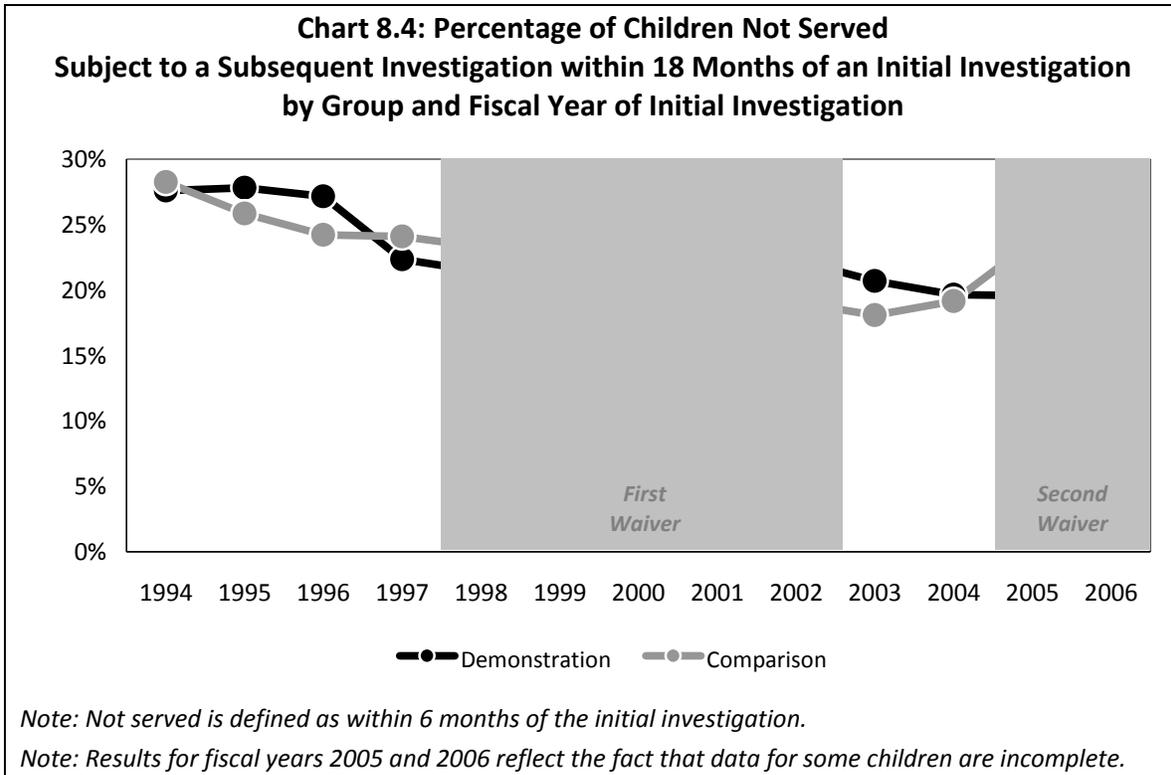
within 6 months of an initial investigation declined slightly among demonstration and comparison counties (Chart 8.3). During the middle of the first waiver period, the percentage of children not served increased by more than 50 % in both demonstration and comparison counties. The implication, of course, is that the percentage served declined by an equal amount during this period (not shown). Finally, starting in the period between the first and second waivers, the percentage of children not receiving services began to decrease once again.

The within-demonstration/comparison group changes, although substantial, do not suggest a relationship with the waiver, in large measure because the between-group patterns are strikingly similar. From 1999 forward, the unadjusted fraction of all children not served did not differ significantly. Moreover, any change in the demonstration county group was matched by a comparable change in the comparison counties.



Among children not served within 6 months of their initial investigation, we also examined the percentage subject to a subsequent investigation within 18 months of the date of the initial investigation. Over the entire observation period (1994-2006), there is a general downward trend in both demonstration and comparison counties, from just under 30 % of children with a re-investigation to below 20 % (Chart 8.4). That said, there is nonetheless an interesting pattern beginning in 2001 and going through 2006: maltreatment among the not-served children declined in the demonstration counties, whereas in the comparison counties, maltreatment among the not-served population was

trending upward.⁹ When we limit our view of the data to substantiated maltreatment (not shown), similar patterns are observed.



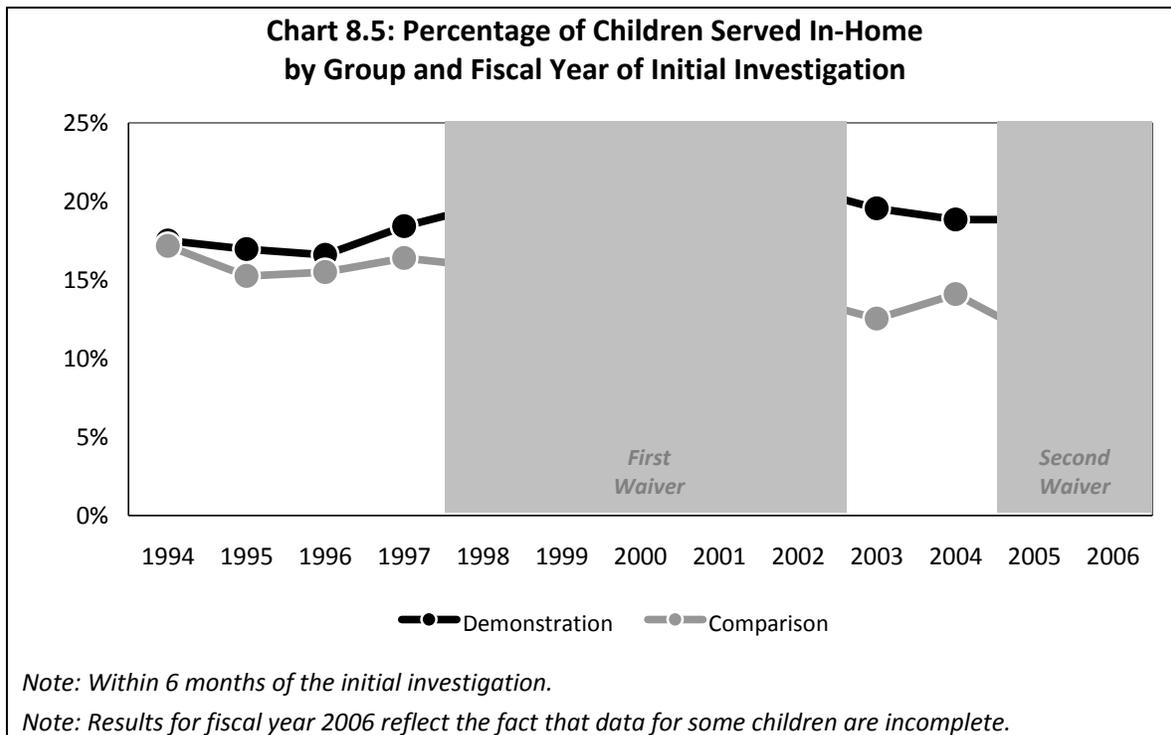
8.3.4 Children Served In Home

The opportunity to increase investments in ‘front-end’ services is thought to be one of the primary advantages of flexible funding strategies such as the Title IV-E Waiver program. Thus, in the context of the waiver, one would expect to see an increase in children served at home. From an evaluation perspective the question is whether rates of maltreatment increase among the served-at-home population. We examine this question in this section, starting with whether there was an increase in children served at home, as a fraction of initial maltreatment investigations.

The percentage of children served in home by demonstration/comparison counties and fiscal year is presented in Chart 8.5. These data show that, among demonstration counties, the percentage of children served in-home within 6 months of an initial investigation has remained fairly constant over time (Chart 8.5). In contrast, among comparison counties, the percentage served in-home gradually decreased. As a result, by fiscal year 2006, 18.7 % of children in the demonstration counties were served in-home compared with only 10.5 % in the comparison counties. Because the difference developed gradually over time, and is mostly a reflection of a reduction in the percentage served in-home within

⁹ For some children in the fiscal year 2005 cohort, and all children in the 2006 cohort, the observation time is less than 18 months, which means that not all subsequent investigations can be observed.

comparison counties, it is difficult to conclude that the waiver increased the likelihood that a child would be served in-home. However, one might conclude that the trends suggest that the waiver stimulus helped demonstration counties maintain historical levels in the midst of pressure to reduce in-home service availability.

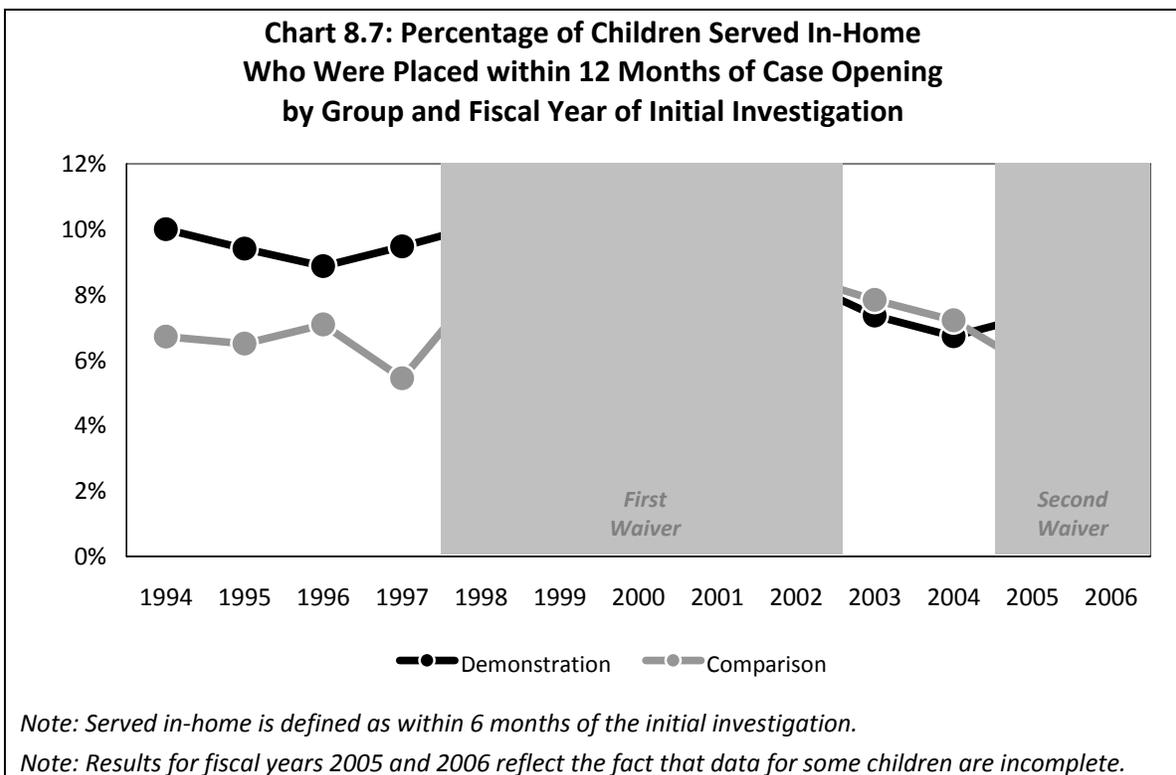
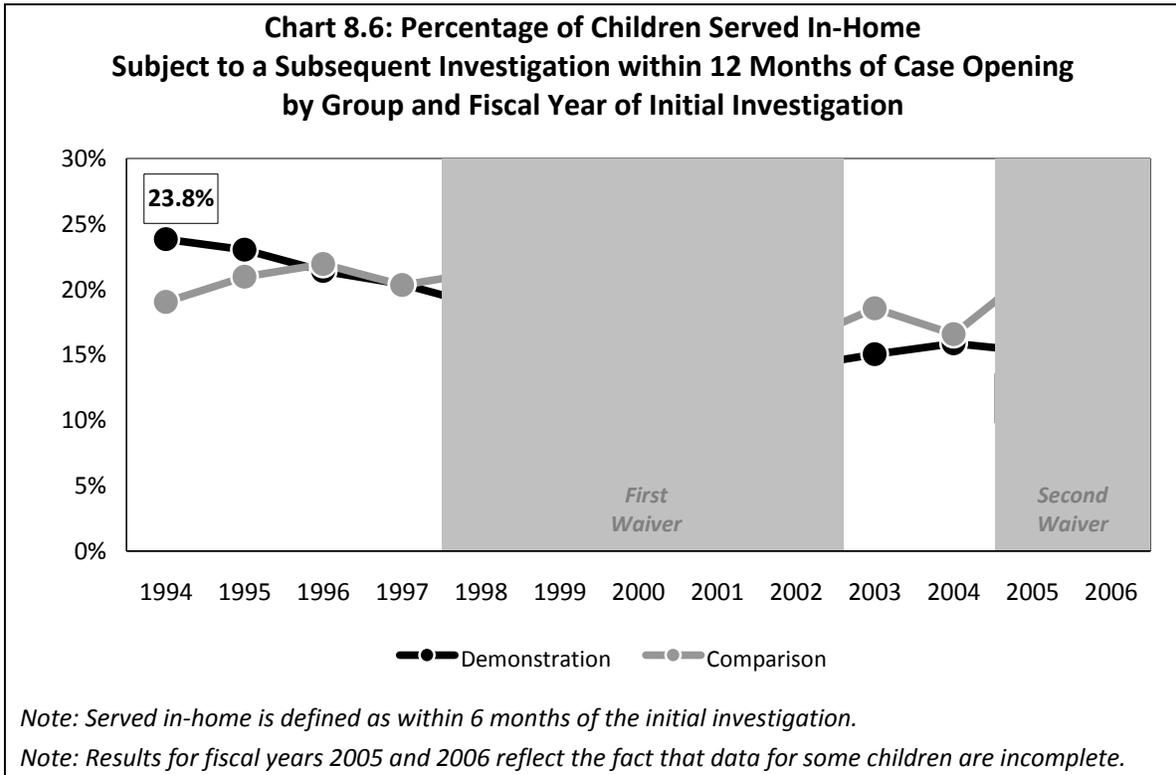


With respect to the safety of children served in home, we examined whether the in-home service choice was followed by either a subsequent maltreatment investigation or placement. These data are found in Chart 8.6. Among children served in home, the percentage of children with a subsequent maltreatment report appears to have decreased over time in both demonstration and comparison counties. The trend among demonstration counties is particularly clear, with the percentage of children served in-home with a subsequent investigation declining from 23.8 % during fiscal year 1994 to 15.2 % during fiscal year 2005.¹⁰ Gradual decreases in the percentages of children with *substantiated* investigations are also observed (not shown).

Another way to look at safety is whether a child served in-home is subsequently placed. Through the middle of the first waiver period, the percentage of children served in-home who were placed within 12 months of the date of the in-home case opening remained relatively flat among both demonstration and comparison counties (Chart 8.7); although the percentage also fluctuated somewhat among comparison counties. During this same period of time, the percentage of children served in-home who were placed was higher among demonstration counties than it was among comparison counties. By the end of the

¹⁰ For some children in the fiscal year 2005 cohort, and all children in the 2006 cohort, the observation time is less than 12 months, which means that not all subsequent investigations or placements can be observed.

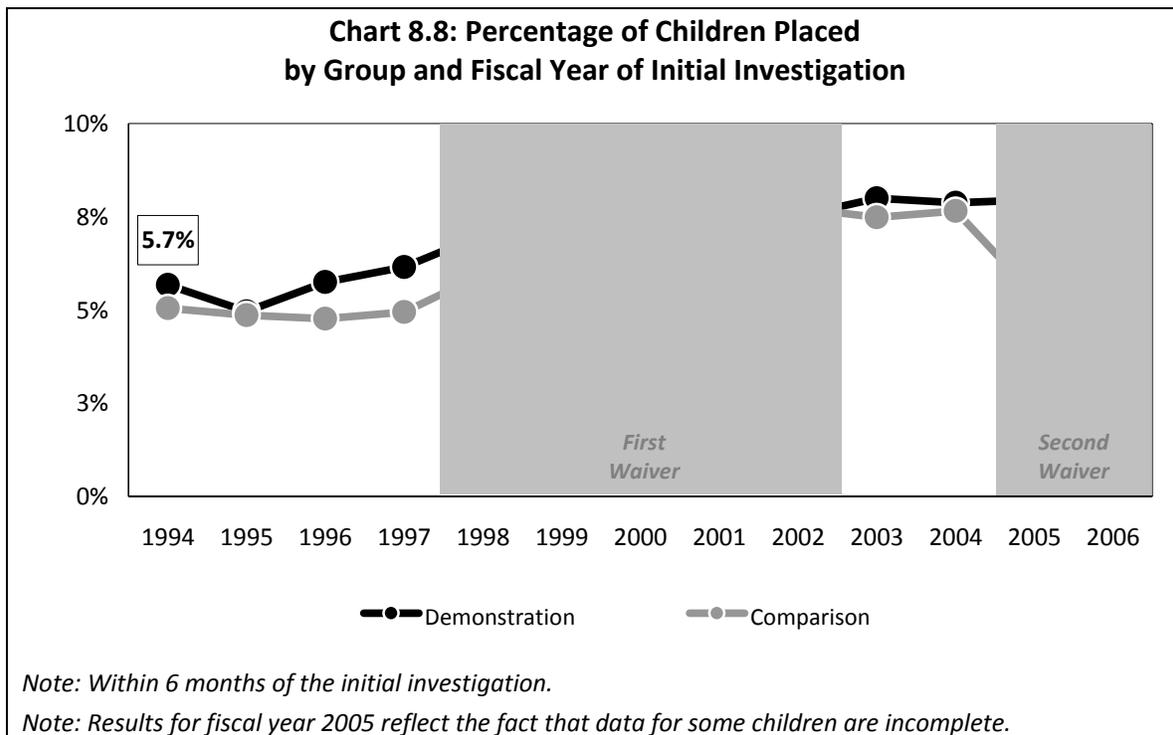
first waiver period (fiscal year 2002), however, these percentages had converged, and now appear to be trending downward.



8.3.5 Post-Placement Safety

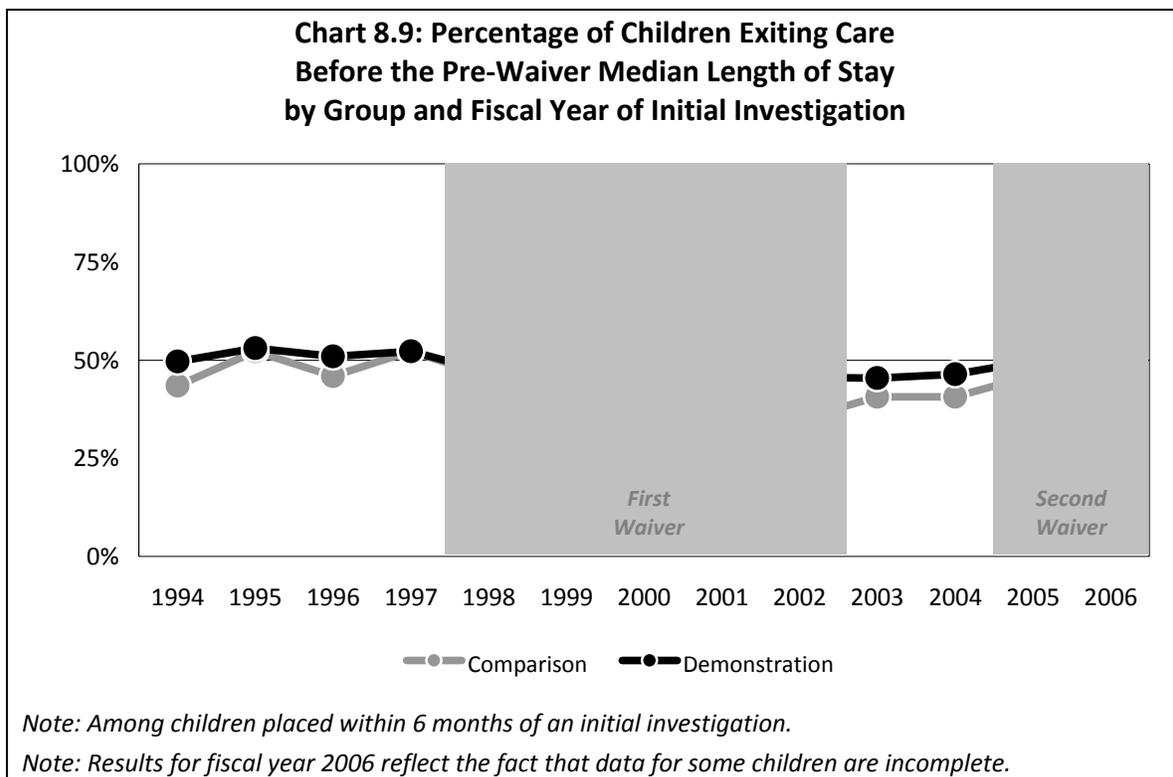
As discussed, the waiver is designed to affect the context in which service decisions are made – the likelihood of in-home services should rise, the likelihood of placement should fall, and placement duration should shrink – all without an increase in safety risks to children. In this section, we examine safety within the placement dynamic, with a particular emphasis on safety among children discharged from placement after what we call ‘short’ placements. Short placements, for these analyses, are placements that were shorter than the pre-waiver median duration. Children in this group tend to go home (reunification) or they are discharged to relatives. We are interested in whether post-discharge reports of maltreatment increased during the waiver period.

We start by looking at the placement decision: of children with an initial investigation, what fraction is placed? These data, which are presented in Chart 8.8, show that among demonstration counties, the percentage of children placed within 6 months of an initial investigation increased steadily from 1994 through the middle of the first waiver period (fiscal year 2000) (Chart 8.8). From 2000 through 2005, there was little change in the likelihood of placement following the initial investigation. In 2006, the second year of the second waiver, there was a relatively sharp drop in the likelihood of placement, but this may be due to the fact that the data are incomplete (i.e., there was too little time to observe all the placements). Within the comparison counties, the underlying trend is somewhat less stable. With the exception of 2001 through 2004, the likelihood of placement in the comparison counties was always slightly below what was observed in the demonstration counties.



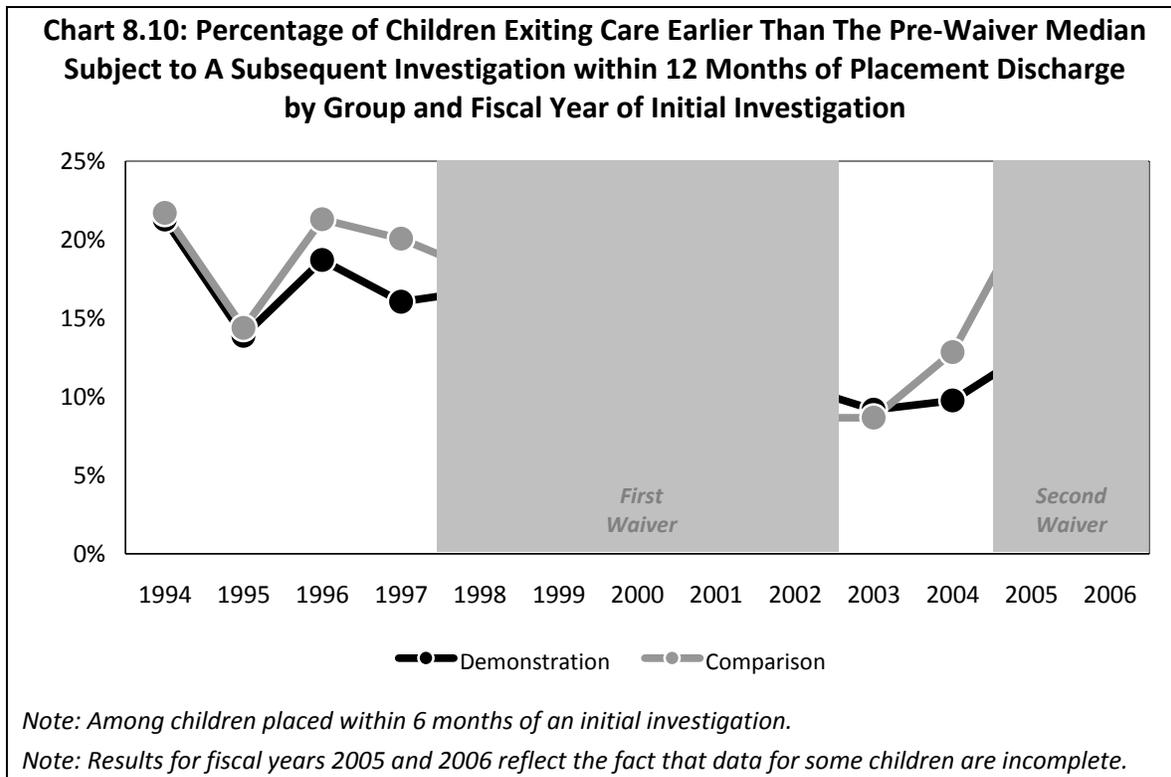
In order to assess whether the time to discharge from care has changed, we calculated the percentage of children exiting care earlier than the unadjusted median length of stay (91 days) during the pre-waiver period (1994 - 1997). Larger percentages exiting earlier than the pre-waiver median indicate shorter lengths of stay, while smaller percentages indicate longer lengths of stay (LOS).

During the first waiver period, the percentage of children exiting earlier than the pre-waiver median LOS (91 days) declined gradually among both demonstration and comparison counties (Chart 8.9). In other words, children's length of stay (unadjusted) appears to have gradually increased during this period. This change appears to have been more pronounced among comparison counties: during fiscal year 2002, only a third exited care earlier than the pre-waiver median. After the first waiver period, the percentage exiting earlier than the pre-waiver median length of stay appears to have increased gradually in both demonstration and comparison counties. In other words, length of stay appears to have decreased slightly, from the end of the first waiver period forward.



Finally, to assess whether changing lengths of stay affected child safety, we examined the percentages of children subject to a subsequent maltreatment investigation among those children who exited care earlier than the pre-waiver median. Among both demonstration and comparison groups, there appears to have been a substantial decrease in the percentages of children with a subsequent investigation during the pre-waiver, and part of the first waiver, periods (Chart 8.10). Then, after a

period of relative stability, the percentage of children with a subsequent investigation appears to increase during fiscal years 2004 and 2005.¹¹



8.4 SUMMARY

A primary question motivating this evaluation is whether child safety in the demonstration counties changed in the wake of the program changes brought about by the waiver stimulus. All told, the findings concerning the percentage of children experiencing a subsequent investigation do not appear to bear this out. Specifically, since the beginning of the first waiver period, there has been very little change in the percentage of children with a subsequent investigation among the demonstration counties that stand apart from changes observed in the comparison counties. It appears that safety has improved, but the same can be said of both demonstration and comparison counties.

Of particular note is that the higher relative likelihood of in-home service utilization among demonstration counties does not appear to be associated with a relative decrease in child safety among demonstration-county children. Specifically, there is little or no evidence that children served in home, or children discharged from care earlier than the pre-wavier median length of stay, were more likely to be the subject of a subsequent maltreatment investigation in demonstration counties than they were in comparison counties.

¹¹ For some children in the fiscal year 2005 cohort, and all children in the 2006 cohort, the observation time is less than 12 months post discharge, which means that not all subsequent investigations can be observed.