Smartphones and Child Injuries

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Abstract

From 2005 to 2012, injuries to children under five increased by 10%, possibly because smartphones distract caregivers from supervising children. I exploit the expansion of AT&T's 3G network in both a difference-in-differences and a triple difference framework and find that hospitals experienced a 5% increase in emergency department visits for children ages 0-5, but none for children ages 6-10, after getting 3G. Age-specific injury patterns on playgrounds, from poisoning, and in sports further support the conclusion that smartphones distract caregivers.

Keywords: smartphones, child injuries, distractions

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Nonfatal, unintentional injuries to children under five increased by 10% from 2006-07 to 2011-12 (CDC 2012). Table I, using data described below, shows that emergency department visits increased substantially for children under five but hardly at all for children five and older. This increase is puzzling because many investments over the past few years have gone towards improving child safety. This rapid increase in child injuries is a public health concern and worthy of policy consideration, but currently there is no understanding of what caused this sudden increase or what policy could address it.

Could the rapid adoption of smartphones explain this puzzle? In a *Wall Street Journal* article, Worthen (2012) advances the hypothesis, supported by many specialists, that smartphones distract parents from supervising their children, which increases the risk of injury. But he notes that no study has provided causal evidence linking smartphone use to child injuries. This paper is the first work towards establishing that causal evidence. Because smartphones—i.e. cell phones with the ability to browse the internet, stream videos, send and receive emails, and run various software applications—are a recent innovation that provide unprecedented access to information and distractions, our understanding of their impacts on caregiver-child relations are limited and need to be explored.

The effect of smartphones on child injuries has broader implications for how smartphones affect children's human capital development. While child injury patterns do not always inform us about factors affecting human capital development, there are several benefits to using them as a proxy in this context. First, just like parents and caregivers prevent child injuries by providing attentive supervision, the highest return investments in children's human capital come from attentive, stimulating interactions (Price 2008, Gertler et al. 2014, Sacerdote 2007, Bjorklund et al. 2006). Caregivers who let smartphones distract them during interactions with children may reduce the return on their investment. In fact, caregivers could become so distracted that they forgo investing in human capital at all (Olken 2009). Second, child injuries are salient and costly, such that if caregivers are distracted enough to let children be injured, they may also be distracted during critical learning opportunities whose effects are not realized until the long-run.

Identifying the causal effect of smartphone use on child injuries is difficult. Hospitals do not collect data on what caregivers were doing when the child is injured, and any data attempting to survey this would be subject to reporting errors. Also, because caregivers select into device use, research that relies on observing caregiver–child interactions (e.g. Radesky et al. 2014) confound causal effects with selection driven by caregiver preferences. Ideally, one would use random assignment to address the question, like Byington and Schwebel (2013) do in a lab setting to look at how smartphones increase personal injury risk in a simulated street-crossing experiment.

Instead of directly investigating the impact of smartphones on injuries, I examine a narrower question: did hospitals experience a causal increase in emergency department visits after getting access to the 3G network? I use the advent of Apple's iPhone 3G combined with the rollout of AT&T's 3G network to provide exogenous variation in the ownership and use of smartphones. At the iPhone 3G's release in 2008, consumers could use it only on AT&T's network, and not all cities had immediate access to its 3G network. Because 3G coverage is independent of individual caregiver characteristics and other accident-causing factors, differences between covered and non-covered areas reflect the influence of smartphones on injuries. For the analysis, I use data that matches AT&T's rollout to hospitals in the National Electronic Injury Surveillance System (NEISS), created by the Consumer Product Safety Commission (CPSC) to track all consumer product related injuries in emergency departments at a nationally representative sample of hospitals.

Using the hospital-level variation in 3G access, I find hospitals experience a 9% increase in emergency department visits for children under five after receiving AT&T's 3G network. To further control for confounding factors that are correlated with 3G entry, I also perform a triple difference analysis, using children between the ages of five and ten as the control group because of their weaker dependence on caregivers for supervision. The triple difference results show that 3G increased ED visits for children under five by 5.3%. Using information on how the injury occurred, I find that injuries increase in riskier activities, when supervision can make a decisive role in preventing accidents, but these effects are absent in activities where the caregivers are not the primary supervisors and in activities where supervision makes no difference on outcomes. The evidence from these results strongly supports a scenario where caregivers are distracted by their smartphones and decrease supervising children.

This paper contributes to a literature on the effects of media on the family and society. Methodologically, I follow the literature, as surveyed by Price and Dahl (2012), using natural variation in the availability of the media to estimate the causal effect. Economists have looked at how television and video games affect child human capital development and have found positive (Kearney and Levine 2015, Suziedelyte 2015) or no effects (Gentzkow and Shapiro 2008). Although I do not have direct measures of cognitive effects, I contribute to the literature on media and the environment affecting children. For instance, evidence from Brazil and India shows that television empowered women and decreased fertility(La Ferrara et al. 2012, Jensen and Oster 2009); thus, television could indirectly improve children's long-run outcomes by empowering their mothers and reducing the quantity of children needing resources. Furthermore, Olken (2009) shows that radio and television decreased participation in Indonesian social organizations, and some worry that smartphones may have a similar effect on social capital by diverting the attention of those who still participate. My results indicate this may be an issue, since caregivers are allowing themselves to be distracted, resulting in their children being injured.

Regarding cell phones specifically, I examine a new effect previously unexplored in the literature. One of the most extensive literatures on cell phones is the effect they have on car accidents through distracted driving (for a survey, see McCartt et al. 2006). Although this is an important issue, most of the time people use their phones is outside of a vehicle, and the literature has little to say on the effects of these uses. Of course, studying these other outcomes is difficult because of measurement and identification. A large contribution I make is introducing a new identification strategy, the rollout of 3G, that may help address other questions on the effect of smartphones on daily life.

1 How Smartphones Increase Injuries

The primary mechanism by which smartphones would increase injuries is by distracting caregivers from supervising children. Distractions have always existed–whether it is reading a book, chatting with a friend, or using a phone–but smartphones provide access to a greater variety and frequency of distractions. Smartphone owners can browse the internet, check social media, and stream videos. Prior to smartphones, caregivers had infrequent access to distractions; text messaging and phone calls can distract only when the other party responds, but smartphones can always access the internet independent of other parties' actions. Moreover, many smartphone content creators have engineered the content to explicitly capture the user's attention in ways unavailable in other formats. The designers optimize content to keep the user engaged, and when the user stops, the content issues alerts to draw the user back.[1] By lowering the costs of accessing distractions, the smartphone increases the opportunity cost of supervising children, and therefore caregivers increase phone usage and decrease supervision.

Another mechanism to consider is that smartphones alter how and where users spend their time. If the distraction mechanism is like a substitution effect, this mechanism would be the income effect:

[1] The social network game *Cow Clicker* by Ian Bogost provides an example of how games can become successful just by exploiting psychological engineering. Players had one objective: click on a cartoon cow every six hours to try and acquire the most clicks amongst your friends. Users could share their clicks on Facebook. The game became a surprise hit. https://www.wired.com/2011/12/ff_cowclicker/all/1

because caregivers are no longer tethered to their place of work, they effectively have more time. For example, after getting a smartphone, a caregiver may take the child to the playground more instead of having the child watch television because the caregiver can now access e-mails there. The child might get injured more, but not because the caregiver is distracted but instead purely because the child goes to the playground more. The smartphone has changed the activities in which the child participates. Under this participation effect, smartphones do not have to distract caregivers to increase injuries: if caregivers and children participate more in activities that are risky, then mechanically the number of injuries will increase. However, data from the American Time Use Survey hints that the participation effect did not drive the increase in injuries. From 2003 to 2015 the average time adults spent in activities with children decreased (Hofferth et al. 2015). Thus, the main effect will come from the distraction mechanism.

1.1 Using the 3G Rollout

To understand how smartphones influence child injuries, I look at the effect of 3G on hospital emergency department (ED) visits for children under five. This approach relies on the fact that not all cities received 3G at the same time, which affects smartphone use across cities. The identifying assumption is that after controlling for hospital and month-year fixed effects, the arrival of 3G is orthogonal to unobserved factors influencing injuries.

I use the expansion of AT&T's network because it was the exclusive iPhone carrier until 2011.[2] The iPhone started the modern smartphone era, and it quickly gained a considerable market share after its release: in the iPhone 3G's first year, consumers bought over 20 million units and downloaded over 1.5 billion applications from the App Store (Apple 2009), and Apple's market share in December 2009 was five times that of its closest competitor, Android (ComScore 2010). Other networks also provided 3G, but only AT&T could provide the market's most popular phone, and therefore AT&T provided the most important change.

An important question to consider is what drove AT&T's supply decisions; i.e. why markets receive 3G at different times and whether it affects the identification strategy. Establishing a network requires installing enough infrastructure to provide seamless coverage, and this infrastructure requires large capital outlays. For instance, for AT&T to build a 3G network, in less than two years it spent $30 million in Houston, $40 million in Chicago, $50 million in Dallas, and $65 million in San Francisco (AT&T 2009a,b,c, 2010). At a minimum, a firm would not enter the market unless the

[2] Apple released the iPhone 3G, 3Gs, 4, 4S during this period, and sales of iPhones increased consistently during the entire period.

present expected value of revenues exceeded the cost of installation. Uncertainty about adoption reduces the expected revenues, and uncertainty could vary across markets. Furthermore, AT&T has to consider that the technology is evolving and that upgrades (such as 4G) will also require costly installations. Thus market entry is an optimal stopping problem, which creates variation in the timing of entry across markets.

For my identification strategy to be valid, AT&T's supply decisions must be orthogonal to other unobservable factors that influence injuries and changed over time. Figure AI shows the geographical distribution of hospitals with dots representing how long they had 3G, and an urban-rural pattern, where urban areas receive 3G sooner, is clearly present. A hazard analysis, reported in Table A2, confirms this pattern and reveals that early 3G adopters had higher population levels and density, but did not have higher household incomes, than later adopters. Population-based expansion is consistent with similar products that benefit from network externalities; for example, population patterns predict Uber's rollout too (Hall et al. 2017). Population differences and other unobserved, time-invariant factors that influence AT&T's supply decisions will be captured in the hospital fixed effects. If population growth is parallel across cities, then the month-year fixed effects will capture it; nevertheless, Section 4 examines differences in population growth specifically. The month-year fixed effects also control for the macro shocks, such as the recession, that affect all cities at the same time. Controlling for these fixed effects, the presence of 3G is exogenous to other factors affecting ED visits.

The variation in AT&T's 3G coverage coincides with the increase in child ED visits. Figure 1 shows the fraction of hospital-months that have 3G coverage by year. In 2008, just under 30% of hospital-months had 3G coverage, and in the final years about 80% of hospital-months were covered. About 20% of the hospitals even today do not have 3G coverage. Figure 1 also plots the aggregate number of injuries for each year in the same graph, and a very strong correlation emerges. The correlation is insufficient to prove the causal hypothesis, but it gives proper motivation for the rest of the analysis.

1.2 Predictions

Because individual-level data linking smartphone use and child injuries is unavailable, I follow similar methodology as Moretti (2011) and outline intuitive comparative statistics that I can use on aggregate data. Taking the two mechanisms (distraction and participation) mentioned above into consideration, I outline three hypotheses that will guide my empirical work.

Hypothesis 1: *If smartphones increase child injuries, then areas that receive 3G will have a greater increase in child ED visits than those without.*

The availability of 3G affects the value of a smartphone because without it owners cannot fully exploit the phone's features. Some consumers will purchase a smartphone before 3G in anticipation of the network's availability, but even then the consumer cannot use it as much as others who have the network already. Hence, injuries will increase faster in 3G areas. This hypothesis holds for both of the mechanisms.

Hypothesis 2: *Smartphones will increase injuries more for younger kids than for older kids.*

In reducing unintentional injuries, supervision has a differential impact by the child's age for two reasons. First, younger children are less able to identify risk and do so slower than older children (Hillier and Morrongiello 1998). Caregivers have a strong effect on preventing unintentional injuries (Morrongiello and Dawber 2000, Power et al. 2002, Schwebel and Brezausek 2004), and being distracted will weaken this effect. Even the policies of childcare services recognize that younger children are at a greater risk of injury without proper supervision.[3]

The second reason why distractions will have a smaller effect on older children is that older children participate in fewer supervised activities. Because children gain more awareness as they age, caregivers give them more freedom. Furthermore, older kids spend most of the day in school, where smartphones will have little to no effect on the teacher's behavior because of external enforcement. This means that even if caregivers adjust their supervision based on the child's age, the effect is still larger for younger kids.

Hypothesis 3: *Holding participation rates constant, smartphones will increase injuries in risky activities but not in low-risk activities.*

Decreases in supervision have a differential impact based on the activity's risk-level. If smartphones distract caregivers, then we should see more injuries in high-risk activities. High-risk activities alone, however, will not inform us why injuries are increasing. While the distraction mechanism unambiguously implies that injuries will increase in high-risk activities, the participation mechanism could go either way. If smartphones decrease the opportunity cost of low-risk activities relative to high-risk activities—e.g. children play games on smartphones instead of at the park—then injuries in high-risk activities might decrease. On the other hand, if it decreases the opportunity cost of high-risk activities relative to low-risk—e.g. caregivers take their kids to the playground more be-

[3] For example, to be accredited by the National Association for the Education of Young Children (NAEYC), an early childhood program must have higher staff-to-child ratios for younger children: two teachers can coteach a class of up to 24 kindergartners (about age 5), but the same two teachers for a class of one to two year olds could have at most 12 toddlers(NAEYC 2013, p 89).

cause they can still do work there—then injuries in high-risk activities will increase. To distinguish between the participation and attention mechanisms, we need to find a high-risk activity whose participation rates do not change once caregivers have smartphones.

If smartphones decrease supervision, then there should be no increase in injuries in activities where the child is supervised by someone without or unable to use a smartphone or in activities where adult supervision has a negligible impact on injuries. For example, at school, teachers face external consequences for using cell phones when they monitor students. Because their cell phone use is restricted, we should not observe increases in injuries at schools. Similarly, in sport-related activities, injuries occur regardless of adult inputs because supervisors cannot directly intervene and verbal inputs can be drowned out by the environment. This implication provides a falsification test of the attention hypothesis.

2 Data on Injuries and 3G Coverage

Testing the hypotheses from Section 1.2 requires combining injury data with the 3G rollout, as described below.

2.1 NEISS

Data for nonfatal child injuries come from the National Electronic Injury Surveillance System (NEISS) run by the Consumer Product Safety Commission (CPSC) to aid its mission of protecting consumers. From the population of U.S. hospitals, the CPSC used stratified random sampling to select 100 for observation, employing enumerators at each hospital.[4] The enumerators review daily records for all of the hospital's emergency cases and record "all consumer product-related emergency visits" (CPSC 2014). The CPSC defines "consumer product" and "product-related" broadly enough that the database represents 67% of all unintentional injuries.[5] NEISS records the patient's age, where the injury occurred (e.g. home, school, etc.), the product involved, and the hospital where the patient was treated. The CPSC has collected these data since 1971, and it has made no major

[4]The CPSC divided all 5,388 hospitals in the United States into four strata. One stratum consisted of all children's hospitals in the universe (n = 50) regardless of size. They divded the remaining hospitals into four strata based on the total number of annual emergency department visits. Surveyors then randomly selected hospitals within each strata. For further information on the sampling procedure, see Schroeder and Ault (2001).

[5]The CPSC defines consumer product as "'any article produced or distributed for use by a consumer in or around a home, school or recreational area." Product-related includes (1) all poisonings and chemical burns to children under 5 years of age, (2) all injuries where a consumer product, sport, or recreational activity is associated with the reason for the visit or related to a condition treated, and (3) illnesses only if a consumer product/activity is associated with the onset of the illness. For more information, see CPSC (2014).

changes to the system since 2000 CPSC (2000).

The data are particularly useful for this research question because the CPSC's interest in how an injury occurred helps overcome the reporting problem. Other data sets might contain a broader sample of hospitals or more detailed information on the injury and treatment, but the research question concerns *how* injuries happen. No data set can overcome the reporting problems involved with directly measuring whether the child was injured because the caregiver was looking at a smartphone–the adult might not be aware of the smartphone's role or could obfuscate to cover guilt. To be clear, no variables correspond to whether a phone was involved in the injury. The CPSC does provide a code for whether a phone was involved, but these are cases where the patient was on the phone or hurt by a phone. For example, in one case a 45 year-old man missed catching a thrown phone, and it hit him in the mouth giving him a 3.5 cm laceration on the lip.[6] In these cases, the phone directly interacts with the patient, but the injuries we are interested in occur when the phone interacts with a supervisor and the patient gets injured. Nevertheless, because the data detail the injury's circumstances, I can provide indirect evidence of smartphone distraction by classifying situations according to how risky they are without supervision. For instance, young children face a lot of risk on playgrounds when caregivers do not monitor them; therefore, a sudden increase in playground injuries once smartphones arrive supports the distraction hypothesis. The analyses in Sections 3.1 and 3.2 look at all injuries, regardless of the activity or product involved, and then subsequent analyses in Section 3.3 take advantage of specific location and product codes to get at the distraction mechanism.

2.2 AT&T's 3G Rollout

Data for the AT&T 3G coverage comes from AT&T press releases available on its website. With the completion of almost every 3G installation, AT&T issued a press release announcing the cities where it had extended coverage. Using the date of the press release and the cities named, I can reconstruct the rollout of coverage. To validate the press releases and fill gaps, I also grabbed historical data for the AT&T Coverage Viewer from the Internet Archive Way Back Machine[7]. To construct my treatment variable, I found when each hospital received 3G coverage from AT&T.

Coverage is a binary variable equal to one after the iPhone release in July 2008 as soon as a city

[6] CPSC Case #101218722. Common injuries under this category include senior citizens falling out of bed while reaching for a phone, people tripping over telephone cords, or users getting shocked while plugging a phone into an outlet.

[7] The AT&T Coverage Viewer provides a lsit of cities with 3G available and can be accessed at http://www.wireless.att.com/coverageviewer/popUp_3g.jsp. The Internet Archive Way Back Machine allows users to access cached versions of web pages by date and is available at http://www.archive.org/web.

within 30 miles of the hospital receives 3G.[8] Some cities received 3G in 2008 before July, but the treatment is access to 3G and the iPhone 3G, so these observations are coded as not having 3G until July.[9] The 30 mile radius accounts for two factors. First, the coverage lists only mention big cities when they certainly mean surrounding areas also received 3G at the same time. For example, the NEISS data include a hospital in Gaithersburg, Maryland, but Gaithersburg is never mentioned in the coverage lists or press releases; however, it is 15 miles from Washington, D.C. and 30 miles from Baltimore, and a coverage map from 2008 clearly shows that the Gaithersburg area was covered then. The radius accounts for the coverage data citing only major cities. Second, even if a town does not have 3G, residents may commute to an area that does. The 30 mile radius catches the broader labor market and is more conservative than the USDA defined commuting zones, whose average radius is about 42 miles.[10]

2.3 Privacy restrictions

Because the NEISS contains sensitive patient information, the CPSC cannot release the hospital or city where the patient was treated. The CPSC can release the list of all cities in the data, but it cannot release matched city–incident data. To maintain privacy standards, I used the city list and coded the 3G roll-out, as detailed above; I then sent the matched list to the CPSC, who merged the coverage data onto the NEISS and removed any identifying information. The data contain anonymized hospital identifiers, which allow me to identify the same hospital across observations, but I do not know where the hospital is in the U.S. The CPSC agreed to provide the data because my treatment variable is binary and 3G expands in waves, limiting the opportunity to identify specific hospitals. The CPSC does not allow continuous variables, on the other hand, because someone could more easily use them to identify the observations and violate the privacy concerns.[11] Thus, I cannot add variables such as population or smartphone use rates. This restriction prevents me from estimating the elasticity of injuries to the population of smartphone users, but it does not inhibit finding causal effects of 3G on ED visits.

[8] In Table AI, I list the cities in the NEISS according to when they got 3G. For clarity of presentation in the table, I group the hospitals into five periods, but in the analysis I use the exact month and year the city received 3G.

[9] In regressions not reported here, I defined an additional treatment variable for whether a city had 3G in a month before July 2008. The point estimate for this treatment was less than 0.001 and was statistically insignificant, which suggests getting 3G without an iPhone had no effect. However, the treatment could be identified off of only three months of data, and therefore was not included in this paper.

[10] Autor et al. (2013) use these same commuting zones to look at the effect of trade with China on local labor markets.

[11] For example, if the data contained the city's population, it would take little work to match the observations with the city list.

The big strength of the data is that it is a large, high-frequency panel. Controlling for hospital fixed effects eliminates permanent, unobservable differences across hospitals correlated with the 3G expansion. Furthermore, I can control for hospital-specific trends in injuries. The data do have their weaknesses. For instance, children could be injured in a non-consumer product related accident, which account for a third of unintentional injuries, or enumerators could miscode injuries. More importantly, using this data imposes a restriction that the injury had to be severe enough to warrant a hospital visit.

The unit of observation in the final data set is the total ED visits at hospital h in month t for age a. The sample starts in January 2003 and ends in December 2012. The final data set also aggregates injuries at the hospital-month-age level for some products and locations. While the product data is rich in detail, the injury location data (i.e. whether the injury occurred at home, in a public space, etc.) is not as complete, with 25% of the injuries listing "unknown" for the location. I include all of these observations in the main results, but I cannot use them in the analyses in Section 3.3 that rely on knowing the location. I assume the unknown locations are random and that omitting them from the location-specific subsets does not bias the results.

3 Testing for a causal link between smartphones and injuries

If smartphones cause injuries by distracting caregivers, then the data should reflect patterns consistent with the hypotheses in Section 1. Testing the hypotheses' econometric analogs shows the data are consistent with smartphones distracting caregivers.

3.1 Hypothesis 1: Child injuries should increase after 3G arrives

To test the effect of 3G on child ED visits, I do a difference-in-differences analysis exploiting the differential exposure to 3G. I estimate the following regression

$$\sinh^{-1}(y_{hta}) = \delta_h + \delta_t + \delta_a + \beta 3G_{ht} + \varepsilon_{ht} \tag{1}$$

where y_{hta} is the number of visits at hospital h in month t for age group a. The hospital dummies (δ_h) control for permanent differences across hospitals, and the month-year dummies (δ_t) control for transitory shocks that affect all hospitals in the same period. The age dummies (δ_a) account for differences across the ages included in the sample, which is limited to patients under the age of five. I use the inverse hyperbolic sine (IHS) as the dependent variable to resolve concerns with months where a hospital records zero ED visits for an age group.[12] In a regression framework, the IHS behaves like the log transformation (Burbidge et al. 1988), but the IHS is defined at zero. The coefficient can be interpreted as a percentage change. The results are robust to other dependent variable specifications, which I include in Appendix Table A3.

The results show that ED visits for children under five increased after 3G arrived. Table 2 shows that after 3G entered, ED visits increased by 9%, and the estimate is significant at the 5% level with standard errors clustered at the hospital level. Column 2 in Table 2 estimates the same regression with hospital-specific trends to control and the magnitude drops to 4%. This second estimate is not significantly different from zero, but it is also not significantly different than the first estimate. Measuring a smaller treatment effect after including hospital-specific trends is consistent with the discussion in Meer and West (2016), which shows that time trends as controls can attenuate estimates in a difference-in-differences framework.

The difference-in-differences' validity relies on the parallel trends assumption, and to test for this assumption I estimate a regression that includes dummies for six-month bins before and after 3G entrance. The parallel trends assumption predicts that $\beta_t = 0$ for all t before 3G and $\beta_t > 0$ for all t after. The results are consistent with these predictions. I plot the coefficients and their standard errors in Figure 2. The omitted group is the six months prior to the entrance of 3G, such that all coefficients are relative to the injuries occurring in this period. All of the pre-period coefficients are indistinguishable from zero; furthermore, there is no distinct trend in the magnitudes, and the coefficients even flip signs. On the other hand, all of the post-period coefficients are positive and most are statistically significant. Thus, before 3G arrived, treated areas looked similar to non-treated areas, and only after 3G arrived did they diverge.

3.2 Hypothesis 2: Injuries should increase more for younger children than older children

Hypothesis 2 says we can use variation across age groups in a triple differences analysis to identify the effect. If injuries increase because smartphones distract caregivers, then older children provide a natural comparison group because they rely less on caregivers when evaluating risky situations. By splitting the observations into under five years old and five and older, I estimate the following

[12] 14.8% of hospital-month-age group observations record zero visits.

regression

$$\sinh^{-1}(y_{hta}) = \delta_h + \delta_t + \delta_a + \delta_h * Under5_a + \delta_t * Under5_a + \beta_1 3G_{ht} + \beta_2 3G_{ht} * Under5_a + \varepsilon_{hta}. \quad (2)$$

This regression includes all injuries to children 10 and younger, with the $Under5_a$ dummy indicating whether age group a is young enough to be affected by 3G. Hypothesis 2 predicts that there should be no effect of 3G on older children ($\beta_1 = 0$) and a positive effect for younger children ($\beta_2 > 0$).

The triple difference approach supports the conclusion that smartphones increased child injuries and is responsible for most of the increase from 2005 to 2012. The results reported in Table 3 indicate that the effect falls on the younger children. Older children experience a 3.6% increase in injuries, but this coefficient is not statistically significant. Younger children, on the other hand, see a 5.4% increase above the older children, significant at the 1% level. Again, consistent with findings in Meer and West (2016), including hospital-age-group trends attenuates the estimate for younger children to 4.2% (significant at the 10% level) and eliminates any effect for older children. This finding is robust to other regression specifications reported in Appendix Table A3. Of the estimated effect in the original difference-in-differences regression, factors that affect everyone explain 41%, and 3G expansion explains the remaining 59%.

The triple difference results eliminate many confounding factors because they show no evidence of increase for injuries to older children. If migration or improved facilities were the cause, we would observe a change for children of all ages. Indeed, any alternate hypothesis must explain why the effect differs by age. The expansion of 3G coincides with the same time period as the recession, which had a differential effect across cities and could through some mechanism increase injuries or ED visits. But a recession story cannot explain why ED visits increase only for the younger kids. Second, the analysis dispenses supply-side explanations. These supply-side stories involve local changes that increase ED visits—for example, new playgrounds that provide more opportunities for children to get hurt, or hospital improvements that increase the demand for ED services—and are correlated with the treatment. But again, these explanations cannot explain the differential results by age.

3.2.1 Treatment Heterogeneity by Age

While splitting children between younger and older than five is not completely arbitrary, it can mask heterogeneous effects by age. Separating the groups at age five makes sense considering children in the U.S. start school at that age and therefore move into an environment where the probability of injury greatly decreases. Yet there is no reason why the effect should be the same for all kids under five, and even kids five and over could still suffer from distracted caregivers. The hypothesis states the effect should be decreasing in age, since two year-olds need more supervision than three year-olds, and so forth.

To address the heterogeneity, I run a triple difference regression separately for each age group younger than ten, using injuries to ten year-olds as the control group; i.e. I run ten different regressions for each age group from zero to nine, each with ten year-olds as the control group. Figure 3 plots the coefficients for the 3G treatment from each regression, and the pattern is clear: the effect is decreasing with age.

3.2.2 Feasibility of Magnitudes

So far, I have focused on making a causal case for injuries increasing after 3G arrives, but I have ignored the magnitudes. The triple difference results indicate that hospitals had 5.3% more ED visits for children under five after 3G entered. But is this even a feasible effect size given the market penetration of smartphones?

A back-of-the-envelope calculation shows this is a reasonable effect. Assuming the effect is constant for the whole country, I take the 2005-06 average injuries for each age group under 5, found in Table I, and multiply them by 5.4%, the coefficient from the triple difference specification. This calculation says that we would find an extra 79,913 injuries by 2011, when the 3G coverage is complete. According to the PewResearch Internet Project (Pew Research Center, 2011), in 2011, 7% of Americans adults owned a smartphone and had a child under the age of 5. Taking the adult population of the U.S. to be 250 million, then the implied injury rate is 4.6 out of every 1,000 parents of children 5 and under who use a smartphone experience an injury. To put this number in perspective, the injury rate for cars is about 10.6 per 1,000 drivers (NHTSA, 2010); the injury rate from cars is more than twice the injury rate for smartphone users, yet the car injury rate is not high enough to prevent millions of drivers from taking the risk every day. Hence, while the increase is significant relative to the overall injury rates, it does not seem unreasonable to believe relative to the population of smartphone users.

Comparing the effects of smartphones on child injuries with the effects of cell phones on car accidents raises questions on how phone users adapt to increased risk. The two most convincing papers in the cell phones and car accidents literature find opposite results. Redelmeier and Tibshirani (1997) use cell phone bills to match phone use with the timing of accidents and find that the risk of collision was four times higher while using a phone. On the other hand, Bhargava and Pathania (2013) use variation in cell phone pricing in a regression discontinuity design and find small effects. But the data from these two studies are separated by 10 years, and users might have adapted to the increased risk over this period. I look at a period of introduction and rapid adoption, allowing me to look at outcomes before users can adapt. Figure 1 shows that injuries quickly increased from 2008 to 2010 and then leveled off, suggesting that caregivers might have already adapted by the end of the period. More work should be done on the short– and long–term effects of cell phones on accident risk.

3.3 Hypothesis 3: Smartphones increase injuries where supervision is important

Testing this hypothesis uses variation in how the injury occurred to identify distraction as the causal mechanism. If smartphones distract caregivers, we should observe injuries increase in activities where caregivers play a significant role in preventing injuries and no change where they have little influence. Below I provide three examples of activities that vary in their supervision levels.

To test for differences in caregiver influence, I hold risk constant by looking at injuries that occur on playground equipment but at different places. With the NEISS data, I can split injuries between those that happen at school playgrounds and those that happen at non-school playgrounds. Injuries that occur at school are unlikely to be influenced by smartphones since the teachers face external incentives to not use their phones when they should be supervising. Furthermore, teacher supervision is divided across many children, such that an overall decrease in supervision is a much smaller decrease when considered in per child terms. On the other hand, if the distraction effect exists, then non-school playground injuries could increase since caregivers are more likely to supervise children in these situations.

Two distinct triple difference strategies suggest smartphones increased injuries. The first strategy compares ED visits across ages for the same type of playground. For example, the regression in the first column and row of Table 4 includes only injuries that happen on non-school playgrounds and compares children age zero to one with ten year-olds. The identifying assumption here is that ten year-olds do not need supervision and are therefore unaffected when smartphones distract caregivers. Under this strategy, ED visits from school playground accidents serve as a placebo test. The first two columns of Table 4 report the triple difference coefficient from 20 regressions, 10 ages and two types of playgrounds. Consistent with the hypothesis, injuries increased for non-school playgrounds but did not change for school playgrounds. Indeed, the school playground injuries hint that playground-related injuries may have decreased over this period had smartphones not entered. It is important to note that there was no increase for non-school playground injuries children under one, who are too small to play on playgrounds and effectively serve as an additional placebo test strengthening the identification strategy's validity.

The second strategy compares ED visits for the same age across playground types. The identifying assumption is that smartphones affect injuries at non-school playgrounds but not at school playgrounds. This specification eliminates concerns of whether ten year-olds serve as an adequate control group for two year-olds by using the same age as a control group. Table 4's third column reports the 11 triple difference coefficients. The story stays the same—injuries increase on non-school playgrounds after 3G arrives.

Unfortunately, these results alone cannot tell us whether the increase in injuries comes from the distraction effect or the participation effect, since either could cause this increase. Indeed, the equal effect across all ages could be because the participation mechanism is non-zero since the distraction effect implies a larger impact for younger kids. To test Implication 3, which isolates the distraction effect, I need to identify activities whose participation rates will be unaffected by the introduction of smartphones.

One class of injuries that fits this qualification are poisonings; smartphones should not put caregivers in situations where children are more at risk of ingesting hazardous materials. In fact, the only way 3G could be related to an increase in poisoning is if caregivers are distracted and fail to warn children. In Figure 4 I plot the effect of 3G on ED visits involving poisoning, again using a triple-difference regression by age with 10 year-olds as the control group. The pattern unmistakably reflects distracted caregivers: children less than one experience no change because they are not mobile; and children three and older do not either because they have been taught to avoid these materials; but one and two year-olds, the children most dependent on supervision because they are mobile and curious, experience an 8% increase in ED visits. The results are consistent with distracted caregivers.

Finally, I look at an activity where supervision makes little difference but participation might increase: sports-related injuries. Spectator input has little effect on the moment-to-moment outcomes in sports, even in the small audiences that view children's sports. Thus, supervision should have no effect on injuries. However, children might play sports more if their caregivers are more willing to take them to play, which would increase injuries. In the sports-only sample[13], in Figure

[13] The sports included in the NEISS data are: bowling, boxing, croquet, football, golf, lacrosse, archery, horseback

5, 3G has no effect on injuries. Because sports are one of the areas where the participation effect would be most evident, this suggests that participation is not driving the injuries.

4 Robustness Checks

4.1 Timing of the Effect

Finally, one concern to resolve is a general time-trend that could confound the analysis. The data cover a long time horizon, and one fear when comparing pre- and post-treatment means is that the time trend was increasing through the entire sample period, and this gradual trend is lost in measuring the treatment effect. That the result is robust to hospital-specific trends mitigates this issue, but to provide a more convincing case in favor of the smartphone mechanism, in Table 5 I present results using three different time horizons: the full sample, one year before and after the iPhone 3G release, and six months before and after. Restricting the sample size reduces the power of my estimates, yet the results are consistent. Even when looking in 2008, I find an increase in injuries only for children under five and only in cities that had 3G coverage at the time of the release. This result is particularly important because this window contains cities that eventually receive 3G but do not have it by the end of 2008, so the result is not an artifact of city-specific characteristics that affect both child injuries and where AT&T chose to expand its market.

4.2 Demographic Trends

The work thus far has assumed that 3G expansion is unrelated to other factors affecting child injuries. This assumption is sound because AT&T is not deciding supply based on how dangerous a city is for kids, and it certainly does not time its expansion for when these cities become more dangerous. Yet AT&T does consider population trends in its supply decisions, as shown in Section 1.1, and differential population trends could generate these results.

To be clear, demographic trends cannot explain the puzzle that injuries to children under five increased by 10% over seven years. There is no evidence of a demographic transition that occurred sharply at the same time unintentional injuries rapidly increased. However, demographic trends

riding, horseshoes, mountain climbing, billiards, surfing, water skiing, volleyball, soccer, table tennis, wrestling, scuba diving, tetherball, ice hockey, handball, snowmobiles, field hockey, snow tubing, water tubing, skeet shooting, roller skating, skating, badminton, fishing, rugby, ball sports, street hockey, ice boating, cheerleading, ice skating, martial arts, fencing, shuffleboard, weight lifting, hockey, swimming, water polo, dancing, curling, snow skiing, tennis, snowboarding, softball, and baseball. Results are robust if restricted only to the five most popular American sports: football, baseball, basketball, soccer, and hockey.

could misattribute the cause to smartphones if 3G expansion is correlated with differential population growth trends, specifically if areas receiving 3G also had faster population growth rates. Because AT&T expanded its 3G network to maximize profits, and because areas with faster population growth provide growing markets, differential population growth could threaten identification. If the five and over population grows at the same rate as the under five population, then the triple difference specification will correct for this and the estimates will be fine. However, AT&T could target areas where the young adult population is growing faster since they are more likely to adopt smartphones, and if this population is also having children at this time, then in 3G areas the population younger than five would also be increasing faster. However, while in theory these differential trends could occur, the data do not support them.

The population in areas with 3G did grow faster than non-3G areas, but this growth occurred only in the adult population. In fact, the population under five in these areas grew slower relative to the rest of the country. Due to data limitations discussed above, I cannot combine population data with the NEISS data; however, I do know the cities where NEISS hospitals are located and can analyze census county population estimates separately to see how trends differ once 3G arrives. Table A6 shows that 3G expansion coincided with the total population growing 1.9% faster than non-3G areas, consistent with AT&T expanding to areas with faster market growth. Nevertheless, the population between 8 and 13 did not differ at all between 3G and non-3G areas, and in fact the population younger than five actually grew 3.8% slower once 3G arrived. Fast total population growth, stagnant older child growth rates, and decreasing younger child growth rates are consistent with growing adult populations but decreasing birth rates. Column 4 of Table A6 tests this hypothesis using the CDC Natality data and confirms that 3G expansion coincided with lower birth rates. One could argue that 3G expansion causes adults to move to the area, and by some stretch you could even say smartphones lower birth rates in a similar mechanism to the argument for their increasing injuries, however these trends cannot explain the increase in injuries and would not misattribute their cause to 3G expansion.

In fact, the demographic changes suggest that the regressions underestimate the true effect of 3G expansion on child injuries. The analyses above assumed that population growth rates did not diverge across hospitals over the sample. However, if injuries increase at the same time as the population shrinks, the effect on the injury rate is much larger than what I have estimated. Table A7 reports estimates after adjusting for population trends and shows effects 40-75% larger than in the unadjusted data, but because the adjustment is inexact, the results are illustrative yet inconclusive.

5 Discussion

I use the rollout of 3G to make a case for a causal effect of smartphone-induced child injuries. Hospitals experienced an increase in emergency department visits after getting 3G coverage, but only for children under five. Furthermore, the activities associated with these injuries are consistent with caregivers being distracted.

One concern with the analysis is that data privacy issues prevent me from using controls to eliminate confounding factors; however, this concern is small. The most convincing piece of evidence against an alternative hypothesis is the triple difference results that show children five and older at the same hospitals experienced no significant increase in injuries. Furthermore, the confounding factor would have to follow the very specific patterns revealed in the empirical analysis. This paper has convincingly made the case for smartphones causing injuries, and hopefully future work will generate greater precision on the size of the effect.

The results have ambiguous welfare implications since I do not model the child's utility. If caregivers decide to use the smartphone while considering the child's utility like a unitary household model, then we would conclude that the whole household is better off, even with the increased injury risk. But if the caregiver does not consider the child's wellbeing or does not realize the phone is a distraction, then the caregiver might be benefiting at the expense of the child. Even if we chose the correct set of modelling assumptions, some might find it appalling to believe that any use of a phone could rationalize increasing the risk of harming a child. On the other hand, others could argue that children are currently overprotected and could benefit from more skinned knees and broken arms, and therefore smartphones improve welfare. The best public policy approach, therefore, may be to increase awareness of the risk and allow households to choose for themselves.

Although the welfare conclusions are ambiguous, the results certainly raise questions as to other domains where smartphones have an effect. Because injuries occur immediately and can require costly care, one would think caregivers would be extra diligent in avoiding distractions. But that does not seem to be the case. When the consequences of distractions are farther in the future, such as inattention during key learning opportunities, caregivers may be even slower to adjust. Future research may include looking at how smartphones affect investments in children, student learning in classrooms, and employee productivity.

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Table 1: Change in National Annual Emergency Department Visits in NEISS Data by Age Group from 2005 to 2012

Age	2005-06 Average	2011-12 Average	Pct. Increase
0	224,443	239,015	6%
1	352,350	386,498	10%
2	370,600	400,494	8%
3	299,590	332,668	11%
4	260,815	288,085	10%
5	243,403	251,342	3%
6	218,943	222,375	2%
7	212,250	217,986	3%
8	218,486	223,527	2%
9	228,344	245,280	7%
10	256,700	266,439	4%

Notes: The figures are two year averages; i.e. from January 1 of the first year to December 31 of the second.

Table 2: Difference-in-differences estimates of the effect of 3G on child injuries

	(1)	(2)
3G	0.091**	0.039
	[0.044]	[0.025]
Hospital-Specific Trend	No	Yes
N	54,865	54,865
Adjusted R^2	0.83	0.84

Notes: The dependent variable is the inverse hyperbolic sine of the number of ED visits for children a years old in hospital h in month t, and sample includes only ED visits for children under five. All regressions contain hospital, age, and month fixed effects. Standard errors are clustered at the hospital level. ** p<0.05

Table 3: Triple difference estimates of the effect of 3G on child injuries

	(1)	(2)
3G*(Age<5)	0.054***	0.042*
	[0.018]	[0.022]
3G	0.036	-0.0033
	[0.040]	[0.026]
Age<5	0.53***	0.58***
	[0.037]	[0.042]
Hospital-Age-Group-Specific Trend	No	Yes
N	120,703	120,703
Adjusted R^2	0.81	0.82

Notes: The dependent variable is the inverse hyperbolic sine of the number of ED visits for children a years old in hospital h in month t, and sample includes ED visits for children ten and under. All regressions contain hospital, age, and month fixed effects as well as age-group-hospital and age-group-month interactions. The hospital-age-group-specific trend is a linear trend for each age group ("under five" and "five and over") in each hospital. Standard errors are clustered at the hospital level. *** $p<0.01$ * $p<0.10$

Table 4: The effect of 3G on playground injuries: results from different triple difference strategies

	(1)	(2)	(3)
Treated	Row Age on Non-School Playgrounds	Row Age on School Playgrounds	Row Age on Non-School Playgrounds
Control	10 year-olds on Non-School Playgrounds	10 year-olds on School Playgrounds	Row Age on School Playgrounds
Age 0	-0.0023	-0.0086	0.00046
	[0.014]	[0.015]	[0.0064]
1	0.034**	-0.0067	0.035**
	[0.015]	[0.015]	[0.014]
2	0.035*	-0.0062	0.036**
	[0.019]	[0.016]	[0.016]
3	0.036*	-0.018	0.049**
	[0.020]	[0.016]	[0.020]
4	0.044**	-0.0011	0.039**
	[0.019]	[0.018]	[0.018]
5	0.047**	-0.017	0.059**
	[0.020]	[0.018]	[0.026]
6	0.041**	-0.02	0.055**
	[0.020]	[0.017]	[0.027]
7	0.041*	-0.022	0.057**
	[0.021]	[0.016]	[0.025]
8	0.050**	-0.018	0.061**
	[0.019]	[0.016]	[0.024]
9	0.034*	-0.011	0.039**
	[0.018]	[0.014]	[0.019]
10			-0.0058
			[0.018]

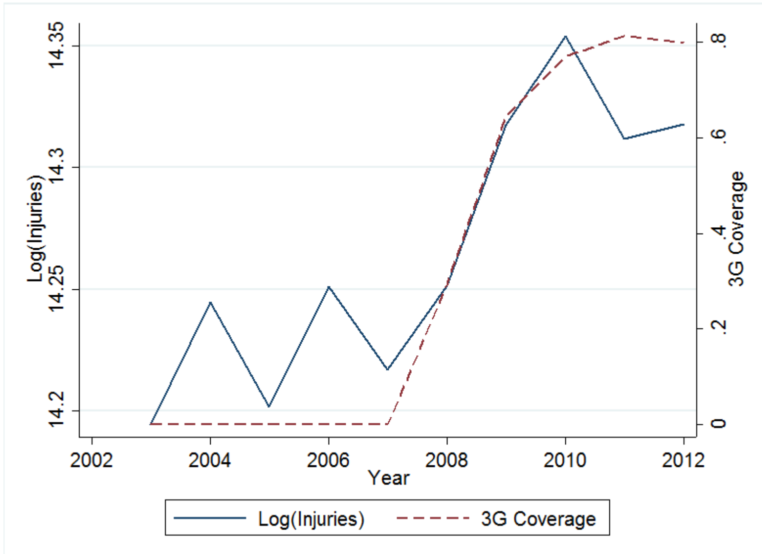
Notes: Each cell reports the triple difference coefficient from a different regression. All regressions use differences across hospitals before and after 3G entrance, but the third difference varies across regressions as indicated by the treated and control groups at the top of each column. All regressions have 21,946 observations, and standard errors are clustered at the hospital level. ** $p < 0.05$, * $p < 0.1$.

Table 5: Estimating the triple difference with different time windows

	Full Sample	Jul 2007-Jul 2009	Jan 2008-Dec 2008
3G*(Age<5)	0.054***	0.049*	0.069*
	[0.018]	[0.028]	[0.040]
3G	0.036	0.027	-0.018
	[0.040]	[0.029]	[0.036]
Age<5	0.53***	0.37***	0.71***
	[0.037]	[0.044]	[0.044]
N	120,703	25,146	12,045
Adjusted R^2	0.81	0.81	0.81

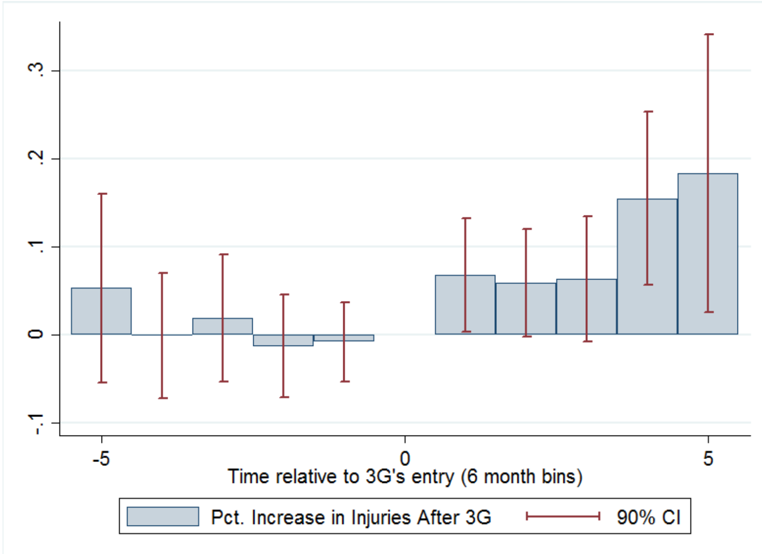
Notes: See the notes in Table 3 for details on the regression specification. Apple released the iPhone 3G in July 2008, which is the center of each of the narrower time windows. *** p<0.01 *p<0.10

Figure 1: Nonfatal injuries involving consumer products for children under 5 in the US and Hospital 3G coverage, 2001–2012



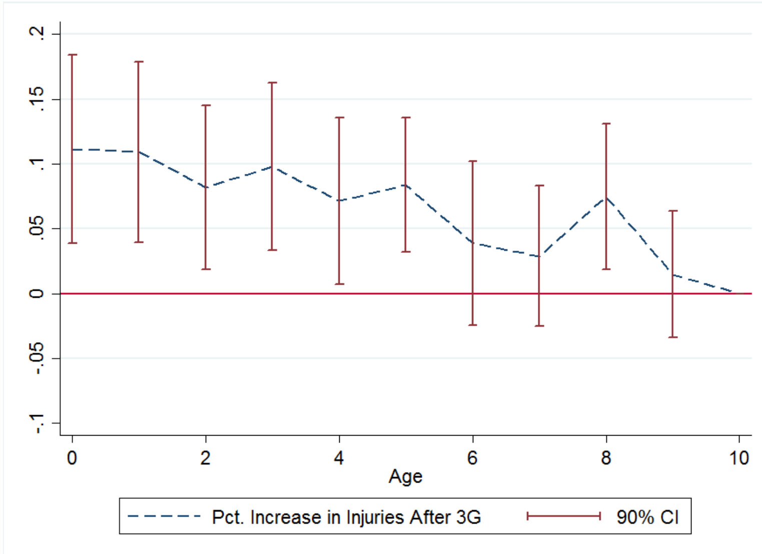
Notes: Injury data comes from weighted counts in the NEISS data. The 3G trend is the share of hospital-months in the NEISS data that had 3G in a given year.

Figure 2: Checking Pre-existing Trends: The Effect of 3G Over Time on Injuries to Children 5 and Under



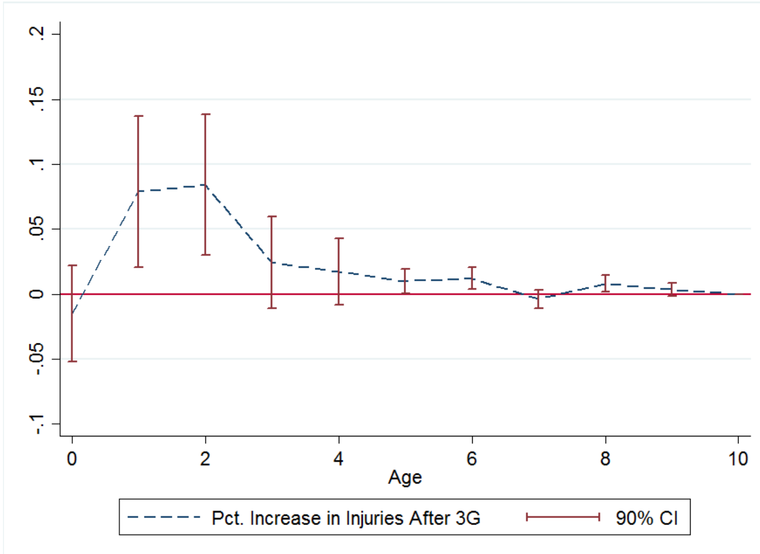
Notes: Coefficients come from a specification similar to Table 2 (see notes) except instead of a 3G dummy the regression uses time dummies grouped in six-month bins. Positive values of T correspond to months after the market received 3G, and negative indicate before.

Figure 3: Effect of 3G on Hospital ED Visits by Age Group



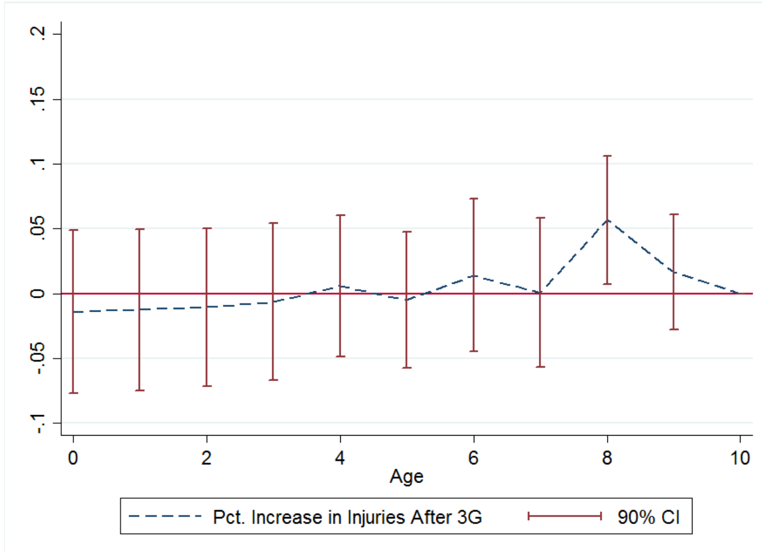
Notes: Coefficients are the treatment effects in a triple difference regression pairing each age group with 10 year olds as the control group. Standard errors are clustered at the hospital level.

Figure 4: Effect of 3G on ED visits involving poisoning



Notes: See the notes for Figure 3. The sample includes only injuries where the patient was poisoned.

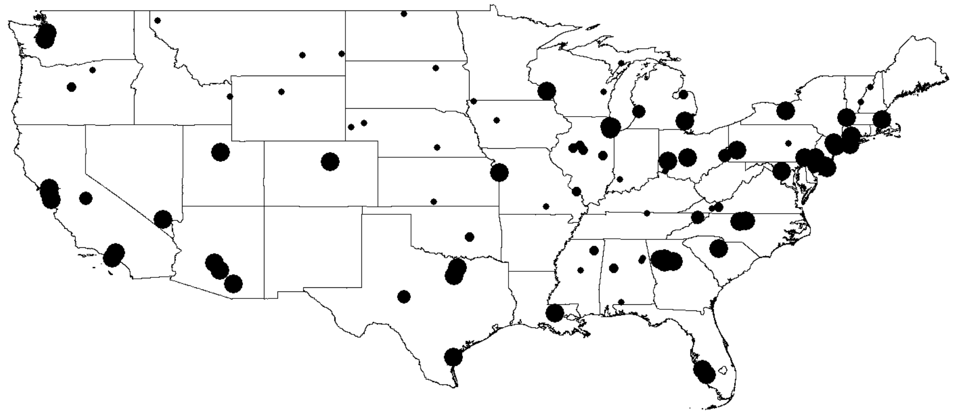
Figure 5: Effect of 3G in activities where supervision has little effect: sports injuries



Notes: See the notes for Figure 3. The sample includes only sports-related injuries. See footnote 13 on the definition of which sports are included.

6 Appendix

Figure A1: Geographic distribution of NEISS Hospitals and Initial 3G Coverage



Notes: Dot size indicates treatment length—largest dots had 3G in July 2008, smallest dots never received it in the sample frame. Map omits Puerto Rico, which had 3G July 2008.

Table A1: Cities with Hospitals in NEISS according to when they received AT&T's 3G network

Have 3G before July 2008	**Receive 3G Aug-Dec 2008**	**Never Receive 3G**
Arizona: Phoenix, Sacaton, Tucson	*Arkansas*: Hope, Nashville	*Alabama*: Anniston, Jacksonville
California: Mountain View, Oakland,	*California*: Mariposa	*Indiana*: Washington
Pasadena, Torrance, Vallejo	*New Jersey*: Bridgeton	*Iowa*: Lake City, Rock Rapids
Colorado: Denver	*Ohio*: Martins Ferry	*Kansas*: Medicine Lodge
Connecticut: New Haven		*Michigan*: Escanaba
Florida: Fort Myers, Port Charlotte	**Receive 3G in 2009**	*Mississippi*: Kosciusko
Georgia: Atlanta, Covington,	*Alabama*: Tuscaloosa	*Missouri*: Mountain View
Douglasville, Riverdale, Fort Benning	*Illinois*: Canton, Chester, Hopedale,	*Montana*: Baker, Forsyth, Libby
Illinois: Chicago, Oak Lawn, Oak Park	Pekin, Peoria, Urbana	*Nebraska*: Alliance, Hastings,
Louisiana: Baton Rouge	*Michigan*: Pigeon, Zeeland	Scottsbluff
Maryland: Gaithersburg, Lanham	*Mississippi*: Tupelo	*North Dakota*: Bottineau
Massachusetts: Boston, Pittsfield	*Oklahoma*: Holdenville	*Oregon*: Heppner
Michigan: Wyandotte	*Pennsylvania*: Waynesboro	*South Dakota*: Aberdeen
Minnesota: Winona	*Texas*: San Angelo	*Tennessee*: Celina
Missouri: Kansas City, Saint Louis	*Virginia*: Wytheville	*Wyoming*: Worland
Nevada: Las Vegas		
New Jersey: Atlantic City, Pomona, Teaneck		
New York: Bronx, Brooklyn, Geneva,	**Receive 3G in 2010**	
New Springville, Patchogue	*Alabama*: Brewton	
North Carolina: Burlington, Greensboro	*Idaho*: Driggs	
Ohio: Columbus, Dayton	*Mississippi*: Tylertown	
Pennsylvania: Coatesville, Philadelphia,	*New Hampshire*: Claremont, Littleton	
Pittsburgh, Ephrata	*Oklahoma*: Fairfax	
Puerto Rico: San Juan	*Oregon*: Prineville	
South Carolina: Winnsboro	*Pennsylvania*: Sunbury	
Tennessee: Johnson City	*Virginia*: Marion	
Texas: Corpus Christi, Denton, Ft Worth	*Wisconsin*: Chilton	
Utah: Orem		
Washington: Everett, Seattle, Tacoma		

Notes: A city is defined as receiving 3G if a city within 30 miles received 3G.

Table A2: Factors that predict 3G entry in a county where a NEISS hospital is located

	(1)	(2)	(3)	(4)	(5)
log(Population 2010)	1.620***			1.344**	1.380**
	[0.130]			[0.182]	[0.179]
log(Population Density)		1.441***		1.198*	1.204*
		[0.0889]		[0.127]	[0.127]
log(Med. HH Income)			4.066***	1.534	
			[1.900]	[0.751]	
Population Under 5					0.863
					[0.107]

Notes: The reported coefficients are hazard ratios from Cox hazard models. All regressions have 97 observations. Data come from U.S. Census Bureau (2014). *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$.

Table A3: Difference-in-differences specification check

	IHS	Log(Count+1)	Log(Count)	Count/Mean	Poisson
3G	0.0906**	0.0795**	0.0986***	0.148***	0.0952**
	[0.0439]	[0.0356]	[0.0366]	[0.435]	[0.0446]
N	54,865	54,865	47,181	54,865	54,865

Notes: Each column except for the last provides a different specification of the dependent variable: IHS is the inverse hyperbolic sine used in the paper; Log(Count+1) adds one to all observations so that a hospital-month-age with zero injuries recorded is included in the regression; Log(Count) takes the log of the observation and omits zeroes; Count/Mean uses the raw count as the dependent variable and the coefficient is then scaled by the sample mean to get a percent increase at the mean. The last column is a Poisson regression using the count as the dependent variable. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Triple difference specification check

	IHS	Log(Count+1)	Log(Count)	Count/Mean	Poisson
3G*(Age<5)	0.0542***	0.0494***	0.0494**	0.110***	0.0306*
	[0.0177]	[0.0146]	[0.0198]	[0.217]	[0.0186]
3G	0.0364	0.0301	0.0492	0.0382	0.0645
	[0.0398]	[0.0320]	[0.0330]	[0.0318]	[0.0452]
N	55,445	55,445	47,129	55,445	55,445

Notes: See Table A3 notes.

Table A5: Coefficients for Figures

	Figure 3	Figure 4	Figure 5
	All	Poisoning	Sports
Age 0	0.111**	-0.0151	-0.0141
	[0.0442]	[0.0226]	[0.0385]
1	0.109**	0.0789**	-0.0125
	[0.0425]	[0.0353]	[0.0380]
2	0.0819**	0.0841**	-0.0105
	[0.0386]	[0.0330]	[0.0372]
3	0.0979**	0.0245	-0.00657
	[0.0393]	[0.0217]	[0.0369]
4	0.0715*	0.0174	0.00584
	[0.0391]	[0.0156]	[0.0332]
5	0.0838***	0.0101*	-0.00493
	[0.0316]	[0.00581]	[0.0319]
6	0.0391	0.0120**	0.0141
	[0.0386]	[0.00515]	[0.0360]
7	0.0289	-0.0037	0.000796
	[0.0331]	[0.00431]	[0.0349]
8	0.0748**	0.00814**	0.0569*
	[0.0341]	[0.00379]	[0.0302]
9	0.0149	0.00355	0.0167
	[0.0296]	[0.00313]	[0.0272]

Notes: Coefficients for all figures in the paper. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: 3G Entrance and County Population Trends

	Population Total	Population Between 5 and 13	Population Under 5	Birth Rate
3G	0.019**	0.0028	-0.038**	-0.024***
	[0.0075]	[0.013]	[0.017]	[0.0079]
Observations	1,144	1,144	1,144	1,010

Notes: Population data come from Census County Population Totals: the April 1, 2010 to July 1, 2015 vintage and the April 1, 2000 to July 1, 2009 vintages. The birth rate data come from the CDC's Natality data set, and birth rate is defined as the number of births divided by total population in the given year. Standard errors clustered by county. All dependent variables are in logs.

Table A7: Treatment effects after adjusting for population trends

	Difference-in-Differences for Under 5		Triple Difference for 10 and Under	
	Raw	Adjusted	Raw	Adjusted
3G	0.10***	0.14***	0.053	0.051
	[0.025]	[0.039]	[0.031]	[0.032]
Age<5			0.26***	0.31***
			[0.029]	[0.038]
3G*(Age<5)			0.051***	0.089***
			[0.015]	[0.019]
Observations	44,534	44,534	97,027	97,027

Notes: The dependent variable in the "Raw" columns is the log ED visits in hospital h at month t for age a. In the "Adjusted" columns, the log ED visits are adjusted based on 3G cohort population trends. A 3G cohort is all hospitals that receive 3G at the same month and year; those that do not receive it in the sample fall into the same cohort. Annual population trends are estimated from Census County Population Totals (see note on Table A6) by cohort at the county level then merged onto the NEISS data. Standard errors clustered by 3G cohort.