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Is there a Relationship between Parents' Screen Usage and Young Children's Development?

by

Delphine Nguyen

A thesis submitted to the University of Plymouth
in partial fulfilment for the degree of

DOCTOR OF PHILOSOPHY

School of Psychology

July 2023

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Author's Declaration

At no time during the registration for the degree of Doctor of Philosophy has the author been registered for any other University award without prior agreement of the Doctoral College Quality Sub-Committee. Work submitted for this research degree at the University of Plymouth has not formed part of any other degree either at the University of Plymouth or at another establishment. This thesis includes two submitted papers which can be found in Chapter 2 (Experiments 1, 2, and 3) and Chapter 3 (Studies 1 and 2). The paper of Chapter 2 involves shared co-first authorship by the candidate with Dr Nadine Fitzpatrick.

The author has collected and analysed the data of Experiments 2, 3, 4, 5 and Study 2. She analysed secondary data collected by Briazu, Floccia, and Hanoch (2021) of Study 1. It should be noted that the author did not collect nor analyse the data of Experiment 1, but Experiment 1 is included because together with Experiments 2 and 3, it is part of a paper currently accepted for publication (Nguyen, Fitzpatrick, & Floccia, in press).

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Is there a Relationship between Parents' Screen Usage and Young Children's Development?

Delphine Nguyen

Abstract

There has been growing concern over the links between children's screen time use and cognitive development (Halton, 2020). However, researchers have generally overlooked the possible impact of parental screen time, which might decrease the opportunities of learning and social interactions for young children. To address this gap, we investigated the relationship between parental screen use and toddlers' development. However, the start of this thesis coincided with the Covid-19 pandemic, and a few experimental tasks had to be adapted online. Thus, this thesis examined first whether online paradigms can provide valid data (word recognition, word learning and language assessment). Second, the main objective was to explore the relationship between parental screen use and young children's language skills, and to revisit the link between parental screen time and children's empathy.

Findings from Chapter 2 provide support for the reliability of online testing with children. These experiments point to promising avenues of investigation in early language studies, and to possibilities for reaching out to families around the world.

Findings from Chapter 3 revealed no impact of parental phone text on children's learning in a lab situation. However, they suggest that parental responses to technoference and attitudes towards smartphones may moderate the relationship between parental screen use and children's development. When examining effects in real life, a first exploratory study indicated an effect of parental screen time (in real life) on children's language vocabulary

when assessed via a parental questionnaire, at least for children aged 16 months and above. A second study was conducted with more objective measures of screen time and children's vocabulary knowledge, and no association was found between parental screen time and children's language when assessed via a standardised face-to-face language test.

Findings from Chapter 4 showed a negative association between children's alone screen time and their cognitive empathy abilities. However, parental screen time was not related to children's cognitive empathy.

The experiments and studies reported in this thesis fail to reveal a robust association between parental screen time and early language, at least in the population that we have studied here. Importantly, the findings suggest how parental screen use may be a moderator in children's development and not a causal factor. They demonstrate the need to investigate more precisely why and how parents use electronic devices such as mobile phones during interactions with their children, might directly influence early language and emotional development.

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1 Chapter 1: General Introduction

1.1 Thesis Overview

Children learn to talk and communicate through interactions with other people. The first few years of children's life is the most critical period for their language development, with language and vocabulary acquisition being directly related to the amount of speech provided by the parents, at least in many Western industrialised cultures. However, parents or caregivers seem to spend more time on electronic devices, such as smartphones and tablets which, in turn, may influence their interactions with their children. The growing use of screens has become an important topic in the media, raising public concerns about their effects on children's language and cognitive development, and social/emotional skills, with most research in the field of technology focusing on children and teenagers' usage rather than on parents'. While parents may be available most of the time with their young children physically, they may have become less socially and emotionally active with them (Kildare & Middlemiss, 2017). This distraction could affect the quality and quantity of time parents spend with their children, with consequences on children's language and socio-emotional development in the critical first years of their life.

This dissertation proposes to investigate the links between parents' usage of screen and young children's development in a series of studies and experiments involving families with young children aged 8 to 45 months, with a main focus on language development, but also on emotional development. Measures of language and emotional development will rely on a combination of parental questionnaires (e.g., Oxford Communicative Development

Inventories: Hamilton et al., 2000), and face-to-face measures (e.g., Preferential Looking Paradigm with eye-tracking; language assessment tool WING: Cattani, Krott, Dennis, & Floccia, 2019). Measures of parental and children's screen use will rely on one-time filled questionnaires (during their visit at the Babylab) and/or online daily questionnaires to complete over a week on an app.

1.2 External factors that influence children's language development

Infants start to understand familiar words from the age of 6 months (Tincoff & Jusczyk, 1999), with word production usually emerging by their first birthday (Huttenlocher, Waterfall, Vasilyeva, Vevea, & Hedges, 2010). At that age, infants typically say only a few words, and achieve the significant milestone of uttering approximately 50 words when they are around 18 months old. They then embark on a phase of language development in which there is a noticeable acceleration of word production and comprehension (Ganger & Brent, 2004; Waxman & Kosowski, 1990), acquiring 100 words by the time they are between 20 and 21 months (Pine, 1995). At 2 years old, they typically master between 200 and 500 words (Fernald, Pinto, Swingley, Weinberg, & McRoberts, 2001), and begin to combine words into two-word utterances. Young children become capable of producing grammatically long complete utterances (Hoff, 2013). By age 3, most children can put together multi-word utterances and participate in short conversations with others. They can partially comprehend what a person in saying to them and formulate an appropriate response in return (Haslett & Samter, 1997). Young children's language development is complex and influenced by several external factors. Whether human infants are predisposed

to acquire language through specialised (Chomsky, 1957) or general (Tomasello, 2003) learning mechanisms, it is undeniable that they learn from their environment and those whom they interact with daily.

Parental interaction with children is key to lexicon development (e.g., Dale, Tosto, Hayiou-Thomas, & Plomin, 2015; Rowe, 2012) at least in many Western industrialised cultures (in other cultures, parent-child verbal interactions do not always have a large influence on early language development: Cameron-Faulkner et al., 2021; Johnston & Wong, 2002). Spinelli, Fasolo, Shah, Genovese and Aureli (2018) investigated whether the quality of maternal input moderates infants' linguistic competence. They showed that infants of mothers engaging in more diverse lexical variability and syntactic complexity to their child at 6 and 9 months showed improved language skills at 18 and 24 months. Another indication that quality of parental language input is one of the best predictors of children's language development comes from a study by Ferjan Ramírez, Lytle and Kuhl (2020). In this study, 6-month-old infants were recruited, and some parents received coaching to enhance parental language. Parent coaching interventions were delivered at 6, 10, and 14 months. On coaching appointments, they received linguistic feedback and listened to language input in their own recordings, to subsequently discuss activities that foster the development of language skills. The number of adult words, the conversational turn count, and the child vocalisation count were assessed at 18 months. Children whose parents did the coaching intervention increased their production of speech between the age of 6 and 18 months (Ferjan Ramirez et al., 2020). Their parents were more engaged in conversational turns and produced more words at 18 months compared to those whose parents did not receive the coaching. It was

also found that an increase in ‘parentese’ speech (baby talk) and parent–child vocal interactions between 6 and 18 months boosted both language and child vocalisations growth during the same time period.

Children’s expressive language abilities are influenced by the amount of information received. Furthermore, as parents shape the environment that their children experience, they are most likely to have an impact on the learning environment at home (Skibbe et al., 2008). Indeed, the home literacy environment is considered to be a strong predictor of language development (Liebeskind et al., 2014). Effects of shared reading on receptive and expressive language development have been investigated (Noble et al., 2019). It was found that children who are read to before the age of one year have more advanced language skills (Dunst, Simkus, & Hamby, 2012). Also, according to a study by Mol et al. (2008), young children who experienced shared book reading during their preschool years (0–5 years) pick up language more quickly and have a wider vocabulary when they start school.

Oral language skills are one of the best indicators of future success in education and readiness for school (Hoff, 2013; Burchinal et al., 2016). Previous studies suggest that socioeconomic status (SES) is strongly associated with academic achievement. For example, in Noble et al. (2015), 90 infants from socioeconomically diverse families were followed at 9 and 15 months, and 89 were followed at 15 and 21 months. Questionnaires and surveys were administered to assess the household socioeconomic status and the adults’ life events over the past year. The Preschool Language Scale-4 (PLS), a standardised language measure, was used to evaluate the receptive and expressive language development of young children using a series of interactive tasks specifically designed to evoke progressively more

advanced language abilities. Noble et al. (2015) found that socioeconomic disparities in language outcomes are already present by the second year of life. Indeed, children from the highest SES families performed above average on the PLS-4. Moreover, results showed that parental warmth (e.g., “Parent caresses/kisses/hugs child at least once during visit”) may be the factor that explains the connection between parental education and the development of language skills. However, when taking into account parental warmth, the influence of the learning and literacy environment on language development was no longer significant. Additionally, Noble et al. (2015) suggest other mediating factors such as the quantity and quality of parental speech at home which have been related to SES differences in language development. Disparities in the quantity and quality of speech could potentially account for SES differences in children's vocabulary acquisition.

However, Sperry, Sperry, and Miller (2018) suggest that the variability in the amount of speech directed by primary caregivers towards their children within a community cannot be solely predicted based on SES. They attempted to replicate Hart and Risley’s (2003) claim that children living in poverty heard fewer than a third of the words heard by children from higher-income families. They found that the number of words that primary caregivers of their low-income sample directed to children was nearly as great as Hart and Risley’s (2003) high-income community (1,838 words per hour versus 2,153 words). In response to their findings, Golinkoff et al. (2019) stated that there are serious consequences to claim that low-income children hear high-quality language as much as peers from higher income homes. So, Sperry, Sperry, and Miller (2019) specified that their results did not support Hart and Risley’s (2003) claim under Hart and Risley’s single definition of the verbal environment

(speech addressed to the child by a primary caregiver). Indeed, Hart and Risley (2003) recorded the number of words heard by the child from the parent, the vocabulary quality and the interaction style used. The researchers started the observations when the children were 7 months until they turned 3 years old and began to spend time outside the home. On the other hand, Sperry et al. (2019) study included observing children starting at 18 months to 4 years old and explored three definitions of the verbal environment: speech addressed to the child by primary caregivers, speech directed towards the child by other family members, as well as the surrounding ambient speech within the child's hearing. Moreover, Sperry et al. (2019) mentioned that their study did not report the measures of the quality of vocabulary and focused only on the number of words spoken to the child and the nature of children's everyday verbal environments. In summary, SES can be a predictor of children's language development, probably because it modulates the quantity and quality of language input provided to the children.

1.3 External factors that influence children's emotional development

Infants around 4 months of age seem to start developing the cognitive concept of self (Sachs, Kaplan, & Habibi, 2019), that is, being a separate entity from others (Rochat & Striano, 2002). At 1 year of age, they are aware that emotions are frequently directed towards objects and people (e.g., "He likes dogs"; "She's angry at her mum") (Phillips, Wellman, & Spelke, 2002). At 2 years old, they begin to develop cognitive empathy which is built on the understanding of other people's emotions (Blair, 2005; Noten et al, 2019; Smith, 2006). At that age, they can categorise facial expressions based on emotions and recognise whether

a person feels happy or sad (Widen & Russel, 2008). Indeed, Widen and Russel (2003) indicate that when children aged between 2 and 5 years old were asked to name facial expressions, they use most frequently the labels happy, sad, and angry. Then, according to Hoffman (1990), at the age of 3, children are able to feel sympathetic and empathetic with others' emotions via perspective. Indeed, they can describe the causes of emotions about a situation (e.g., "He is happy because he is getting a dog") (Grazzani et al., 2018; Harter & Whitesell, 1989; Wellman & Banerjee, 1991). Additionally, infants typically exhibit a natural inclination to focus their attention on people which can be referred to as social attention (van Zonneveld et al., 2017). Biological stimuli such as facial features, eyes, voices, and body movements tend to captivate their attention early on in their development (Dawson, Bernier, & Ring, 2012). Social attention is necessary to recognise people's emotions and can be considered as precursor to empathy (Bons et al., 2013). Young children rely on their caretakers to learn to recognise facial expressions and pick up on these visual clues. Emotion recognition in children is strongly related to social influences of the family and environment (Castro, Halberstadt, Lozada, & Craig, 2015).

One of the mechanisms of emotion learning is through the observation of the way parents interact within the family environment. Relations in the family and the way mothers and fathers express their emotions influence the emotional development of a child (Dunn & Brown, 1994; Parke, Cassidy, Burks, Carson, & Boyum, 1992). Indeed, children model specific behaviours (Bandura, 1977) by observing the reactions of others in order to know how they should act in similar conditions. For instance, Denham and Kochanoff (2002) recorded parental emotional expressiveness via observation at home during a 2- to 4-week period for

each family and via parental-self report which included for instance: “Telling family members how happy you are‘’ and “Blaming one another for family troubles” (Self-Expressiveness in the Family Questionnaire; Halberstadt, Cassidy, Stifter, Parke, & Fox, 1995). Children’s understanding of emotion was measured with the Affect Knowledge Test (e.g., Denham, 1986) which consists of recognition and labelling of emotional expressions, identification, and inferences of emotions appropriate to specific situations (Denham & Kochanoff, 2002). Results showed that mothers’ positive emotions (observed and self-reported), and their readiness to assist children in managing emotional distress by allowing them to express their feelings, finding strategies to cope with those emotions, and resolving the underlying issues, were all positively associated with children’s emotional understanding at ages 3 and 4.

Moreover, previous studies suggest that parenting styles play a role in children’s socio-emotional development (Aunola, Ruusunen, Viljaranta, & Nurmi, 2015; Hart, Newell, & Olsen, 2003). Parenting styles are described as typical parental attitudes and actions that shape the emotional environment in which parents raise their children (Darling & Steinberg, 1993). In a study by Zarra-Nezhad et al. (2015), a Finnish version of the Block’s Child Rearing Practices Report (Roberts, Block, & Block, 1984) was used to assess the parenting styles of both parents. They were asked to rate items intended to measure 3 different aspects of parenting styles: affection (e.g., “I often show my child that I love him/her”), psychological control (e.g., “My child should be aware of how much I sacrifice for him/her”), and behavioural control (e.g., “My child should learn how to behave properly toward his/her parents”). Children’s (aged 7 years) emotions were measured based on the Daily Emotion

Scale (Aunola & Nurmi, 2007) and the parents completed a daily questionnaire across one week by rating items including “My child was angry today”; ”My child was sad today”; “My child felt distressed today”. Findings indicated that both mothers’ and fathers’ parenting styles were associated with negative emotions among children: parental psychological control was related to high levels of negative emotions among children, and mothers’ high affection and behavioural control were related to low levels of negative emotions.

Children’s emotional skills are related to how parents interact daily with them which in turn might be influenced by their financial situation, attitudes and access towards education resources. Research suggests a link between parental socio-economic status and children’s socio-emotional development (Gershoff, Aber, Raver, & Lennon, 2007; Hartas, 2011). For instance, in Hartas’ (2011) study, the socio-economic measures included the family income and the maternal educational qualifications. Children’s socio-emotional competence were obtained from teacher ratings at the end of the first year of primary school (age of 5 years). Teacher ratings involved assessment scales on children’s social and emotional progress based on continued observation during the first year and achievements described in the Early Learning Goals and guidance for the Foundation Stage (Hartas, 2011; Qualifications and Curriculum Authority, 2000). Findings demonstrated that the income level of the family and the educational qualification of the mother influenced children’s scores in social/emotional development.

To summarise, infants as young as 4 months begin to understand themselves as separate entities from others (Sachs et al., 2019), and at one year, they are aware that emotions are often directed towards people and objects (Philipps et al., 2002). At 2 years, their cognitive

empathy develops, and children can categorise and label facial expressions based on emotions (Widen & Russel, 2008). At 3 years, they can describe the causes of emotions via perspective taking (Grazzani et al., 2018). Children's emotional development is influenced by their environment. The way parents interact within the family environment such as their emotional expressiveness, as well as parenting styles, play a role in children's emotional learning (Denham & Kochanoff, 2008; Zarra-Nezhad et al., 2015). The socio-economic status of the family is also related to children's socio-emotional development. Children's emotional skills are related to how parents interact with them on a daily basis, which can be influenced by the family's financial situation, attitudes, and access to educational resources (Hartas, 2011).

It is important to address that emotional and language development are intertwined. From birth, humans use language to communicate their feelings, needs, and desires. Infants begin to express their emotions through nonverbal cues, such as crying when they are hungry or in pain (Bowlby, 1958; Pally, 2001). As they grow, they use language to describe their emotions and communicate their feelings (Bloom, 1998). For instance, as previously mentioned, learning to label emotions with words is an essential step in emotional development and children will typically begin to use the labels happy, sad, and angry at 2 years old when they acquire the vocabulary necessary (Widen & Russel, 2008). Additionally, language provides a means for children to regulate their emotions. They can learn to talk about their feelings, which helps them understand and manage their emotional experiences (Cole, Armstrong, & Pemberton, 2010). Furthermore, language enables children to understand and relate to the emotions of others. Indeed, research suggests that children's

language and conversational abilities can both directly and indirectly impact their empathetic responses and behaviours (Ornaghi, Conte, & Grazzani, 2020; Ornaghi et al., 2017). Moreover, it is essential to note that language also carries cultural norms and values related to emotional expression. Different cultures may encourage or discourage the expression of certain emotions (Engelmann & Pogosyan, 2013). As children learn their native language, they internalise these cultural norms (Vygotsky, 1978), shaping their emotional expression and understanding (Jaramillo, Rendón, Muñoz, Weis, & Trommsdorff, 2017).

To summarise, the interaction between language and emotional development is a dynamic and bidirectional process. Language not only serves as a medium for expressing and understanding emotions but also plays a fundamental role in the overall emotional and social development. It enables children to share and regulate their emotions, and develop empathy, ultimately contributing to their ability to navigate in society.

1.4 Children's screen time and children's development

Previous studies have established that children's language development is greatly influenced by the amount and the properties of the language(s) they receive in their environment. Screens have become an additional part of the children's environment. Technology has changed how we learn and acquire knowledge and engage with one another. Some researchers now consider it to be a part of children's home literacy environments (e.g., Knowland & Formby, 2016). Indeed, language learning in infants can be enhanced by watching videos, as long as it is in the presence of peers as opposed to learning alone (Lytle, Garcia-Sierra, & Kuhl, 2018). For instance, infants' ability to discriminate foreign language

phonemes (the consonants and vowels constituting words) develops better within a social context. We know that babies are able to discriminate phonetic contrasts from birth (Jusczyk, Cutler, & Redanz, 1993), but at the end of the first year, as they become specialised in their native language, they lose the ability to distinguish non-native contrasts. This process is known as perceptual narrowing. Younger infants often display a heightened readiness to react to potential social cues than older infants, as evidenced by their increased sensitivity to non-native speech sounds, unfamiliar facial expressions related to speech, and the features of unfamiliar faces (Maurer & Werker, 2014). Lytle et al. (2018) examined phonetic learning with the head turn task and a brain measure, event related potentials (ERPs), to evaluate infants' ability to discriminate foreign-language sounds. When exposed to sounds from their native language, individuals experience a phenomenon known as mismatch negativity (MMN), which is characterised by a negative waveform. However, when listening to speech sounds from a non-native language, this MMN response is either reduced or absent. The MMN associated with tasks requiring minimal attention exhibits a negative polarity. Conversely, a positive polarity is observed when high attention is demanded, such as in challenging speech discrimination tasks involving non-native speech (Lytle et al., 2018). In their study, Lytle et al. (2018) conducted research with 9-month-old infants who were able to control video presentations using a touch screen. Each touch on the screen triggered a 20-sec clip featuring a Mandarin speaker discussing toys and books. Infants in the individual condition participated in all study sessions alone, while those in the paired condition always engaged in the study sessions alongside another infant. When listening to a foreign language, for those who participated with another infant in the study

sessions, a negative polarity was elicited, whereas for the individual-learning condition, a positive polarity was found. The paired-infants processed the Chinese-Mandarin phonetic distinction with less effort compared to infants in the individual-learning condition. Moreover, infants who participated in individual-learning sessions exhibited increased attention towards their own caregiver and the screen compared to infants involved in paired-exposure sessions. These findings imply that motivation is increased by social connection to another person. Infants' learning of the meaning of words is aided by eye contact and other social cues. The purpose of the paired condition was to give infants more motivation in the form of a peer partner (Lytle et al., 2018). Results showed the importance of motivational mechanisms and social interactivity.

Therefore, interactive videos can help children in their learning. Moreover, other studies show different benefits of using interactive media (Galetzka, 2017). For instance, Radesky, Shumacher and Zuckerman (2015) described that eBooks and reading apps for children can help them develop their literacy by learning new words, phonics and to improve their word recognition. Moreover, it was shown that children can learn better from interactive media rather than from traditional support (Wang et al., 2016): children who were taught clock reading using an iPad touchscreen app outperformed those who learned from paper drawing. Thus, children transferred knowledge skills from touchscreen to physical objects. Besides, it was found that younger children benefited from touchscreen interaction that accompanied a word learning task (Kirkorian, Choi, & Pempek, 2016). Touchscreens enhance the learning process in younger children by promoting the selective attention mechanism (Choi & Kirkorian, 2016). Interactive media could be considered as a learning tool in

educational contexts. Moreover, Madigan et al. (2020) conducted a meta-analysis from 42 studies (on children younger than 12 years old) to examine the associations between the duration of children's screen time and background television (TV), educational programming and co-viewing, and children's language skills. They found that children's co-use of screen was positively associated with their language skills. In contrast, they reported that children's alone screen use was associated with lower language skills (see also Mustonen, Torppa, & Stolt, 2022).

The literature reviewed showed that screen time has both beneficial and detrimental impacts on a child's development. Indeed, non-interactive media where the user will be merely an observer/viewer and not be in active participation such as watching TV can negatively influence children's development. Many studies that were conducted to investigate the relationship between children's screen time and their language development have reported an association between excessive screen time in young children and language delays (e.g., Zimmerman, Christakis, & Meltzoff, 2007). In this research, children from 8 to 24 months old were recruited. The children's normed score on the short-form McArthur-Bates Communicative Development Inventory (CDI) (Fenson, 2007) was used to measure their language development. The CDI consists of a vocabulary list for which both comprehension and production are assessed through a parental questionnaire. SES and parent-child interactions (how often the parent would read, story-tell, or listen to music with the child) variables were controlled. It was found that children who spent more time watching videos tended to use fewer words. For every extra hour of video exposure per day among infants aged 8 to 16 months, their average word production decreased by

approximately six to eight words. However, among toddlers (17 to 24 months), there were no relevant associations between media exposure and CDI scores. This suggests that the association observed in younger children between media exposure and language skills might fade away as the children reach their toddler years. The influence of these media may only be transient according to Zimmerman et al. (2007). Note that the researchers did not conduct experiments to directly determine whether watching videos has a beneficial or detrimental effect on vocabulary development. There might have been other confounding variables such as the CDI, a parental report of the child's vocabulary knowledge which could be biased in that context (if children spend more time on screen, that gives less opportunities for parents to evaluate their language skills). Also, the study did not assess the direct amount of time parents spend engaging in verbal communication with their infants and did not control for the quality of parent-child interactions.

Other findings suggest that too much exposure to screen time can have an impact on language development. In a study conducted by Ma and Birken (2017), parents reported their child's daily screen time, and 894 children were included. By their 18-month check-ups, 20 percent of the children were using handheld devices (which were not educational as the ones previously described) for an average of 28 minutes a day. Then, a screening tool was used to test language delays. The results suggest the likelihood of delayed expressive language abilities in toddlers increased with increased exposure to handheld screens (the ability to say words and sentences). Also, the risk of expressive language delay increased by 49% for every 30-minute rise in daily screen time. To investigate the mechanisms underlying the association between handheld screen time and speech delay, further research is

required to understand the types and contents of screen activities infants are participating in.

Moreover, screen time can be associated with the development of emotion in children. In a study by Skalicka et al. (2019), the Norwegian version of the Test of Emotion Comprehension (TEC) was used to gauge children's emotional comprehension (Harris & Pons, 2000). The TEC is designed for children between the ages of 3 and 11 and evaluates nine aspects of Emotion Understanding, including Recognition, External cause, Desire, Belief, Reminder, Hiding, Regulation, Mixed, and Morality. Parents were interviewed at child's aged four and six to estimate how much time children spent watching TV and playing video games using tablets, computers, game systems, and phones. Results showed that increased screen time at age 4 was associated with poorer levels of emotional comprehension at age 6. Additionally, the presence of TV in a child's bedroom at age 6 predicted a decline in emotional comprehension by the age of 8. Skalicka et al. (2019) offered several explanations for their findings and suggested that they are in line with the displacement hypothesis which claims that increased screen time, including having a TV in the bedroom, leads to reduced amounts of time allocated for direct, high-quality interactions between children and their parents (Mutz, Roberts, & Vuuren, 1993). This, in turn, may limit opportunities for children to learn and practice essential social and emotional skills. Additionally, the presence of a TV in the bedroom is associated with increased television viewing. Finally, it is likely that children engage in more solitary viewing, resulting in fewer discussions centred around the emotions of TV characters with their parents.

Furthermore, electronic screen media use has been associated with sleep problems (Vijakkhana, Wilaisakditipakorn, Ruedeekhajorn, Pruksananonda, & Chonchaiya, 2015). Adequate sleep is crucial for the typical neurocognitive functioning of children. Nevertheless, the amount of quality sleep each child gets can be influenced by their exposure to screen media. In this study, 208 infants at 6 months of age were recruited and followed-up at 12 months of age. At each visit, the infant's sleep onset and wake time were recorded in a sleep diary. The length of night-time sleep was then calculated for both groups. At both visits, an assessment of the household's screen media exposure was conducted. Twelve months old infants who were subjected to screen media in the evening had a 28-min reduction in night-time sleep duration on weekdays. Furthermore, infants at both ages 6 and 12 months who were subjected to screen media after 7pm had shorter 12-month night-time sleep duration compared to infants who were not exposed to screens in the evening (Vijakkhana, et al., 2015).

Other evidence demonstrates the effects of TV exposure on more general developmental skills among young children. Lin, Cherng, Chen, Chen, and Yang (2014) investigated whether screen time affects cognitive and motor developmental skills. Their study included 75 children who had regular exposure to TV and 75 children who either had no exposure or minimal exposure to TV, all ranging in age from 15 to 35 months. The Bayley Scales of Infant Development-second edition (BSID-II) was used to assess cognitive, language and personal social abilities. The Peabody Developmental Motor Scales-second edition (PDMS-2) assessed fine and gross motor skills (Lin et al., 2014). Findings showed that the amount of time young children spent watching TV was related to delays in cognitive, language, and

motor development. Children who did not have developmental delays tended to spend less time engaging with screens compared to children with developmental delays.

Additionally, researchers demonstrated that lifetime TV exposure is significantly linked to poor executive function (EF) performance (Nathanson, Aladé, Sharp, Rasmussen, & Christy, 2014). Parents reported how many hours their child spent on TV viewing during the day. Four tasks tapping onto EF skills were selected for 107 pre-schoolers (3 to 5 years old) to perform. The results indicated that children who had spent less total time watching TV demonstrated stronger EF compared to those who had watched more hours of TV in total. These findings suggest the effects of screen time on young children extends to executive functions (Nathanson et al., 2014). Neuropsychologists find EF to be a significant construct in identifying individuals with brain-related disorders or abnormalities, including attention-deficit/hyperactivity disorder (ADHD) (Barkley, 1997). Similarly, developmental psychologists are interested in EF due to its involvement in challenging everyday behaviours, such as poor self-regulation, struggles with task focus, and difficulties in acquiring literacy skills (Lonigan, Allan, & Philips, 2017).

Besides, previous studies demonstrate that too much TV exposure can be detrimental for children's development because they can miss out on interactions. Young children may lack adequate time and opportunities to develop their cognitive and motor abilities. Anderson and Pempek (2005) reviewed previous research concerning TV and young children. They reported that children spend more time watching TV compared to the early 1990s. TV can be distracting when children try to do other activities such as play or interacting with members of the family. Indeed, when interrupted during fantasy play, it was found that

children have difficulties returning to play (e.g., less engagement and involvement or refuse to play) (DiLalla & Watson, 1988). As child's play and social interactions can contribute to young children's development (Alessandri, 1992), TV could disrupt and reduce parent-child interactions thus inducing negative effects of TV on the child (Anderson & Pempek, 2005).

Additionally, excess TV exposure can make children less alert. Christakis, Zimmerman, DiGiuseppe, and McCarty (2004) investigated the links between early TV watching and subsequent symptoms of attention disorders. Time spent TV watching was assessed from parental reports when children were 1½ and 3½ years of age. Results showed a positive association between TV viewing at 1½ of age and having symptoms of attention disorder. However, researchers explained that although it might be possible that early TV watching causes later symptoms of attention disorder, there is also a possibility that children with attention disorders may exhibit a preference for early TV viewing.

Screen use can both positively and adversely impact children's language development depending on the type of content. As previously mentioned, previous research has shown that spending time alone on TV and handheld devices for non-educational purposes can negatively affect children's language skills (Ma & Birken, 2017; Zimmerman et al., 2007). On the other hand, it was shown that educational shows can help improve children's cognitive and literacy skills (Anderson, Huston, Schmitt, Linebarger, & Wright, 2001). A literature review by Guellai, Somogyi, Esseily, and Chopin (2022) pointed out the differences on the impact of content designed explicitly for infants and young children and that which is intended for a mature audience. They cited Chonchaiya and Pruksananonda's (2008) study

who found that the likelihood of experiencing delayed language acquisition was tripled when children watched adult-oriented programs instead of child-directed ones during the period from 15 to 48 months of age. In addition, Guellai et al. (2022) mentioned Linebarger and Walker (2005) who observed that children exhibited enhanced language skills when they watched programs featuring a compelling storyline and characters who directly engaged with the child, offering moments for the child to respond (such as in the case of "Dora the Explorer"). Conversely, viewing programs with a less structured narrative and featuring intricate stimuli (as exemplified by "Teletubbies") was linked to weaker language proficiency in children.

Furthermore, the use of cartoon images is prevalent in educational media notably apps created for children's use. An interesting study by Zhang, Wu, Yu, and Li (2023) investigated how cartoon images in touchscreen media influence young children's ability to recognise time. The research included 92 children aged 4 to 6 years old who were randomly assigned to one of four experimental conditions with a 2 (clock type: cartoon clock, non-cartoon clock) × 2 (media type: touchscreen, video) design. The media was designed for 10 minutes to instruct children in telling time. Findings showed that children in the touchscreen groups achieved significantly higher overall scores in learning how to read the clock compared to the video groups. Moreover, children in the groups exposed to cartoon images achieved significantly higher overall scores in learning how to read the clock compared to those in the non-cartoon groups. These results indicate that cartoons can facilitate children's learning in educational touchscreen apps (Zhang et al., 2023).

To the best of our knowledge, there have been no studies on a direct comparison between the different types of screen use, however there are findings that suggest that young children are likely to learn more from touch screen devices than TV which is more passive viewing (Anderson et al., 2001; Kirkorian et al., 2016). Indeed, when children engaged in physical interactions with screens (which is more often with tablets or smartphones, and not commonly with TVs), they demonstrated superior learning outcomes compared to other groups, such as those in traditional classroom settings or those learning through video chats (Roseberry, Hirsh-Pasek, & Golinkoff, 2014, see also meta-analysis of Xie et al., 2018). Engaging in interactive screen activities, such as video calls with family members, remote storytime sessions can be advantageous for their development due to the interactivity involved (Gaudreau et al., 2020).

In summary, in many Western industrialised cultures, parental interaction with children is key to children's lexicon (e.g., Ferjan Ramirez et al., 2020; Spinelli et al., 2018) and emotional development (Denham & Kochanoff, 2008; Zarra-Nezhad et al., 2015). Moreover, the home literacy environment is considered to be a strong predictor of language development (Liebeskind et al., 2014) and children who experience shared reading before the age of one year have more advanced language skills (Dunst, Simkus, & Hamby, 2012). Finally, SES can be a predictor of children's language (Noble et al., 2015) and socio-emotional development (Hartas, 2011), probably because it modulates the quantity and quality of parent-child interactions.

As technology has become an additional part of the children's environment, some researchers now consider it to be a part of children's home literacy environments (e.g., Knowland & Formby, 2016). Findings show that language learning in infants can be enhanced with interactive media such as viewing videos with peers, reading eBooks, learning with touchscreen apps how to clock read (Lytle et al., 2017; Radesky et al., 2015; Wang et al., 2016). While children's co-use of screen was positively associated with their language skills (Madigan et al., 2020), children's alone screen use was associated with lower language skills (e.g., Madigan et al., 2020; Mustonen et al., 2022). Non-interactive media where the user will be merely an observer/viewer can negatively influence children's development. Studies suggest a negative relationship between children's screen time such as TV viewing, use of handheld devices and their language skills (Birken et al., 2017; Zimmerman et al., 2007). Moreover, children's electronic screen media use has been associated with emotion understanding protracted development (Skalicka et al., 2019), sleep problems (Vijakkhana et al., 2015), and cognitive and motor developmental delays (Lin et al., 2014; Nathanson, et al., 2014). Also, screen use can reduce parent-child interactions thus inducing negative effects on the child's development (Anderson & Pempek, 2005).

1.5 Parental screen time and children's development

Most research in the field of social media has examined the effects of their use by children and teenagers rather than focusing on the parents' screen time (Corkin et al., 2021; Kildare & Middlemiss, 2017). However, caregivers themselves seem to spend more time on mobile devices such as cell phones and tablets, which may impact their interactions with their

children. Zhou et al. (2002) conducted a longitudinal study that points to the importance of parental warmth in parent-child interactions for children's emotional development. Children's empathy was assessed at around 9 years of age and again 2 years later. During both of their lab visits, they viewed a series of slides (similar to Buck, 1975) showing pleasant, unpleasant and neutral pictures. Their facial reactions to the slides were rated by undergraduate student observers. Also, children were asked to indicate how the slides made them feel. During both visits, parental warmth (smiling, laughing, positive voice of tone, verbal and physical affection) directed toward their children during the parent/child slide procedure was observed and rated as well (Zhou et al., 2002). Findings revealed that parental warmth in interactions was positively related to children's empathy, especially for older children. Thus, a reduction of parents' involvement when interacting with their children might negatively affect the development of empathy in children.

Even if parents might be more available to physically spend time with their young children, they might be less socially and emotionally active with them (Kildare and Middlemiss, 2017) because of mobile phone distraction. eMarketer reported in 2019 that the average US adult will spend close to 3 hours daily on their smartphone (Wurmser, 2019). This line of research involves the collection of primarily survey data surveys targeting adult participants, inquiring about their habits regarding media usage. Additionally, data was obtained from online and mobile activity tracking services, government sources, and interviews conducted with industry experts (Wurmser, 2019). Additionally, Myruski et al. (2017) reported that children aged 7 to 24 months expressed more distress, and were less likely to explore their environment, when their mother was using her cell phone. In the same study, parents

reported their own daily use of devices. They were asked to quantify their screen time in front of their family (less than 30 min, 1 hour, etc) and in front of their infant. When their mothers did turn off their phones, the young children whose mothers reported higher habitual use of mobile devices outside the lab displayed more negativity and less emotional recovery in the lab situation.

Parents being distracted by smartphones while around children has become common (McDaniel, 2019). To explore how cell phones interfere with healthy parenting, Radesky et al. (2014) assessed 55 caregivers' behaviour at fast food restaurants in the Metropolitan Boston area. Many parents quickly took out their phones as soon as they sat down, and the majority continued using them throughout the meal, often showing more engagement with their devices than with their children. Children whose parents were engrossed in their phones were more prone to behaving in a silly or noisy manner. Moreover, parents who used cell phones appeared to be irritable and impatient, which ultimately contributed to worsened behaviour. Radesky et al. (2014) stated that mobile devices can distract parents from face-to-face interactions which would lead to children missing out essential development milestones (Glascoe & Leew, 2010). However, this study has limitations such as the lack of objectivity in measurement as researchers wrote field notes and did not take videos of their observations. In addition, small sample sizes and evaluation of parent digital technology used only during brief episodes like meals or playground trips can be limitations in the emerging literature on parent media use and parent-child interactions (McDaniel & Radesky, 2017).

Another reason why parental screen time, including TV watching, might have a negative impact on children's development is that their behaviour might set a bad example for children. Parental TV-watching has been linked to children getting too much screen time (Bleakley, Jordan, & Hennessy, 2013). Their study included 1550 parents with children in three age groups (children under 5 years, children aged 6–11 years, and adolescents aged 12–17 years). The researchers inquired about the amount of time parents dedicated to watching TV, DVDs, movies, and shows on computers. They also gathered data on the number of rooms in the house that had internet-connected computers and the enforcement of rules regarding watching time. The findings indicated that, on average, parents spent approximately four hours per day watching TV, while children watched three hours. Additionally, for every hour of TV time for adults, their children tended to have an additional half-hour of viewing time. According to Bleakley et al. (2013), children tend to imitate their parents' TV viewing habits, and this influence is more significant than the physical placement of the TV or the specific viewing rules that parents attempt to enforce. This is consistent with Anderson and Pempek 's (2005) suggestion that TV can reduce parents and children's interactions, thus inducing negative effects of TV on the child. In summary, evidence shows that parental screen time interferes with children's behaviour and learning opportunities, both in terms of their own screen time, and in terms of parent-child interactions. However, this interference is not tied to the child's language development, and this thesis asks whether parental screen use might have an effect on language learning as well.

Electronic devices distraction might influence the quality and quantity of time that parents spend with their children, which would translate into a drop in the amount of exposure to

language, slowing down language development. Previous studies (e.g Corkin et al. 2021; Mustonen, Torppa, & Stolt, 2022; Nabi & Wolfers, 2022; Reed, Hirsh-Pasek, & Golinkoff, 2017) have investigated how parental screen time can be associated with the language and emotional development in young children. Nabi and Wolfers (2022) examined whether parental screen activities can be associated with children's (aged 5 to 12 years) general emotional intelligence, empathy, and emotional regulation skills. Four hundred parents were given a questionnaire about their own media use and their co-use with their children. To measure children's emotional intelligence, scales derived from the conceptualisation of emotional intelligence (Salovey & Mayer, 1990) were filled by the parents (e.g., "My child knows when s/he is happy", "My child exhibits emotional control by emphasising positive and deemphasising negative emotion"). Additionally, parents reported their children's emotional regulation from Shields and Cicchetti (1997)'s checklist (e.g., is impulsive; displays exuberance that others find intrusive or disrupting. To assess children's empathy, they were also asked to complete a seven-item empathic subscale of the Davis (1983) reactivity index that Nabi and Wolfers (2022) adapted. For example, an item could include "I would describe my child as a pretty soft-hearted person" and was measured on a 1 (does not describe my child well) to 5 (describes my child well) scale (Nabi & Wolfers, 2022). Their findings demonstrated that parents' use of mobile devices was negatively related to children's general emotional intelligence. However, their results did not show parental media use to be associated with either children's empathy or emotional regulation. One of the limitations of this study could be that children's emotional development and parental media use were not objectively assessed as they both were reported by the parents.

On the other hand, Mustonen et al. (2022) found parental screen time effects on children's vocabulary. Finnish mothers of 164 children (aged between 2.5 and 4.1 years) filled a one-time questionnaire to report their children's screen time, as well as their own, on both a typical weekday and a day off. Children's language skills were assessed using validated tests (Finnish Phonology test; Kunnari, Savinainen-Makkonen, & Saaristo-Helin, 2012, and the Finnish version of the MacArthur Communicative Development Inventories III; Stolt, 2023). The maternal education level and birth order variables were controlled. The findings revealed a negative association between mothers' screen time and their children's vocabulary skills, but not phonological skills. It should be noted that Mustonen et al. (2022) only asked mothers how much time they spent using screen devices, but did not ask them specifically to estimate their screen time when their child is around, which would be a more direct test of the links between parental screen time and child language development.

Reed et al. (2017) found an association between parental cell phone use and children's language skills. They conducted a within-subjects study with 38 mothers and their 2-year-olds to test the impact of parental cell phone use on children's verb learning (Reed et al., 2017). Parent-child dyads were brought into a room. During the teaching phase, mothers were asked to teach their children two new verbs (blicking, which was to mean "bouncing," and frepping, which was to mean "shaking"). Mothers were given 60 seconds to teach the first novel verb. Then, they proceeded to teach the second verb after receiving instructions from the experimenter through a phone call to do so. Another 60-s teaching period ensued and concluded when the experimenter knocked on the door (Reed et al., 2017). During one of the teaching periods, (specifically after 30 seconds had elapsed), the experimenter made

a phone call to the mothers in which they talked to each other for 30 seconds. Then, mothers had another 30-s to teach the target word. Total teaching time for one word in this interrupted condition was still 60-s, the same as in the uninterrupted condition. Children's verb learning was indexed by their preference for matching and nonmatching actions before and after each session in a preferential looking task. It was found that children in the interrupted condition did not show evidence of learning the target verbs while in the uninterrupted condition they succeeded. This suggests that parental distraction through the use of mobile devices is negatively associated with word learning in young children. Reed et al.'s study (2017) informed us about the effects of parental screen time on children's language learning skills in an artificial lab environment: the explicit verb learning interaction is probably not a situation that occurs in most children's experience. Taken together, whether parental screen use interferes with real-life opportunities for language development, and in children younger than 2.5 years (given the results by Mustonen et al., 2022), is an open question.

The aim of this thesis is to contribute to existing knowledge by addressing the gaps mentioned above. Firstly, previous research mostly focused on children's screen time rather than the parents', whose availability for language-rich interactions might be consequently reduced. Secondly, studies that investigated the relationship between parental screen time and children's language development were mostly conducted in a laboratory situation, questioning the implications of these findings for children's learning outcomes in real life. To the best of knowledge, this is one of the first studies to investigate the association between parental cell phone use habits and the language development of children younger

than 2.5 years, observed in real life, and not in a lab situation (see the review by Morris, Filippetti, & Rigato, 2022). Finally, there is a need to revisit the links between parental screen time and children's emotional development.

From this research we may derive recommendations to parents to remind them to limit their own screen time especially when their children are around in today's media-consumed world, so as to not hinder their child's development. Additionally, this thesis seeks to fill gaps found in previous research by offering a perspective on parental screen media use that examines both children's language and emotional developments.

1.6 Research Aims

The primary objective of this work was to investigate the association between parents' screen time and young children's language and emotional development. However, the start of this thesis coincided with the Covid-19 pandemic, so an initial, mandatory stage was to adapt the paradigms that we planned to use to online delivery. Therefore, the work presented here was conducted in two stages. In the first stage (Experiments 1, 2 and 3), we investigated the impact of online testing on the quality of data, by adapting two paradigms widely used in infant research and a language test to be run online with 17 months and 26 months children. The second stage (Experiments 4 and 5; Studies 1 and 2) explores the relationship between parental screen time and children's development. Experiment 4 was conducted on toddlers aged 17 to 19 months to examine whether parental phone texting interrupts word learning in children in a lab situation. In Study 1, parents with children aged 8 to 29 months completed a parental questionnaire to estimate their child's vocabulary size

(the Oxford Communicative Development Inventories, Hamilton et al., 2000) and a survey estimating their screen time and real-life habits. Study 2 involved more objective measures using a standardised face-to-face vocabulary test (WinG) with children aged 19 to 32 months, and a daily questionnaire about parental cell phone use over a week. Experiment 5 was conducted on children aged 3 years to investigate the association between children and parental screen media use on children's emotion recognition. Details of the methods used in each experiment are provided in the following chapters.

1.7 Research Questions

This thesis seeks to provide answers to the following key questions in light of the theoretical frameworks we previously addressed and the knowledge gaps we identified:

1) Can online experiments with children provide valid data? (**Chapter 2**)

2) Is there a relationship between parental screen use and children's language skills? (**Chapter 3**)

3) Is there an association between children's and parental screen time and children's emotion recognition? (**Chapter 4**)

2 Chapter 2: Online Experiments with Children

This chapter explores the adaptation of lab-based children's paradigms to online experiments. The start of the thesis coincided with the Covid-19 pandemic, and it became necessary to adapt lab-based studies to online experiments. To provide a comprehensive context, we will begin with an overview of the possible benefits and disadvantages of remote testing in young children. Building upon this, we will discuss a few previous studies and whether they were successful at adapting experimental tasks online with children. Finally, to investigate the impact of online testing on the quality of data, we will introduce our three experiments: a word recognition task using the Intermodal Preferential Looking Paradigm, a word learning task using the Switch task, and a language assessment tool (WinG; Cattani et al., 2019) where children identify a target word amongst a set of picture cards. This chapter is written as a research article currently accepted for publication by the Journal of Child Language.

2.1 Literature Review and Introduction to Experiments 1,2 and 3

Online research studies have become more popular among developmental researchers since the COVID-19 pandemic (Rhodes et al., 2020; Sheskin et al., 2020). Due to COVID-19 restrictions, studies were not able to be conducted in person but thanks to videoconferencing technologies, many research experiments were run remotely (Blanchard, 2020; Delgado, Bark, & Donahue, 2020; Mills et al., 2022). There are important potential

benefits and promises from using videoconferencing: flexible time and space which benefit both the participant and the researcher, as well as the possibility to widen the scope of participant recruitment, enhancing inclusivity and allowing for a better representation of diversity. However, there might also be some pitfalls in the use of online testing, first and foremost related to the quality of data (due to technological limitations, interruptions, etc). When considering infant and toddler language research, where the majority of responses rely on accurate looking time measures, these pitfalls are to be considered carefully. Also, we wondered whether levels of engagement from the participant would be possibly higher (familiar environment, more attentive) or poorer (less controlled setting, less motivation). Indeed, according to an editorial review by Tsuji et al. (2022), online data collection might be more prone to being noisier due to uncontrollable variables such as distractions, lighting conditions, and the quality of recording devices. However, they also reported that it is worth considering that children might feel more at ease in their home environment, which could potentially result in less variability in measurements during online data collection.

Every researcher using an eye tracker in a lab setting has experienced the complexity of minimising a child's head movements and removing external distractions to optimise data quality; therefore, it is potentially challenging to tackle these issues when testing remotely too. Many researchers, including ourselves, worry that remote testing would fail because of the lack of control over motion, parental interference, distraction, equipment, etc. The question we ask in this paper is as follows: given the minimal amount of constraint we can apply to children's movements in a remote situation, and the difficulty to control for external distracting factors, can we still collect data from classic paradigms of early language studies

that compare in statistical robustness to what we would obtain in a lab situation? Previous studies aimed to answer that question by testing whether specific paradigms could be adapted to online settings (see review by Tsuji et al., 2022). For instance, Bochynska and Dillon (2021) did not successfully replicate findings from the lab. They conducted two asynchronous online experiments where they adapted the change-detection looking-time paradigm with infants aged 7 months. Their findings indicated that the infants did not show detectable sensitivities to the basic shape information that differentiates between 2D geometric shapes, which contrast with previous lab experiments results. They reported that failure to discriminate between shapes might be due to distraction and infants having difficulties perceiving two distinct events when displayed on small compact screens of personal computers. Indeed, for this paradigm, most lab studies used two separate monitors or large projector screens (Bochynska & Dillon, 2021). On the other hand, Bánki et al. 's (2022) study successfully tested infants (aged 4-6 months) in an eye-tracking task that measures the detection of audio-visual asynchrony. They found a higher quality of webcam-based eye-tracking data collected online and no differences of participant attrition rate and technical issues between the in-lab and online context. In addition, Bacon, Weaver and Saffran (2021) found that children's (aged 23 to 26 months) word recognition accuracy on the online synchronous looking-while-listening task was greater than accuracy on the in-lab task. Furthermore, Bulgarelli and Bergelson (2022) investigated, with both in-lab and online experiments, how talking variability (e.g., a new talker of another gender produces the word) during learning could potentially influence children's (aged 7-9 months) ability to learn and recognise words. Using a one-word Switch task paradigm, results collected online,

and the results collected in the lab were fully similar. The researchers reported a few limitations of testing remotely such as not being able to control the distance to the screen device or the size or the monitor but concluded that the one-word switch task could be easily adapted for online testing and provide successful results.

This paper adds to this body of knowledge in a number of ways. First, we aim here to demonstrate that effects such as increased looking behaviour modulated by linguistic cues are measurable in children doing the task online and provide benchmarking data between online and lab-based studies, to provide guidance for the design of future studies. We also explore modifications to accepted in-lab procedures, such as increasing the number of trials and using automatic trial presentation, in place of the standard infant-initiated trial start (see Experiment 1). We chose three paradigms which are widely used in infant research: a word recognition task using Intermodal Preferential Looking (IPL, or look-while-listening procedure), a word learning task using the Switch task, and a language assessment tool relying on children identifying a target word amongst a set of picture cards. For each of these tasks, we conducted an online, simple experiment, whose results we compared to existing data collected face to face by our lab or other labs in the pre-pandemic period. We also explored testing infants online when the experimenter was present (synchronous) or not present (asynchronous) (see **Table 1** below for an overview of each experiment).

Table 1: *Experiments 1, 2, 3.* Overview of the three experiments

Experiment	Paradigm	Task	Adaptations to in-lab procedure	Children
Experiment 1: Word recognition	Intermodal preferential looking task	- Replicated from other labs - Online, Gorilla - Asynchronous	- Greater number of trials than comparable procedures - Trials **<u>are not</u>** infant-initiated	*N* = 20 24 months
Experiment 2: Word learning	Switch task	- Replicated from other labs - Online, Zoom -Synchronous	- Lower number of trials than comparable procedures - Familiarisation instead of habituation	*N* = 19 17 months
Experiment 3: Language assessment	WinG test	- Replicated from own lab - Synchronous	- Similar than in-lab task - Comparison online vs in-person	*N* = 62 19-26 months

It is important to note that the UK implemented national lockdowns (late March 2020 to June 2020, January 2021 to July 2021), and local lockdowns (tiers) (September 2020 – November 2020) as stated by the Parliament by the Secretary of State for Work and Pensions by Command of His Majesty, which limited gatherings and travel for everyone except essential workers. It involved the closure of all non-essential businesses, including hospitality venues and retail stores. Additionally, schools were shut down, and people were encouraged to work from home (Brown, Coventry, & Pepper, 2021; United Kingdom Government, 2022).

The Plymouth Babylab was closed from March 2020 and re-opened in July 2021. See **Table 2** below for details of COVID lockdowns and details of children's participation in our experiments.

Table 2: Experiments 1, 2, 3. Lockdowns dates and experiments' information.

Experiment	Dates of participation	Lockdown status
Experiment 1	17/09/20 – 13/12/20	20 children were tested online when "people could only leave home to meet one person from outside their support bubble outdoors"
Experiment 2	22/03/21 -27/10/21	- 11 children took part online during the "stay at home" order - 8 children participated online when this order was lifted
Experiment 3	-22/02/21-31/05/21 -02/07/21-14/12/21	- 32 children were tested online during the "stay at home" order - 30 children took part face-to-face at the lab (no lockdown)

2.2 Experiment 1: Word recognition in an intermodal preferential looking task at 24 months

2.2.1 Introduction

The IPL paradigm is widely used to probe lexical knowledge in the early years, as well as examine infants' sensitivity to various aspects of linguistic details in words (Golinkoff, Hirsh-Pasek, Cauley, & Gordon, 1987). Our aim was to guide the implementation of an online adaptation of the IPL to collect eye movement data using a participant's webcam in their home context. While this type of asynchronous collection of eye movement data in young children has already been explored using platforms such as Lookit (e.g., Nelson & Oakes,

2021) and Labvanced (e.g., Bánki et al., 2022), there are no published findings using the Gorilla Experiment Builder platform (www.gorilla.sc, Anwyl-Irvine, Massonnié, Flitton, Kirkham, & Evershed, 2019). Most studies testing children using Gorilla have tested older children and collected accuracy and reaction time measures (e.g., Chere and Kirkham, 2021), with tasks requiring, for example, a button press response (e.g., Ross-Sheehy et al., 2021) rather than looking behaviour in infants. This might be because, while Gorilla Experiment Builder can run behavioural studies with the functionality to access a participant's webcam and record looking behaviour, this option is still in Beta. Thus, Experiment 1 tests how well the platform can accommodate an IPL task when testing infants.

Two key aspects of this adaptation were considered. The first was to understand how an online procedure may affect issues of timing in the experiment, due to factors such as internet speed and different device types. The second was to see how much usable data could be collected when children are tested in their home environment and when trials are presented automatically, that is, not infant-led as would be the case in many lab-settings.

A word recognition task was chosen because of its relatively reliable large effect size and replicability when conducted in a lab setting. In a meta-analysis of typically used methods in language development studies, Bergmann, Tsuji, Piccinini, Lewis, Braginsky, Frank, and Cristia (2018) found an average effect size of d = 1.24 (SE = 0.26) in online word recognition studies (N = 6). Thus, choosing this method offered the best chance of developing a proof of concept for an online IPL procedure for paradigms with potentially smaller effect sizes, such as a semantic priming study (e.g., d = .32, Jardak & Byers-Heinlein, 2018).

In a typical word recognition task, a participant is played an auditory stimulus which is the label of one of two simultaneously presented visual stimuli. In a lab setting, the participant typically fixates on the named visual stimulus for longer than the unnamed visual stimulus, which is taken as evidence of word recognition. Infants are able to fixate a target referent as young as 6-9 months (Bergelson & Swingley, 2012) in a look-while-listening procedure, with word comprehension and recognition generally observable by 12 months (Vihman, Thierry, Lum, Keren-Portnoy, & Martin, 2007). Therefore, by testing at the older age of 24 months we had an optimum chance of replicating the same effect in an online modality. If running the experiment in an online modality was significantly different to an in-lab modality, this might mask the effect of a longer proportion of looking time to the target image.

2.2.2 Method

Pilot study

Using the online experimental platform, Gorilla Experiment Builder (www.gorilla.sc, Anwyl-Irvine, Massonnié, Flitton, Kirkham, & Evershed, 2019), a small number of participants participated in a pilot study (adults: *N* = 2, infants *N* = 4). As previously mentioned, Gorilla Experiment Builder can access a participant's webcam and record, with their consent, but this feature is in Beta, and has its limitations. One of which is its inability to simultaneously record a participant and the experiment, or precisely what the participant sees on screen and when. While the timing of stimuli presentation and duration can be precisely programmed into the experiment on Gorilla Experiment Builder, when the experiment is run

on a participant's device, some variability may exist because of the differences in devices used, internet browsers, and internet connection speeds, though timing accuracy does seem quite stable (Anwyl-Irvine, Dalmaijer, Hodges, & Evershed., 2020). Another potential variable aspect of the webcam recording feature is a delay in the command from Gorilla requesting access to a participant's webcam, and the point at which the recording starts. Although this can be up to 500ms according to one of the developers (personal communication, 23rd May, 2021), we found only marginal delays (10-20ms) through piloting. Additionally, a design feature was added to the experimental design (see below) to note which trials began recording before visual stimulus onset, and which did not.

Piloting the experiment on adults and infants was crucial to devise satisfactory solutions to these limitations and to decide how to best minimize variability in executing the experiment online. Email correspondence with parents and viewing the data that was successfully generated allowed us to make a set of small changes to the paradigm. Differences between pilot and test are described below in the Procedure section.

Participants

Participants were recruited through the University of Plymouth BabyLab database and Facebook page. Following recommendations for minimum sample sizes for infant studies that are based on a simulation study of the systematic effect of sample size on the results of infant studies (*N*= 20-32; Oakes, 2017), 20 monolingual British English-learning infants (13 boys, 7 girls) were tested. The target sample size was reached before analyses of the data. The mean age of participants was 24 months 3 days (range 23 months 3 days - 25 months

28 days). Participants were considered ineligible if they spoke more than one language, were born more than six weeks prematurely, or had a diagnosed language or developmental delay. No participants had to be excluded on these bases. For each of our three experiments, parental education was measured on a scale from 1 to 6 (1= primary education - 6= postgraduate degree) with the highest value taken from either parent (e.g., Mäkinen, Laaksonen, Lahelma, & Rahkonen, 2006; Mossakowski, 2008).

Materials

A total of twenty-four target words (e.g., bed, key) were selected which were familiar, common, highly imageable nouns known by at least 60% of English monolingual 18-month-olds according to the Oxford Communicative Development Inventory (Hamilton, Plunkett, & Schafer, 2000) and the UK CDI (UK-CDI Database, 2016) (see **Table 3** below for the list and exact percentages). All words were monosyllabic.

Table 3: *Experiment 1.* Percentage of 18-month-olds with knowledge of the stimuli words used in the online IPL task.

Target	% known at 18 months OCDI	% known at 18 months UKCDI
bed	85	97
bird	88	88
book	95	98
bowl	58	77
box	48	63
bread	72	77
car	95	97
chair	80	95
cheese	63	78
cot	70	68
dog	98	99
duck	90	86
fish	75	81
foot	70	92
frog	56	68
hair	91	86
key	74	81
pig	77	82
plane	81	72
shoe	99	97
spoon	77	76
swing	64	68
train	66	81
tree	69	78

Auditory stimuli were recorded individually by a female adult with a neutral south-west British accent. The carrier word “Look!” was also recorded separately. Visual stimuli were colour photographs from the internet, cut out from their background and placed centrally on a light grey background to reduce brightness on the screen. Two versions of each image

were created: one for presentation on the left of the screen, and one for the right. Animate objects were positioned to face the centre of the screen.

Target words were organised into word pairs in which there was no semantic or phonological overlap. The twelve pairs formed one block. In each pair, one word acted as the target and the other as a distractor. The distractor words then became the targets in the second block of trials, and these were paired with a different word that had acted as a target in the first block.

Procedure

Through piloting, the following modifications were made to the experimental design and procedure:

- Participants were restricted to using a laptop or computer. Those without such a device were deemed ineligible. This criterion was set to ensure visual stimulus presentation would be as large and as predictably positioned as possible. Gorilla Experiment Builder's default positioning of two adjacent images is to space them as far apart, to each edge of a device's screen as possible.

- The experiment was programmed to only run on the web browser Google Chrome as there were some upload and display issues with other browsers.

- A calibration phase was added at the start of the experiment to ensure a participant's screen was not working in a 'flipped' mode, and to validate that when an image was presented on the right only, the child looked to the right.

- A short beep of 100ms was added to coincide with the visual stimulus onset. In the absence of seeing when the pictures appeared on screen in a participant's webcam recording, the beep was a feature to enable the coder to have a reference point when manually coding eye movement offline. Each trial was checked for the presence of the beep during analysis, to ensure that the webcam recording started ahead of the images being presented on screen.

- Trials were divided into two blocks and separated using a short video to maintain attention. As the experiment could not be driven by the child's attention to the screen on every trial, the short video was a way of re-focusing the child in the event that they had lost interest. Piloting showed inattention to be very infrequent.

- A duration of 500ms was added to each trial, resulting in the images remaining on screen for 5500ms (compared to 5000ms in a typical lab-based experiment). This was to compensate for any potential clipping towards the end of the recording.

All of our studies were approved by the University of Plymouth Faculty of Health Ethics Committee. Parents were invited to participate in the study through the Plymouth BabyLab database and through adverts posted to the BabyLab's social media accounts. When a parent expressed interest, further communication moved to email. A participant information sheet was issued and the technical requirements for the online study were reiterated through email communication. A day and time were agreed, on which to complete the study. On the appointed day, an email with instructions for the study were sent to the parent and a unique link to the experiment was activated on the Gorilla

Experiment Builder website. By using a unique link, it meant participants could leave the experiment and return to it later, continuing where they left off. The reason behind establishing a day and time to do the online experiment was to ensure a researcher could be available for any questions or support required while participants did the task[1]. Parents were instructed to begin the procedure without their child present, to minimise the time a child would need to stay engaged. It was made clear that the parent would be instructed when to prepare their child for the task.

When clicking on the Gorilla Experiment Builder weblink[2], an overview of the study was displayed, including the eligibility criteria for participation. The next screen was an eligibility questionnaire, to ensure participants were the right age; were not born more than six weeks prematurely; were exposed only to English; and did not have a language or developmental delay. At this point, a participant could be excluded in which case the parent would see an ineligibility screen and be asked to email the Plymouth BabyLab if they believed this to be incorrect, or if they wanted to find out about other studies running that their child might be eligible for.

If eligible, a participant had to consent to the study by completing an online questionnaire which detailed the procedure, the data collected and the right to withdraw.

[1] Parents did occasionally need technical support which often related to needing a new link to be sent. This mainly resulted from not reading the instructions, or pressing a button in error. We modified the email and experimental instructions to try to minimise this. In a couple of cases, parents' browsers blocked the Gorilla pop-up requesting permission to record via the webcam. Since we were online while the parent did the experiment, we were able to talk through various checks to resolve the issue.

[2] The full procedure can be viewed using this link: https://app.gorilla.sc/openmaterials/627362

Following this, participants progressed to a technical eligibility check so they could test their sound and webcam before the experiment, and to grant Gorilla access to webcam recording. A Gorilla pop-up appeared in the web browser asking for consent to access the webcam, at which point a parent could refuse access if they did not agree to their data being accessed in this way. Furthermore, the recording test established the audio and video recording capabilities of a participant's device and it also allowed parents to playback the recording to fully understand the footage that would be recorded of their child when the experiment began. Throughout the procedure, an 'Exit' button was made available in the bottom left-hand corner of the screen in case a participant chose to withdraw from the study. There was explicit mention in the instructional email that a participant should click on this 'Exit' button if they wanted to withdraw and to request, by email, the withdrawal of any data collected on their child up to that point if they desired, without any explanation for their decision.

Demographic information was collected in a series of short online questionnaires before the experiment proper[3].

The experimental procedure began by instructing the parents to place their child on their lap, with their device's webcam focused on their child's eyes. Detailed instructions were provided, using images, so that the parent could see how to prepare their child for the experiment. Rough measurements were provided (e.g., place the device at arm's length, mirroring what other researchers were trialing at the time for online experiments), and

[3] See https://app.gorilla.sc/openmaterials/627362 for the exact questions asked.

opportunities were available for the parent to perform test recordings before they began testing. Based on all this information, the parent deemed when the position of their child was satisfactory, and when they were ready to begin the task. Parents were instructed to not engage with their child when starting the experiment proper.[4]

The experiment was preceded by four calibration trials in which the word "Look" was followed by the word "biscuit" and an image of a biscuit appeared on the left-hand side of the screen. This process was repeated on the left side with the word "monkey" and a corresponding image. The two words were then repeated with the same images now appearing on the right-hand side of the screen. Neither of the words were used as targets, or distractors on critical trials. The calibration phase established a baseline for the participant's individual looking pattern and validated that the image was presented on the correct side and not in a 'flipped screen' mode.

The parent controlled the start of the word recognition task by clicking on a button. The experiment began with a 5000ms black and white attention-getting video showing simple geometric shapes accompanied by sound, as it was unclear how attentive a child would be using an online paradigm. Then, the automatic presentation of trials began and did not stop in their delivery until all trials had been presented, which lasted for about three minutes.

Each experimental trial began with a smiley fixation point in the centre of the screen for 1000ms to focus the child's attention to the middle of the screen. This was replaced by two

[4] Though this could not be controlled due to the remote nature of the testing, which includes the ability to stop the parent looking at the screen during the experiment, video recordings of parent and child indicated the parent sometimes looked at the screen but not continuously, and sometimes they were not next to the child at all.

visual stimuli, positioned on the left and right sides of the screen for 5500ms. In an equivalent lab-based study, a trial would last 5000ms but an additional 500ms was added in case of clipping at the end of the recording. The auditory stimuli began with a beep for 100ms to coincide with the visual stimulus onset, necessary for analysis. This preceded a silence and the carrier 'Look' before the target word onset at 2500ms. Each trial was thus divided into a 2500ms pre-naming and 2500ms post-naming window (see **Figure 1** below).

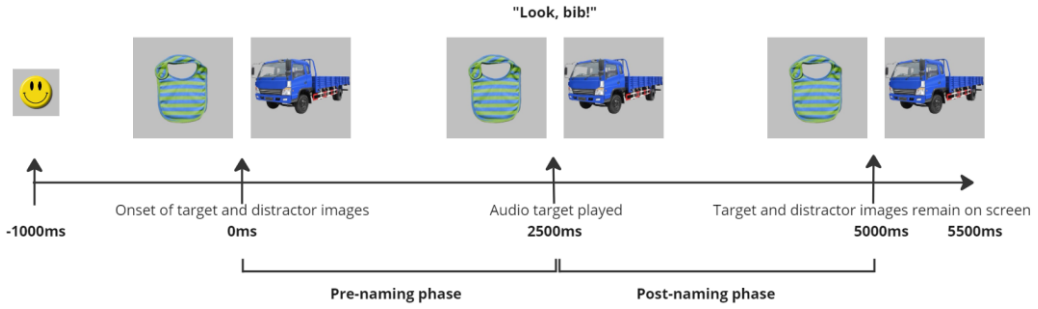


Figure 1: Experiment 1. Trial timeline. Onset of the auditory label of the target picture was always at 2500ms

After 12 trials, the same attention-getting video from the start was played to maintain the child's attention before a second block of 12 trials resumed. The video also separated the two blocks in which visual stimuli acted as targets in one block and distractor pictures in the other. The order of blocks was counterbalanced. The side of the target visual stimulus was counterbalanced. The experiment ended with the same 'reward' video that was played at the start and middle of the experiment.

To complete the procedure, the parent marked a list of target words as known or unknown to the child, before a final debrief screen, inviting any questions or comments and a chance to mark whether any technical difficulties had been experienced during the tasks.

After completion of the full procedure, a participant's data were downloaded, and the calibration trials were checked to confirm audio and video recording was satisfactory. Questionnaires were reviewed to see if the participant had experienced any technical issues or if further information relating to their responses in the questionnaires was required. A final email was sent, requesting clarification pertaining to comments in the questionnaires (where necessary) and issuing a certificate and £5 Amazon voucher to acknowledge participation. The final email also included a short debrief of the study's aims and application and invited the participant to ask questions if necessary.

Technical Specifications

Devices were restricted to laptops or computers, yet this can still mean a range of screen sizes. Gorilla records the device type used by a participant, including its screen size. The average viewpoint size on screens used was 1432x742 with parents classifying the mean quality of audio as 5 (Very good, on a scale of 1 to 5). Most participants were using the latest operating systems for their devices, and the latest version of Chrome. The full range of technical specifications can be seen in **Table 4** below.

Table 4: *Experiment 1.* Overview of Device Types Used in the online IPL study.

Particpant	Participant OS	Participant Browser	Participant Monitor Size	Participant Viewport width	Participant Viewport height	Audio Quality (1-5)
0hl65s6s	Windows 10	Chrome 87.0.4280.88	1536x864	1536	754	Clear enough- 4
26o5cpdq	Mac OS 10.14.5	Chrome 86.0.4240.193	1440x900	1440	821	Very clear- 5
4k5u5xs8	Windows 10	Chrome 86.0.4240.183	1366x768	1349	625	Very clear
4uoxz04w	Windows 10	Chrome 85.0.4183.121	1536x864	1438	704	NA
ksry8lfl	Mac OS 10.13.6	Chrome 86.0.4240.80	1680x1050	1680	971	NA
reuryabw	Mac OS 10.14.0	Chrome 87.0.4280.67	1440x900	1050	752	Clear enough
22vg0z4l	Windows 10	Chrome 86.0.4240.75	1536x864	1519	722	Clear enough
5ym93g5p	Windows 10	Chrome 86.0.4240.75	1366x768	1366	625	Very clear
cpqtjso9	Windows 10	Chrome 86.0.4240.193	1366x768	1349	625	Very clear
qiamsumn	Windows 10	Chrome 67.0.3396.99	1366x768	1349	662	Very clear
uibpbg89	Windows 7	Chrome 86.0.4240.111	1920x1080	1920	1009	Very clear
ye42nool	Windows 10	Chrome 87.0.4280.66	1280x800	1280	689	Very clear
lfioaben	Windows 10	Chrome 86.0.4240.198	1280x720	1280	610	Very clear
odcoevc8	Windows 10	Chrome 86.0.4240.198	1920x1080	1920	969	Very clear
plrudr83	Windows 10	Chrome 86.0.4240.183	1368x912	1368	783	Very clear
pmwyldgf	Windows 10	Chrome 86.0.4240.198	1680x1050	1680	939	Clear enough
xtu5nbo8	Windows 7	Chrome 86.0.4240.193	1536x864	1198	630	Clear enough
heojqujc	Windows 10	Chrome 86.0.4240.111	1366x768	1349	657	Very clear
iiahp11j	Mac OS 10.15.7	Chrome 86.0.4240.193	1440x900	1200	667	Very clear
s5xh3nt0	Windows 10	Chrome 86.0.4240.111	1366x768	1366	625	Very clear

Data Processing and Analysis

Using a bespoke online encoder developed by the University of Plymouth School of Psychology technical team, videos of individual trials were uploaded and automatically split into 50ms frames. For each frame, the primary coder, blind to the visual and auditory stimuli presented, assessed the digital videos off-line frame by frame, manually marking the position of the participant's eye position as left, right, away, or indeterminate by using four corresponding keys on the keyboard. This information was saved in .csv format and later downloaded for analysis.

A second, skilled coder manually coded 10 per cent of the full dataset. Inter-rater reliability agreement between coders was 87% and according to Cohen's Kappa calculation, was moderately reliable $\kappa = 0.47$. On further inspection of the discrepancy between the two

coders, out of the total 13% disagreement, 6% was specific to whether a gaze was indeterminate or not, meaning the gaze was still on screen, but unclear where exactly. This might explain the lower-than-expected reliability measure.

Trials were excluded from analysis if a child did not fixate for a minimum of 750ms, somewhere on the screen (left, right or indeterminate), or if the child did not know the target word based on a parent's report of their child's word knowledge. The latter ensured that an infant was evaluated only on their understanding of known words.

The raw .csv files, generated by coding eye movements using the University of Plymouth Encoder, were uploaded in R Studio (v1.4.1717; R Core Team 2021) for all further analyses[5]. The R tidyverse and dplyr packages were used.

[5] The analysis code is available on request.

2.2.3 Results

Descriptive Statistics

Table 5 provides the descriptive data for the children's ages and looking times.

Table 5: *Experiment 1.* Descriptive data of the whole sample

Means and Standard Deviations of the children's age and looking times

	M	*SD*
Age (days)	735.05	19.04
Boys' age (days)	728.58	23.31
Girls' age (days)	737.50	19.06
Parental education	5.48	0.60
Proportion of the looking time pre-naming to the target	0.50	0.07
Proportion of the looking time post-naming to the target	0.62	0.07

Note. Parental education level is the highest of the two parents' highest educational levels, ranging from 1 to 6.

When aggregating all participants' looking time by condition, on average, participants spent 82% of the time looking at either the left or right side of the screen, with an additional 16% of the time looking at the screen but at an indeterminate point on the screen (i.e., neither clearly left nor right). This time also accounts for saccades between the left and right sides of the screen. Finally, 2% of looks per participant were looks away from the screen.

Out of a possible 480 trials (a maximum of 24 trials for each of the 20 participants), a total of 459 trials were included for analysis. Reasons for exclusion were entirely due to the target word not being known to the child (21 trials or 4.38% of trials), which was measured by parental report. No trials were excluded due to inattentiveness, measured as <750ms spent looking at the screen per trial. The average number of valid trials per participant was 22.95 (*SD* = 1.4). In summary, 24-month-old infants were very engaged in an online looking task when administered in their home. By way of comparison, in a meta-analysis looking at looking while listening studies, among other methods, Bergmann et al. (2018) used a linear mixed effects model to predict an exclusion rate of 30% of data for this task type, including minimum looking time criteria. In a more recent study, Byers-Heinlein, Bergmann, and Savalei (2021) saw an exclusion rate of 5.07% for equipment failure, parental interference and fussiness, in addition to 23.03% data loss due to infants not attending to objects during the specified window of analysis.

There was no effect of gender on response rate $t(18) = .44$, $p = .66$.

Proportion of Looking Time to the Target

Participant's looks were aggregated by condition (i.e., target, distractor, away, indeterminate) and the proportion of time spent looking at the target compared to the distractor was calculated for the pre- and post- naming windows.

The pre-naming window of analysis was set at 200ms – 2500ms which allows for an initial 200ms shift in eye gaze (Fernald, Pinto, Swingley, Weinberg, & McRoberts, 1998; Fernald, Swingley, & Pinto, 2001) from an attention-getter to one of the pictures, followed by 2300ms

of free-looking. The post-naming window was set at 2700ms - 5000ms to allow for initial processing of the onset of the audio, followed by the same amount of free-looking time (equivalent to 46 frames of 50ms per trial, per participant).

The proportion of looking time (PLT) towards the target visual stimulus, relative to the distractor stimulus, was calculated as the dependent variable for the pre-naming and post-naming windows, per trial:

Looks to target/ (Looks to target + Looks to distractor)

A two-tailed, paired t-test was run on the PLT in the pre-naming and post-naming windows of analyses. Twenty-four-month-olds looked at the target longer in the post-naming window (M = 0.62, SD = 0.07) compared to the pre-naming window (M = 0.50, SD = 0.07) (see **Figure 2** below with the white square indicating the mean). The difference between looking behaviour in these two periods was significant with a very large effect size, $t(19) = 17.22$, $p < .0001$, $d = 1.61$. This indicates that participants looked longer at the target picture after it had been named, indexing word recognition.

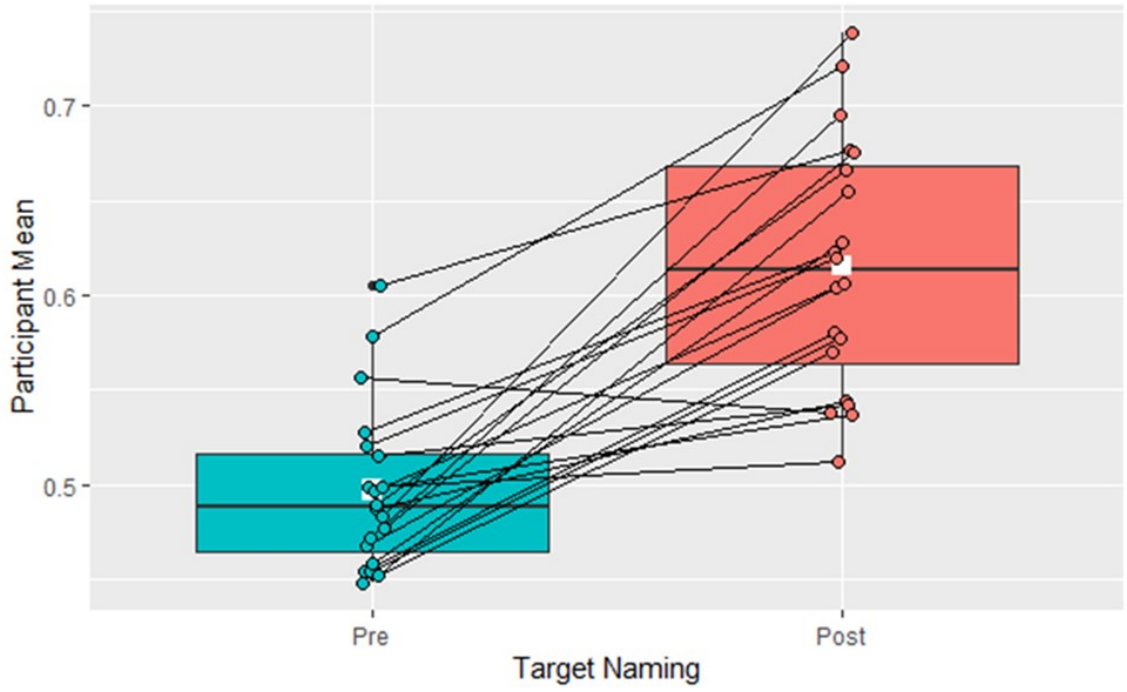


Figure 2: *Experiment 1.* Proportion of Looking Time Pre- and Post-naming during the online IPL study.

2.2.4 Discussion

A simple word recognition experiment was run using the online experimental platform Gorilla Experiment Builder (www.gorilla.sc, Anwyl-Irvine et al., 2019) as a proof of concept to test the feasibility of running online preferential looking experiments with infants. The results from Experiment 1 indicate that with some modifications to lab-based procedures, an online version of an infant methodology can indeed be run successfully. Experiment 1 adapts the IPL task into an online modality, providing a validation of the general testing paradigm. As far as we are aware, this procedure is one of the first of its kind to be conducted on Gorilla Experiment Builder with young children. This is important as it contributes to the evidence base for testing young children online, using a different online platform than what

is currently being used. We found Gorilla Experiment Builder to be a user-friendly platform, requiring no coding experience and that can support an IPL procedure. It is unclear if this platform has the potential to replicate in-lab findings when testing a procedure on infants with a smaller anticipated effect size, such as a semantic priming task, and this is something we have begun to test (Fitzpatrick & Floccia, submitted 2023) and continue to explore. The full set of materials of this experiment have been made open source for other researchers to use for replication studies.

The results clearly showed that infants aged 24 months looked at a picture on screen longer when the picture was named, compared to a picture that was unnamed. This is an expected outcome which indexes word recognition in children and replicates previous lab-based findings (e.g., Vihman et al., 2007). The novelty lies in the fact that the 24-month-olds were performing the task online, in their own homes and using their own devices. Participants were not overly distracted by their surroundings, nor were there significant issues with differing device types and internet speeds. Compared to lab-based studies, the effect size found in Experiment 1 (d = 1.61) is larger in magnitude (e.g., Bergmann et al., 2018, found an average effect size of d = 1.24 in a meta-analysis) which is a promising finding for other online studies collecting eye movement data.

Interestingly, participants remained engaged throughout the procedure despite the fact that trials were not infant-led, that is, they ran automatically without pause. This is a very different approach to many lab-based studies in which the start of every trial is initiated by the experimenter when the infant's attention is focused on the computer screen (e.g., Arias-Trejo & Plunkett, 2009; Chow, Aimola Davies, & Plunkett, 2016; Floccia, Delle Luche,

Lepadatu, Chow, Ratnage, & Plunkett, 2020; Singh, 2013; Styles & Plunkett, 2009). Automatic presentation of trials was borne out of necessity while using Gorilla Experiment Builder to administer the task online. According to the findings of this study, running the experiment without pause does not seem to have had a negative impact on a child's ability to perform the task. This may be thanks to the features integrated into the design of the experiment such as fixation points and video rewards at the start, middle and end of the procedure.

Participants also remained engaged in the face of a twenty-four-trial experimental design, which is double the number of trials commonly used in infant studies at this age (Arias-Trejo & Plunkett, 2009; Arias-Trejo & Plunkett, 2013; Jardak & Byers-Heinlein, 2019). This is encouraging support for future studies as using this number of trials will help with the power of future studies in the case of potential data loss occurring, as mentioned above (i.e., distraction, technical issues etc).

With regards to this particular study, there was very little attrition or data loss (<5%) compared to some lab-based studies, which can lose up to 30% according to a meta-analysis performed by Bergmann et al. (2018). This might be due to a participant feeling more relaxed in their home environment compared to a lab environment. By informally looking at the experimental videos, children did not seek out contact as frequently with a parent by turning around, as they do in the lab. Similarly, the child might have felt more at ease on a parent's lap, rather than in an unfamiliar car seat/ booth in a lab. These hypotheses are supported by the data; there was a high proportion of looks on-screen to the left or right (82%) versus off-screen (2%). This amount is likely to be larger considering looks on-screen

but to an indeterminate location (16%) may have actually been looks left or right. One explanation might be the manual coding of eye movement which minimised data loss, compared to lab-based studies in which the eye-tracker losing signal leads to data loss.

Taken together, these findings provide encouraging support that other infant paradigms might be suited for adaptation to online testing. What remains to be seen is whether paradigms with smaller effect sizes, such as the effect sizes found in semantic priming studies (e.g., d = .32, Jardak & Byers-Heinlein, 2019), can be evidenced using the same online procedure. Findings from this study indicate that infants can complete twice as many trials as other, comparable word recognition studies specify, while still maintaining attention. Using an increased number of trials will help increase power for testing such hypotheses.

2.3 Experiment 2: Word learning in a Switch task at 17 months

2.3.1 Introduction

Infants can learn word-object associations that can be robustly measured at 12 months (Curtin & Zamuner, 2014). Waxman and Booth (2001) findings suggested that infants of 14 months can identify novel noun words (e.g., "This one is a *blicket*") and specifically map them to new objects (e.g., carrot, orange). Stager and Werker (1997) developed the Switch task to investigate how infants behave in a situation that requires them to link a new label with a new object. In the Switch task, infants are exposed to a novel word–object pairing where they see a novel object moving back and forth across the screen, while simultaneously hearing a novel word repeatedly. This presentation continues until a

predetermined decline in looking is observed in infants. In the following test phase, infants are tested with two types of trials. On the “same” trial, the initial object-word pair stays the same while on the “switch trial”, the object is paired with a different word. If infants notice the difference, they should look longer on the “switch” than on the “same” trials (Fennell & Waxman, 2010; Stager & Werker, 1997). A recent meta-analysis has found a low to moderate effect size of Cohen’s *d* = 0.32 (141 Switch tasks in infants aged 12 to 20 months; Tsui, Byers-Heinlein, & Fennell, 2019). Previous research revealed that infants of 14 months learned to associate two distinct sounding words (*lif* and *neem*) to two different objects by looking longer to the “switch” trial. However, infants aged 8 and 12 months fail to associate the different soundings (Werker et al., 1998). We decided to test 17-month-olds following Werker et al. 's (2002) demonstration that infants at this age could apply phonetic detail when learning new words within a short exposure period. We reasoned that it would give us better chances to observe a large effect and an increased power of word learning with phonetically dissimilar words when testing online, especially given that at 17 months, infants are experiencing a boost in vocabulary learning (e.g., Cochet, Jover, & Vaucler, 2011).

Experiment 2 describes an online adaptation of the Switch task with 17-month-olds, using a combination of Zoom and offline coding. The infants were tested using a modified habituation paradigm similar to the design used by Cohen, Lloyd, Casasola, and Stager (1998) but with only one word-object pairing and not two, as in Fennell and Waxman (2010) and with a different habituation criterion. Specifically, we did not measure a habituation, that would be indexed by a pre-specified decrease in looking times, but we fixed a familiarisation time identical for all participants (see the procedure for more details). The

sample size target was 16 participants as in previous Switch tasks experiments (Fennell & Waxman, 2010; Fennell & Werker, 2003). It must be noted that the data reported in Experiment 2 was collected before we read about the study by Bulgarelli and Bergelson (2022) who also conducted a one-word Switch task but with 18 younger children (7-9 months), and we will address their findings as compared to ours in the Discussion.

2.3.2 Method

Participants

A total of 19 parents with monolingual children (10 boys and 9 girls) aged 17 months, ranging from 16 months 4 days to 18 months 10 days, were recruited from the Plymouth Babylab database (with the same inclusion criteria). They were all residents of Plymouth and its surroundings and had signed up to the Babylab to take part in any proposed studies.

Stimuli

The audio stimuli were two nonsense consonants–vowel labels: *neem* and *lef* recorded in infant-directed speech (IDS). IDS is efficient in capturing and keeping the attention of infants (Fernald, 1985). These stimuli highly differ in articulation and a highly dissimilar nonsense consonant-vowel noun, *pok,* was used during the pre- and post-test trials.

An English-speaking female from the South West of England produced several tokens of each syllable in a rise-fall intonation phase, in an infant-directed speech (Fennel & Werker, 2003; Stager & Werker, 1997). The final stimuli contained 10 exemplars, each lasting

approximately 0.7 sec, including a 1.5-sec silent interval between each exemplar, resulting in audio files of 22.5 sec in duration.

The stimuli were shown as 3D moving objects to highly attract and maintain infants' attention (Baldwin, 1989; Cohen, 1973, Fennel, 2012). A marker toy windmill object was used for the habituation and test trials (see **Figure 3a**) and a trophy topper was used for both the pre- and post-tests (**see Figure 3b**). During the trials, the two objects spinned, moved back and forth. The video clips were edited via the Photos laptop Windows application. The Switch task was administered online with the Zoom app using computer/laptop devices and it was recorded through the Zoom app for coding purposes.

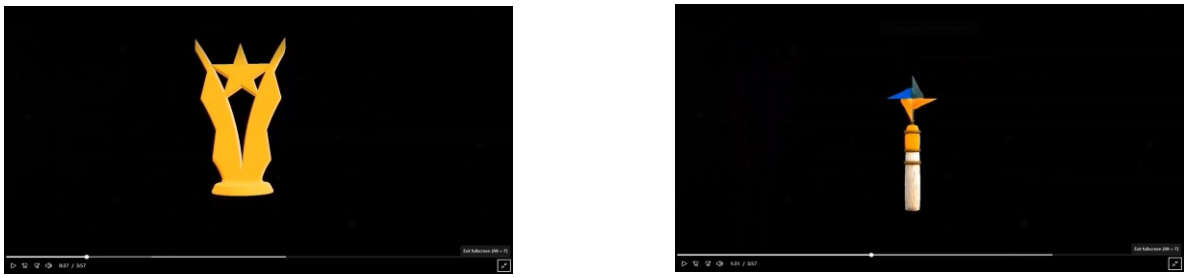


Figure 3: *Experiment 2*. (a) Trophy topper and (b) Marker toy windmill new objects.

Material

Zoom was chosen as the platform of testing for this experiment because unlike many other virtual technologies, it includes advantages that can be used for research purposes. Indeed, according to Archibald et al. (2019), Zoom's capacity of safely recording and storing sessions without a third-party software is one of the advantages to protect sensitive research data. Also, they reported that the capacity to back-up recordings to online server networks such

as “the cloud” or local drives, is an additional security benefit as it allows for recordings to be shared safely for teamwork and real-time encryption (Zoom Video Communications Inc., 2016).

Pilot

Using Zoom, a small number of 9 participants participated in a pilot study (2 girls and 7 boys aged 17 months, ranging from 16 months 17 days to 18 months 4 days with an average parental education level of 4.88). Piloting the experiment was essential to test the quality of the stimuli (video and sound), the data collection and to check how many habituation trials were needed for this online Switch task. A laptop Lenovo ThinkPad and the Photos app in Windows 10 were used to create, edit the video stimuli, and conduct the experiment. Participants were asked to operate with a computer or laptop to ensure satisfactory visual stimulus presentation.

A limitation of testing online with Zoom was the impossibility to control the pace of trial presentation due our specific set up using the auto-advance feature of the Photo app. Therefore, all trials were presented at once without being able to control when to present the next one (as in Experiment 1 using Gorilla). Piloting showed that looking times significantly declined after 4 trials, therefore the habituation phase was set at 4 trials for further data collection.

Procedure

The parent was sent via email the consent and information forms. At the same time, the parent completed a Communicative Development Inventory (short form of the Oxford CDI,

Hamilton et al., 2000). The Oxford CDI is a list of words that are typical in children's vocabularies. Parents were asked to tick whether their child could understand and/or say the words on the list. Then, the parent and child were invited to participate in the online Switch task.

Contrary to Experiment 1 where the researcher was not in the same virtual space as the child, here the researcher, the parent and the child were on Zoom together. The researcher was sitting in front of a laptop, while the child was sitting at home in front of the family electronic device. The session was video recorded. The child was asked to look at the computer's screen and the parent sat in a chair next to his/her child.

When the child was attentive, the researcher started a 3min30s video to the participant consisting of 8 trials from the Switch task including a short clip of 30 sec (a talking bunny chasing a flying kite) to test if the participant's devices' sound and camera were correctly working. The infants were tested using a modified habituation paradigm, similar to the structure used by Werker, Cohen, Lloyd, Casasola, and Stager (1998) but with only one word-object pairing and not two, as in Fennell and Waxman (2010). Also, it was modified for the trial duration (increased from 14 sec to 22.5 sec) and habituation criterion (instead of waiting for a decrease in looking times, we fixed familiarisation time to exactly 4 trials for each child, which seemed reasonable based on data collected from the pilots and past papers). Each trial started with a flashing red light to get the infant's attention on the screen. On the first trial, infants were presented with a pre-test stimulus: the label *pok* paired with the trophy topper. This pre-test stimulus was re-presented at the end of the experiment, during the post-test phase, and acted as a control of infants' attention. During the following

habituation phase, the infant was shown one word–object pairing (word *neem* and object *toy windmill*). After exactly 4 trials, the habituation phase ended, and was followed by the test phase. One test trial was the "same" trial, in which the word-object pair presented during the habituation phase was shown again to the infant. The other test trial, called the "switch" trial, contained the familiar *toy windmill* object but was paired with a novel word *lef*. The order of presentation of the trials was counterbalanced across participants. If infants had learned the pairing, it was expected that they would notice the switch and look longer during the "switch" trial than during the "same" trial (Fennell & Werker, 2003). In the final post-test trial, the child was presented again with the word *pok* and the *trophy topper*. It was expected that if infants remained engaged throughout the experiment, the looking time during this last trial would be similar to the looking time during the pre-test trial (Fennell & Werker, 2003) (see diagram in **Figure 4** below for more details).

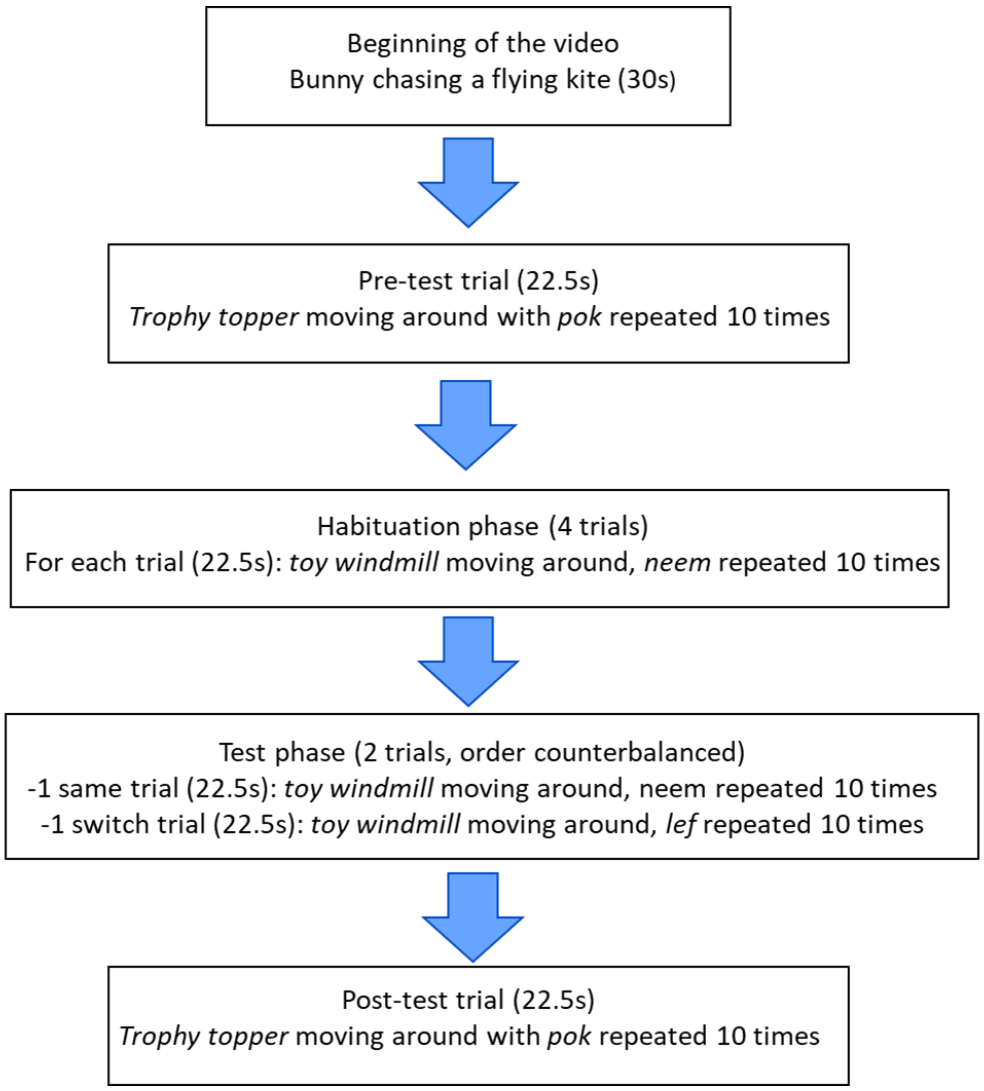


Figure 4: *Experiment 2.* Diagram of the online Switch task

Coding

Using a frame-by-frame analysis (1 frame = 50 ms), coders scored infants' looking times. To ensure the reliability of the main experimenter's coding, a second trained coder scored the looking times of 25% of the participants. Inter-rater reliability agreement between coders was 81.78% and according to a Cohen's Kappa calculation, was strongly reliable, κ = 0.86. 18.22% of disagreement between the two coders was due to whether the gaze of the child

was still on screen or away, but that was equally distributed across the Switch and Same trials, which means that it wouldn't have had an impact on the direction of the results.

2.3.3 Results

Table 6 provides the descriptive data for the children's ages, gender (10 boys and 9 girls), parental education, income deprivation scores, CDI scores and looking times.

Table 6: *Experiment 2.* Descriptive data of the whole sample

Means and Standard Deviations of the children's age, gender, CDIs scores and looking times during the different phases of the Switch task trials.

	M	*SD*
Age (days)	517.21	18.74
Boys' age	514.46	18.14
Girls' age	525.00	18.73
Parental education	4.48	0.93
IDS	0.15	0.20
CDI knows (percentile)	38.52	18.78
CDI says (percentile)	9.52	10.20
Looking time pretest (s)	19.67	5.39
Looking time posttest (s)	14.95	7.80
Looking time same trial (s)	13.37	7.96
Looking time switch trial (s)	17.89	5.52
Difference score (s)	4.53	8.60

Note.

Difference score is the difference between the looking time to same and switch trials.

To ensure that infants did not lose interest throughout the experiment, a paired sample t-test was conducted to compare looking time on the pre-test versus post-test trial. Contrary to what was expected (Fennell, 2012; Werker al., 2002), children were significantly more engaged at the beginning of the task during the pre-test (*M* = 19.67, *SD* = 5.39) than during the post-test (*M* = 14.95, *SD* = 7.80), *t*(18) = 3.85 *p* =.001.

The main set of analyses addressed infants' performance on the test trials. A paired sample t-test revealed a significant main effect for test trials, with the children looking longer to the switch trial (*M* = 17.89, *SD* = 5.52) than the same trial (*M* = 13.37, *SD* = 7.96), *t*(18) = -2.31, *p* = 0.03, Cohen's *d* = 0.53. There was no main effect of gender and age on looking times. Thus, the 17-month-old infants exposed to the first pairing of word-object did notice the switch in label (see **Figure 5** below).

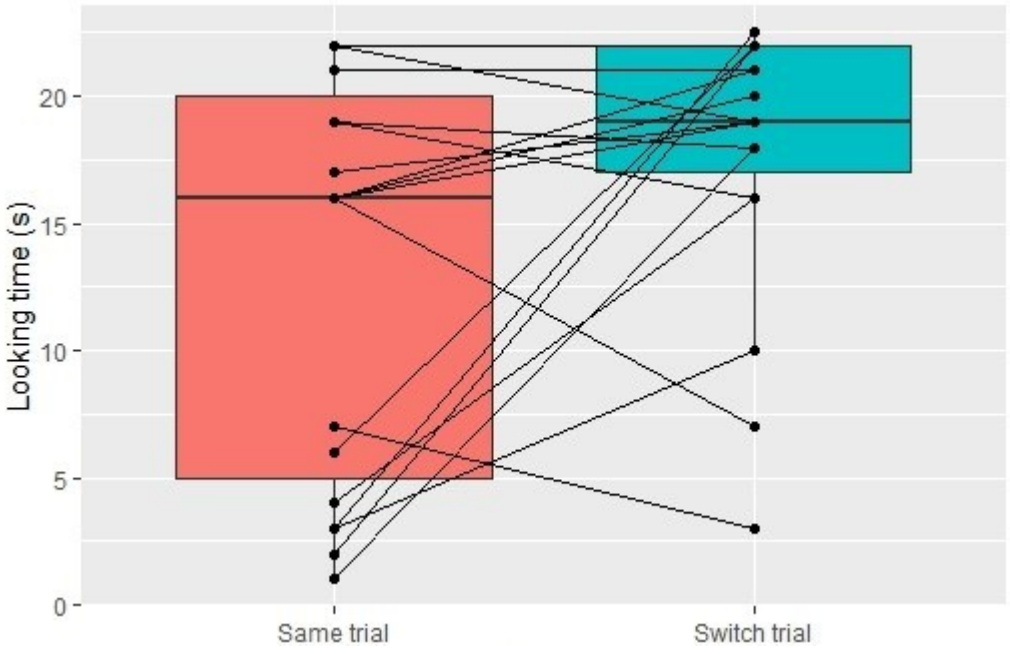


Figure 5: *Experiment 2*. Mean looking times to same and switch trials for each child

A Pearson correlation was conducted between vocabulary knowledge as assessed by the CDI (see Table 2 for vocabulary statistics) and the performance on the Switch task as indexed by the "switch" versus "same" difference score in order to determine whether vocabulary size is related to children's Switch task performance (Werker et al., 2002). The correlation was not significant for comprehensive, $r(17) = -.54$, $p = .816$, nor for production scores, $r(17) = -.34$, $p = .883$. Age and gender did not have a significant effect on children's performance either.

2.3.4 Discussion

In this second experiment, 17-month-old infants successfully learned the association between a new object and a new label, as indexed by their longer looking time in switch trials as compared to same trials. Thus, they were able to encode phonetic detail when learning a new word, which is consistent with previous in-lab findings (e.g., Stage & Werker, 1997; Yoshida, Fennell, Swingley, & Werker, 2009). Our results are also consistent with Bulgarelli and Bergelson's (2022) which showed that younger infants successfully performed the one-word Switch task on Zoom.

No significant relation was found between vocabulary size and performance on the minimal-pair word-learning task, which is not in line with Werker et al. (2002). They found that at 14 months, both comprehensive and productive vocabulary size correlated with performance on the Switch task, and at 17 months, the correlation was found for comprehension only. However, they did not find an association between vocabulary size and performance success

on the Switch task at the age of 20 months. It must be pointed out that many previous studies did not find a consistent relation between vocabulary knowledge as assessed by the CDI and word recognition (Hamilton et al., 2000; Swingley & Aslin, 2000). According to Werker et al. (2002), this would imply that vocabulary knowledge is only predictive of the phonetic detail when children are first building their vocabulary. After the vocabulary reaches some critical threshold, as measured by either comprehension or production, the relation is no longer consistent.

Another unexpected finding is that we did not find a renewed interest in the post-test phase as compared to the pre-test, suggesting that children's interest in the task decreased as the trials went on. It should be noted that Bulgarelli and Bergelson's (2022) Switch task did not have a post-test phase and therefore did not control infants' attention throughout the task. One first reason for our finding is that we used a fixed familiarisation phase, due to technical limitations, contrary to previous researchers who applied a sliding habituation criterion (e.g., Bulgarelli & Bergelson, 2022; Fennel & Waxman, 2010; Werker et al, 2002). Therefore, some of our participants might have lost interest by the time the test phase ended. Maintaining children's interest and engagement for a prolonged period of time can be a limitation of online methods, at least for the Switch task. Another reason might be that our selection of new objects might have been less interesting than for example the objects used by Fennell (2012). Also, the effect size obtained in our study (Cohen's d = 0.53), which is smaller than the effect size of 1.04 by Bulgarelli and Bergelson (2022), was noticeably higher than the average effect size of 0.32 computed in the meta-analysis by Tsui et al. (2019), which might potentially suggest a robust online replication of the main finding in the Switch

task, that is, that children react to a change of word-object pairing. It should be noted that this interpretation cannot be certain as we cannot know whether our results reveal the robustness of the effect or an over-estimate of the effect size.

2.4 Experiment 3: Language assessment task in 19 to 26 months

2.4.1 Introduction

Developmental language research typically involves the estimation of children's language knowledge, which tends to rely on parental questionnaires like the MacArthur CDI (Fenson et al., 2006). However, there are situations where a face-to-face assessment is needed, to complement or replace a parental questionnaire. In this experiment, a comparison between a parental report of the child's vocabulary knowledge and a vocabulary test directly administered to the child was explored (regardless of the setting). But most importantly, we also asked whether administering a test online would provide equivalent data to running it face-to-face. Most available language tests have been standardised with face-to-face data, with clinical evaluation requiring a face-to-face assessment of a child's language skills. It was an open question as to whether similar scores could be obtained for an online and a face-to-face version of the same standardised test. This is a pragmatic question: could early years professionals, practitioners and researchers trust data obtained in a virtual space? In our third experiment, we collected data with a standardised language assessment test, the WinG test (Cattani, Krott, Floccia, & Dennis, 2019) to estimate toddlers' vocabulary knowledge, either online or in the Babylab. It was expected that children's performance on the WinG test would be affected by the environment the test is administered in (home vs

Babylab). Our initial hypothesis was that face-to-face children would outperform online children on the WinG test, because it would be more difficult to maintain their attention remotely, and because sound and picture quality might get in the way of a clear communication.

Parents were also asked to fill in the Oxford CDI, which they would do similarly in their own time, whether the session would take place online or in the lab, and therefore the setting (online or face-to-face) was not expected to affect the CDI scores. Additionally, we analysed whether our WinG scores collected were positively correlated with the CDIs scores. Indeed, when the external validity of the WinG was assessed, a subsample of children performed one or more other language assessments including the Oxford CDI. The receptive score of the CDI was significantly positively correlated with the WinG comprehension subtests (noun (n = 116) and predicate (n = 104) separately). Similarly, the expressive score of the CDI was significantly positively correlated with the production subtests (noun and predicate separately) of the WinG (WinG manual: Cattani Krott, Floccia, & Dennis, 2019).

A sample size of 60 participants was chosen for a study described in another manuscript, which examined the relationship between children's vocabulary knowledge and parental screen time. Before reaching the sample size target, we did not analyse and compare the results of the online and face-to-face groups.

2.4.2 Method

Participants

Seventy children were tested and the data from 8 children were excluded due to the non-full completion of the WinG test (4 online and 4 face-to-face participants). The final sample included sixty-two healthy monolingual infants (31 boys and 31 girls) aged 19 to 26 months who were recruited from the Plymouth Babylab database with the same inclusion criteria as before. Thirty-two participated in the experiment online due to Covid restrictions at the time of testing and thirty were invited to do it face-to-face in the Babylab when restrictions were lifted. Participants were recruited the same way but were not randomly assigned to participate in the experiment remotely or face-to-face as a result of the COVID-19 pandemic.

Materials

After completing a consent form, parents first filled in a demographic questionnaire to collect information about the family's socioeconomic status (SES). Then, they were invited for their child to do a language test, the WinG test (Cattani et al., 2019), either online during the pandemic lockdown, or in the Babylab. At the same time, they were asked to complete the Oxford CDI, prior to the WinG test or during the visit to the Babylab. Parents were also involved in another task related to their usage of screens, with data reported elsewhere (Nguyen, Hanoch, & Floccia, submitted 2023).

For the video chat condition, the WinG was administered online with the Zoom app using computer/laptop devices. The test consists of 44 groups of 3 cards, 4 pre-tests and 40 experimental. Each set of 3 cards contains a comprehension card, a production card and a

distractor card. The comprehension task contains 20 noun words and 20 predicate words, the production task also contains 20 noun words and 20 predicate cards. For each of the four components, a standardised score and percentile for the number of correct answers that should be reached for each age and each gender, can be calculated. Following the WinG recommendations, only the comprehension tasks for both the noun and predicate were administered with children aged from 19 to 24 months old, whereas for children aged 24 to 26 months, the production task for the noun score was additionally given. The WinG scoring sheet was used to code the child's answers, as included in the WinG manual. For the video chat condition, the WinG test was recorded through the Zoom app, and children's responses were transcribed later. For the face-to-face experiment, the WinG test was recorded on a Canon video camera and responses reported afterwards.

Procedure

The parent was sent via email the consent and information forms. Then, the parent and child were invited to participate in the WinG game test. Thirty-two participants did the WinG test online and the 30 children performed the test in-person. Forty-six parents completed the CDI.

For the Zoom session, the WinG cards were set standing against a cardboard box on a table, so that the cards would be visible through the child's screen. The researcher was sitting in a chair behind the table and a laptop was placed in front of the table, facing the picture cards. The child was in a room at home and sat in front of the electronic device using Zoom and the parent sat in a chair next to their child.

For the face-to-face language test condition, the parent and child were invited to enter the Babylab, in which the WinG cards were set upon a table, with two chairs adjacent to each other on the table (for the child and the experimenter). The parent was sitting beside their child. The camera recorded the session to code the responses from the WinG test on the scoring sheet.

The WinG test was administered in line with the instructions from the WinG manual (Cattani et al., 2019), where children were invited to pick up or touch the card corresponding to a target word (in the comprehension task). However, for the WinG test online with Zoom, children could not touch or take the cards. Instead, they were asked to point to their computer's screen at the correct card. The session was video recorded, and the child's answers were scored according to their hand gesture and/or eye gaze going to the right, middle or left card. The WinG test started with 2 pre-tests of 3 cards each to give the child practice of what is required for the game. The 3 cards were presented in a random order in a line in front of the child, one comprehension, and 2 distractor cards. The children were first asked to point out or touch which one the comprehension card was named; once they pointed to one of the cards, it did not matter if it was the right one. Then, the comprehension and distractor cards were taken away to move on to the next set of cards (see diagram in **Figure 6** below). This was repeated for the next set of pre-test cards, all 20 experimental noun cards, the 2 sets of pre-test cards for the predicate condition and all 20 experimental predicate cards. Praise was always regularly provided, irrespective of the child's answers.

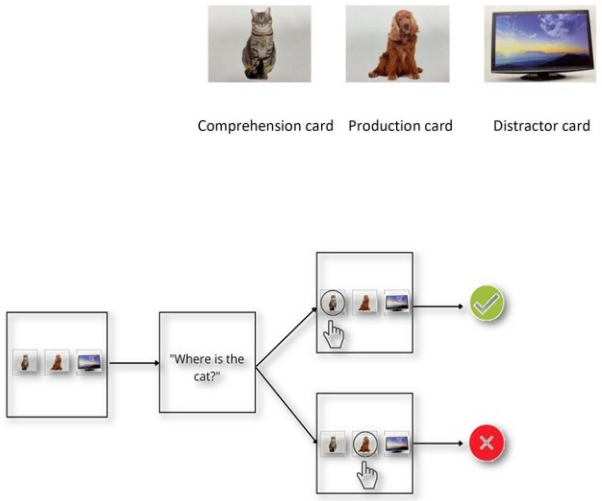


Figure 6: *Experiment 3.* Diagram of the structure of the WinG

The WinG test was performed to the best of the child's ability, lasting around 30 minutes. Not all children can stay focused during the entire length of the testing session. Following the WinG manual, when a child began to show signs of boredom or restlessness, he/she was offered a short break (e.g., getting a snack or drink). When the child was ready to resume testing, the administrator restarted from the last set of pictures before the break. If necessary, the test was stopped and was resumed another day within one week. The data collected for this study was the children's percentile score for noun comprehension and predicate comprehension as calculated by the standardised scores in the WinG manual. Moreover, the two parents' highest educational levels were used as the SES. The parent's postcode was collected with the demographic questionnaire and was used as a proxy for income, leading to the income deprivation score (IDS). The IDS were obtained from a government website (Ministry of Housing, Communities, and Local Government, 2019). The

scores hold significance and correspond to the percentage of the relevant population experiencing that type of deprivation in that area. So, for instance, if an area receives a score of 0.27, it indicates that 27 percent of the population in that area is experiencing income deprivation. The larger the score, the more deprived the area.

It should be noted that the production task data was not reported here because none of the online children did the production task of the WinG test, so we could have not compared production scores between the online and face-to-face participants.

2.4.3 Results

Table 7 provides the descriptive data for the children's ages, gender (31 boys and 31 girls), parental education, income deprivation scores, CDI scores, and WinG scores.

***Table 7**: Experiment 3.* Descriptive data of the sample

Means and Standard Deviations of the children's age, gender, CDIs scores and WinG scores.

	M	*SD*
Age (days)	666.89	63.48
Boys' age (days)	666.32	60.64
Girls' age (days)	667.45	67.20
Parental education	4.82	0.82
IDS	0.10	0.06
CDI knows (percentile)	69.93	17.43
CDI says (percentile)	34.52	28.26
WinG nouns (percentile)	37.02	23.67
WinG predicates (percentile)	40.08	21.17

There was an absence of correlation between parental education and the income deprivation score (r = -.062, N = 77, p = .63). Therefore, only parental education was kept as the SES indicator as it is usually the best predictor of children development (Davis-Kean, Tighe, & Water, 2021; Duncan & Magnuson, 2003).

First, participants from the two groups were compared on demographic measures. The online group included 16 boys and 16 girls, and the in-person group had 14 boys and 16 girls. Online participants had similar educational levels (M = 4.94, SD = 0.70) to the in-person parents (M = 4.70, SD = 0.91; t(60) = -1.15, p = .25. Children from the online group were

about a month younger a month younger (*M* = 649.22, *SD* = 54.14) than those in the in-person group (*M* = 685.73, *t(*60) = -2.35, *p* = 0.02).

Then correlations were made between the CDI scores and the WinG scores. No associations were found between the CDI comprehension scores and the WinG comprehension (neither on nouns nor on predicates) scores. Our sample might have not been large enough to detect a relation between the CDI and WinG scores.

Next, independent t-tests were conducted to compare the online and face-to-face children's WinG performances. The results were adjusted by the Bonferroni correction (Abdi, 2007) as the nouns and predicates are both measures of comprehension. Thus, the significance value was divided by 2 and adjusted to 0.025.

It should be noted that standardised WinG scores incorporate age and gender. Online children performed significantly better on the WinG test noun comprehension (*M* = 45.47, *SD* = 22.05) than face-to-face children (*M* = 28.00, *SD* = 22.27); *t* (60) = 3.10, *p* = .003. Similarly, online children did better at the WinG test predicate comprehension (*M* = 45.94, *SD* = 21.08) than the in-person group (*M* = 33.83, *SD* = 19.73), *t(*60) = 2.34, *p* = .023. **Figure 7** below illustrates the comparison of the online and in-person performances on the noun comprehension.

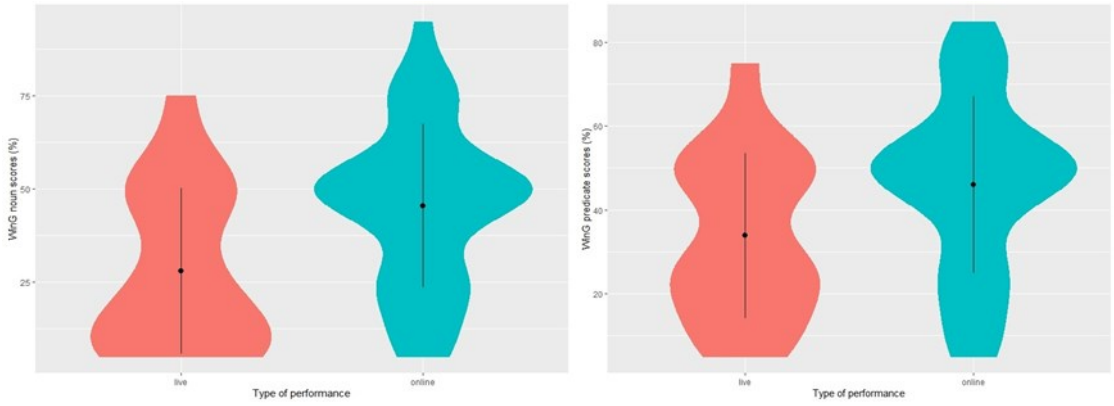


Figure 7: *Experiment 3.* Comparison of WinG comprehension scores between online participants (N = 32) and face-to-face participants (N = 30).

On CDI comprehension, a regression model forcing age, parents' education, gender and the type of performance (online/in-person) led to a significant model (R^2 = .32, $F(4,41)$ = 4.83, p = .003) with only age as a significant contributor (β = .15, t = 4.09, p < .001). On CDI production, the same regression model led to a significant model (R^2 = .37, $F(4,41)$ = 5.90, p = .001) with only age (β = .20, t = 3.44, p = .001) and gender (β = 17.52, t = 2.50, p = .016) as significant predictors.

Independent t-tests were conducted to compare the online and face-to-face groups on their CDI comprehension and production scores. No corrections for multiple comparisons were made as comprehension and production vocabulary scores are different measures of language. Results indicated that there were no significant differences on the CDI comprehension between online children (M = 68.06, SD = 16.57) and in-person children (M = 71.03, SD = 18.11); $t(44)$ = 1.06, p = .57. Also, there were no significant differences on the CDI production between online participants (M = 26, SD = 20.56) and face-to-face

participants (*M* = 39.52, *SD* = 31.18); *t*(44) = -1.60, *p* = .12. Children who were tested online did not have significantly higher scores on the CDI. It supports the finding that online participants outperformed those who did the language test at the Babylab, but only for the WinG test. However, no significant differences on the CDI scores between the online and face-to-face groups does not establish that there is no difference between the two groups, which we will discuss further in the discussion.

2.4.4 Discussion

In this last experiment, we investigated the reliability of using a language assessment test, the WinG, online as compared to face to face. We originally expected that the children who did the WinG in the Babylab would outperform the online participants. Indeed, online children were not able to touch or take the cards which could diminish their engagement. In addition, they might have not seen the pictures and heard the words as clearly as in face-to-face interaction. However, the findings are exactly opposite to this hypothesis as online children outperformed the in-person group. Critically the two groups did not differ on CDI scores. A possible explanation for those results is that online children might have been more focused on the task because, first, they were in a familiar environment at home, and second, looking at a computer's screen might be more unusual and compelling. This is in line with what was found in the two previous experiments, where high effect sizes and low attrition rates were observed when testing online. Those results are in line with Nelson, Scheiber, Laughlin and Demir-Lira (2021) who compared children's performances between face-to-

face and online tasks. They tested children aged 4 to 5 years old on various tasks related to working memory, visual spatial, and numeral competences for example. On five tasks out of eight, findings did not reveal differences across the two formats that they administered, but on the three other tasks (two related to verbal comprehension and one related to fluid reasoning), online children were found to outperform face-to-face ones.

There could be other explanations for our findings. Participants were recruited the same way and have similar SES but were not randomly assigned to participate in the experiment remotely or face-to-face as a result of the COVID-19 pandemic. Differences in composition of the sample can be one of the reasons why the online and offline group results differ. Kartushina et al.'s (2022) findings suggested that children from middle-class families during the first lockdown showed vocabulary gains and had higher CDI language scores (based on normative data) than pre-pandemic children. However, in our experiment, we did not find a difference in CDI scores, so our results are unlikely to be due to the online group having better language skills than the offline group. It might be more likely that participants recruited during the lockdowns might have performed better on the language test because they might have been more at ease with computers due to parents engaging more with them this way at home.

Our findings demonstrate that online data collection might be a feasible option for children's language assessment; however, it also means that norms may not be useful when testing online. Identifying children with language delays based on online scores would lead to potential misses, because a child who would score on the 10th percentile face to face might score on the 20th when tested online. Note that our data do not allow us to conclude firmly

in this direction: the face-to-face group scored around 30 on the WinG test, and the online group around 50. As expected of standardised scores, 50 is what would be expected from a representative group similar to the population from which standardised scores were derived. It is possible that our face-to-face participants scored particularly low, or that our online children particularly high. The important conclusion here is that children tested online, and who were drawn from the same population as those tested face to face, outperformed the latter. It would have been interesting to replicate these findings, but the data collection opportunity was unique and unrepeatable due to the exceptional lockdowns' circumstances.

2.5 General Discussion of the online Experiments 1, 2 and 3

We adapted three paradigms into online experiments to investigate various ways to estimate looking behaviour in young children. The results from the three experiments provide support for online testing reliability. With some modifications to lab-based procedures, the IPL and Switch tasks successfully collected eye movement data and provided solid replications of established results. In Experiment 1, previous lab-based findings were replicated (e.g., Vihman et al., 2007) and showed word recognition in children. In Experiment 2, infants significantly learned a new word which is consistent with previous in-lab (e.g., Yoshida et al., 2009) and online research (Bulgarelli & Bergelson, 2022) involving the Switch task. Finally, Experiment 3 demonstrated that children can perform well on a language assessment test administered online and that they were strongly engaged and responsive to the task.

The three experiments presented here have highlighted a number of advantages to testing in an online paradigm. Firstly, there can be high levels of engagement for young participants when tested in the home environment (Experiments 1 and 3). Indeed, we found that instead of being distracted by their surroundings, children remained engaged for the duration of the experiment which might be due to children feeling more comfortable and at ease in their home, according to Tsuji et al. (2022) A higher level of engagement in online experiments might also explain why children performed better in our Experiment 3 and in other previous studies (e.g., Bacon et al., 2021; Nelson et al., 2021).

Another advantage to testing online was the higher-than-expected effect sizes (Experiments 1 and 2). Comparing online to in-lab testing we found that effect sizes were not only replicated but were much higher in magnitude. This is promising support for testing online, especially for studies in which small effect sizes are usually expected (e.g., semantic priming studies).

Another interesting finding was that trials which are not infant-led still replicated findings in-lab (Experiments 1 and 2), which generally require participants to attend to the screen before proceeding to the next trial.

A final, but important advantage to testing online was our finding of very little attrition or data loss (e.g., Experiment 1 <5%) compared to some lab-based studies, which can lose up to 30% according to a meta-analysis performed by Bergmann et al. (2018).

Having considered the advantages to testing online, we now turn to specific considerations when testing online. As with all new findings, more replication studies are required before

generalising beyond these three paradigms that testing online is suitable for other infant paradigms and other infant populations.

Another point to consider is that children's attention might fade throughout the online session (Experiment 2). Indeed, Tsuji et al. (2022) reported that it may be more difficult to maintain children's engagement and interest during online tasks than in the lab. They quoted Chuey et al. (2021) and Shields, McGinnis, and Selmeczy (2021) who recommended to keep the tasks short and to elicit regular responses from children with synchronous tasks to monitor children's engagement.

Additionally, experimental findings in online testing might differ to clinical measures, such as being able to identify language delays (see Experiment 3).

Another limitation is that certain types of paradigms might not be adaptable to an online format depending on the age. Indeed, Lapidow, Tandon, Goddu, and Walker (2021) showed age-related differences in the performance of young children (aged between 2 and 5 years) that are not apparent when conducting studies in person. Their study examined the same developmental task across three different methodologies: in-person, an online synchronous version, and an online asynchronous version. They investigated whether children's inferences of unobserved populations are influenced by the variability of the observed samples. To examine this, children observed an experimenter randomly selecting balls from two identical containers (Lapidow et al., 2021). One container contained four balls of different colours (varied-sample), while the other container contained four balls of the same colour (uniform-sample). Subsequently, children were asked to determine which container was more likely to hold a ball of a different colour. In the online context, only children older

than 3.5 years in the synchronous version and above 4 years in the asynchronous version performed above chance. The findings suggest that children's age significantly influences their performance in an online setting. Notably, older children performed better compared to younger children. These results differ from what would typically be observed in a lab or in-person setting.

An important consideration when testing online is that some platforms collecting eye movement data can involve offline coding of video data which is time-consuming (see Nelson & Oakes, 2021), though there are platforms, such as LabVanced, which can automatically code the looking behaviour (see Bánki et al., 2022). Despite this fact, performing offline coding on the video data can reduce data loss (see Venker et al., 2020) compared to the automatic calculations performed by in-lab eye-tracking software. Data loss from testing in a lab setting tends to occur when an eye-tracker loses connection, but manually assessing each frame when coding video data offline for online experiments does not present this issue.

Though our findings do not indicate that parents influenced the behaviour of their children during testing, the lack of control over the testing environment and how parents behave during testing should be considered. At a very minimum, clear instructions should be given (with instructional images or videos where possible) to the parents, indicating how they should behave, with an explanation of why this is important. However, further online testing might indicate if this type of instruction is necessary during at-home or in-lab testing, which could inform the instructions we give to parents during in-lab testing.

A final limitation to testing online is that while online testing has the potential to reach broader demographic groups in theory, in order for online testing to work well, basic requirements such as a suitable up-to-date device and a stable internet connection are linked to a financial situation and lifestyle that enable this access. Furthermore, using the same avenues for recruitment (i.e., an institutional database of families) does not extend our reach to test under-represented groups.

These studies provide encouraging support that other infant paradigms might be suited for adaptation to online testing. For instance, paradigms for measuring children's knowledge of syntax can be applied to online testing such as the elicited production that investigates whether young children have abstract knowledge of a particular structure (e.g., Ambridge, 2011). Paradigms to assess socio-emotional regulation in infants can also be adjusted for online experiments, for example the Face-to-Face Still-Face paradigm (e.g., Barbosa et al., 2020; Giusti, Provenzi, & Montirosso, 2018) where the parent and infant engage face-to-face for 2 min (e.g., Play episode). Next, the parent is told to stop engaging and communicating with the child. Instead, they are instructed to maintain eye contact with the child while keeping a still face for 2 min (e.g., Still-Face episode). This paradigm could work online by video live recording the interaction between the caregiver and the infant via a video call application. Indeed, for example, a recent study by McElwain et al. (2022) validated an online procedure that assessed mother and infant behaviour during the Still Face Paradigm (SFP). They compared data collected during in-person lab visits with data collected during remote Zoom visits to establish the validity of the online procedure. For the online procedure, prior to the online session, mothers received an email giving information

about the necessary equipment needed for the Zoom visit, such as a bouncy seat or high chair (McElwain et al., 2022). The online session was recorded and during the visit, the experimenter used the Zoom's screen sharing functionality to display slides containing comprehensive instructions for each task. Throughout all the activities, the experimenter disabled the video camera and microphone, with the exception of the Baseline video where he/she remained unmuted. He/she collaborated with the mother to find the most suitable video angle, ensuring that the faces of both the mother and infant were captured effectively (McElwain et al., 2022). When comparing virtual visits to laboratory visits, during the SFP, mothers and infants had similar vocalisations, gaze directions and proportions of facial expressions. Additionally, infants also displayed similar behavioural changes across SFP episodes.

In conclusion, this paper demonstrates that the recent pandemic has inadvertently opened promising avenues of investigation in early language studies, and it is likely that future research will harvest the benefits of the enforced development of online experiments, reaching out to multicultural and multilingual populations around the world.

3 Chapter 3: Parental screen time and children's language development

This chapter aims to explore the associations between children's and parental screen use and young children's language development. To provide a comprehensive context, we will begin with a summary on the development of children's word comprehension and word production from birth to 3 years old (based on the thesis introduction). Following this, we will present a summary of why parental screen time could affect children's language development (also based on the thesis introduction). Then, we will describe two studies that have explored the relationship between parental screen use and children's language development. We will focus onto Reed et al.'s (2017) study who found a negative impact of parental phone call use on children's word learning in a lab situation. We will also describe a study by Konrad et al. (2021) who did not find that parental phone texting could impact action imitation learning in young children in a lab situation. We will introduce our Experiment 4 which aims to replicate and extend Reed et al.'s (2017) findings but using parental phone text instead of phone call. Then, after investigating the relationship between parental screen use and children's language skills in a lab situation, we will question the implications of parents' screen time effects on children's language development outcomes in real life. We will describe a study by Mustonen et al. (2022) (previously presented in the thesis introduction) which revealed a negative association between reported mothers' screen time and their children's (aged 2.5 years and older) vocabulary knowledge. Finally, we will present Studies 1 and 2 which, similar to Mustonen et al. (2022) but with younger children, explore whether parental screen time effects can be found on their language

development. Study 1 involves a parental questionnaire to estimate children's vocabulary size (Oxford CDI: Hamilton et al., 2000) and a survey estimating their screen time and real-life habits. Study 2 includes more objective measures using a standardised face-to-face vocabulary test (WinG; Cattani et al., 2019), and a daily screen time questionnaire.

3.1 Literature review

Development of word learning in early childhood

Infants start understanding familiar words around 6 months old (Tinkoff & Jusczyk, 1999) and begin to produce words by their first birthday (Huttenlocher et al., 2010). They achieve the significant milestone of uttering approximately 50 words by the time they are around 18 months old (Ganger & Brent, 2004; Waxman & Kosowski, 1990). From then on, their word production and comprehension abilities accelerate noticeably, acquiring 100 words by the time they are between 20 and 21 months (Pine, 1995). At 2 years old, they typically know between 200 and 500 words and start combining words into two-word phrases (Fernald et al., 2001). As they grow older, young children become capable of forming longer and grammatically correct sentences. By the age of 3, most children can engage in short conversations, comprehend others' speech to some extent, and respond appropriately (Haslett & Samter, 1997).

Parental screen use and children's language skills

Most of the studies investigating screen time effects on children's language development have focused on children' screen usage rather than specifically examining the screen use of

parents (Corkin et al., 2021; Kildare & Middlemiss, 2017). However, parents might be less socially and emotionally active with their children because of mobile phone distraction (Kildare and Middlemiss, 2017). Radesky et al. (2014) observed the behaviour of 55 caregivers at fast food restaurants in the Metropolitan Boston area. Children whose parents were engrossed in their phones were more prone to behaving in a silly or noisy manner. Moreover, parents who used cell phones appeared to be irritable and impatient, which ultimately contributed to worsened behaviour. Radesky et al. (2014) suggested that mobile devices can distract parents from face-to-face interactions, potentially hindering children's development of essential milestones (Glascoe & Leew, 2010). Thus, distraction from electronic devices might influence the quality and quantity of time that parents spend with their children, which would translate into slowing down language development. So far, only a few studies have investigated how parental screen time can be associated with early language and cognitive development. The most relevant one is a within-participant study conducted with 38 mothers and their 2-year-olds to test the impact of parental cell phone use on children's verb learning (Reed, Hirsh-Pasek, & Golinkoff, 2017). Parent-child dyads were brought into a room. During the teaching phase, mothers were asked to teach their children two new verbs (blicking, which was to mean "bouncing," and frepping, which was to mean "shaking"). Mothers were given 60 seconds to teach the first novel verb. Then, they proceeded to teach the second verb after receiving instructions from the experimenter through a phone call to do so. Another 60-s teaching period ensued and concluded when the experimenter knocked on the door (Reed et al., 2017). During one of the teaching periods, (specifically after 30 seconds had elapsed), the experimenter made a phone call to

the mothers in which they talked to each other for 30 seconds. Then, they had another 30 sec to teach the target word. Total teaching time for one word in this interrupted condition was still 60 sec, the same as in the uninterrupted condition. Children's verb learning was indexed by their increase in preference for matching and nonmatching actions before and after each session in a preferential looking task. It was found that children in the interrupted condition did not show evidence of learning the target verbs while in the uninterrupted condition they succeeded. This suggests that parental distraction through the use of mobile devices is negatively associated with word learning in young children.

3.2 Experiment 4: Word Learning with phone text interruption

3.2.1 Introduction

We decided to replicate and extend Reed et al. (2017)'s findings that in lab situations, parental technoference (the interference of parent–child interactions due to parents' use of technology; McDaniel & Coyne, 2016) can influence word learning, but using texting distraction instead of phone calls, to better represent the main communication activity most people engage with on their phone. Another reason is that a distracting phone call necessarily implies spoken language, which may have partially erased phonological short-term memory traces of utterances produced by parents before the phone call. In other words, the absence of learning in the interrupted condition would not reflect an effect of parental technoference, but a memory effect due to the exposure to unrelated, additional spoken language. Using text messages will allow us to test for the effect of technoference alone. Additionally, we chose to test younger children aged between 17 and 19 months to

assess the effect of parental screen at the onset of vocabulary growth, and therefore focused on object nouns rather than verb learning, as the former are typically the primary word forms that are learned. Waxman and Booth (2001) showed that infants as young as 14 months can identify novel noun words (e.g “This one is a blicket”) and map them to new objects (e.g., rolling pin, purple plate), while Werker et al. 's (2002) demonstrated that infants of 17 months could apply phonetic detail when learning new words within a short exposure period.

It must be noted that another study by Konrad et al. (2021) did not find that parental phone texting could impact learning in young children of 19 months in a lab situation. In their study, parents taught actions to their infants by demonstrating three target actions to build a rattle (put the ball in the jar, put the lid on the jar, shake the stick), four times. A phone text was sent before and during the demonstrations to investigate whether texting can interrupt imitation learning in young children. Moreover, to provide a realistic context for the phone use, all parents were told that they would open a text message on a smartphone to answer a questionnaire about how they felt while teaching something new to their infant. After the demonstrations, infants had the rattle pieces within reach and were given 60 sec reproduce any target action. Parents were instructed to not help them. To code imitation learning, the infant received a point if he/she did, push the ball into the jar, put the stick on the jar, and shake the stick. For each child, an imitation score was determined by summing the number of target actions (Konrad et al., 2021). Results suggested that smartphone use did not impact learning of the target actions, which is not consistent with the Reed et al.’s results (2017). However, the task used by Konrad et al. (2021) did not involve word learning.

Also, imitation learning does not necessarily involve verbal interaction, contrary to the word learning task used by Reed et al. (2017).

In summary, the basic design in Reed et al. (2017) was that parents were asked to teach a new word to their child in the lab and were being interrupted – or not - during the session by a phone call. Children's learning of the target words was then assessed in a preferential looking task. In the current study, parents were told that they would receive a text message on a smartphone asking them to complete a questionnaire about how they felt during that task.

In another depart from Reed et al. (2017), we manipulated the instructions provided to parents. Reed et al's. (2017) paper does not explicitly state if the researcher told the parents the true purpose of the study in the instructions, and especially whether parents were told that the researchers wanted to investigate the impact of phone calls on word learning. The researcher simply shared directions with the mother, indicating when it was time to transition from one word to another, and that the researcher could call and "chit chat with you, like we're friends in real life". In Konrad et al.'s (2021) study, all parents were told a cover story and not the true purpose of the study. In our study, we used a between-participant design, where half of parents were told the true purpose of the study, namely that we were examining whether parental phone texting can impact word learning in children. The other half were told a cover story similar to Konrad et al's. (2021), namely that we were interested in investigating how children learn new words from their parents and how parents felt while they teach a new word to their children. We hypothesised that the

true story condition would produce the least effect on phone text interruption because parents would probably overcompensate by repeating the word more often before and after the interruptions. This manipulation was meant to tear apart what was due, in Reed et al.'s (2017) results, to some parents guessing the true purpose of the situation and compensating for the interruption.

For our current study, we also measured parents' screen time to capture realistic daily phone use of parents. Parental average screen time of the week was collected based on their phone data, to examine whether it would have an impact (outside of a lab situation) on their child's total vocabulary (as assessed by the CDI) or on their performance in the word learning task (in a lab condition).

Finally, following Reed et al. (2017), the parent-child interaction was video recorded and analysed to investigate whether the child's behaviour was affected by the phone text interruption (for example, by demanding attention, or losing interest) and whether it could be related to their word learning performance.

3.2.2 Method

Participants

The study protocol was approved by the University of Plymouth Faculty of Health Ethics Committee. Forty-two healthy monolingual children aged 17 to 19 months (14 boys and 28 girls) were recruited from the Plymouth Babylab database (*M* = 17 months 23 days, *SD* = 22 days, ranging from 16 months 27 days to 19 months 3 days). Data from an additional 2 children were excluded because parents did not complete the questionnaire sent to them

by text during the task, failing to follow protocol. Additionally, another group of 22 children did not fully complete the word recognition eye-tracker task because of fussiness or their data were excluded because of poor eye-tracker data.

Materials

During their visit at the Babylab, the parents first completed a demographic questionnaire to collect information about the family SES. At the same time, they were asked to complete the short form of the Oxford CDI (Floccia et al., 2018; Hamilton et al., 2000). Then, during the word teaching phase, they completed an online Qualtrics questionnaire about their feelings while teaching on their smartphone that consisted of 5 questions (e.g. "How comfortable do you feel with teaching words to your child?") and multiple-choice answers (e.g. extremely comfortable, very, moderately, slightly, not at all) (see **Appendix A** for more details). Finally, they were asked to show their daily average screen time of the week on their phone (they had previously been asked to turn on their smartphone screen time feature in the week preceding the Babylab visit).

Design and Variables

This study used a 2x2x2 mixed design to contrast word learning across an interrupted and uninterrupted teaching period. Following Reed et al. (2017), parents taught two novel words to their child, one at a time, counterbalanced for order of presentation (factor Order). Counterbalanced assignment determined whether during the first or second teaching period, parents would be interrupted to fill a questionnaire for a short amount of time (factor Interruption; average time to complete in our sample ~34 seconds). Finally, half of

the parents were told the true purpose of the study and the other half were told the cover story (factor Scenario; see in the procedure for more details).

A Tobii 300 eye tracker (Tobii Technology, Danderyd, Sweden) was used to assess children's comprehension of the novel noun words after the word learning task. By corneal reflection techniques, the x and y position of the eye on the screen was recorded at 120 Hz. The pictures were shown on a screen that was positioned approximately 70 cm from the child's eyes. Following Reed et al. (2017), children saw two pictures (a target and a distractor, presented side-by-side simultaneously) as they heard an instruction to find a particular object. If children learned the new words, they should look more at the target picture. The proportion of time that children attended to the target picture during test trials was the dependent measure.

Procedure

Parental training. Before each session, an experimenter showed each toy to parents in a separate room and labelled them with the novel word. A rolling pin was the object for the noun 'blicket', and a screwdriver was used for 'poma', and it was checked with the parents that the children did not know the real words for these objects. The experimenter told the parents to expect an occasional text from her on their cell phone set to sound loud. Then, parents received instructions on how to open and complete the questionnaire. They were asked to fill the questionnaire as soon as they received the phone text, and then to resume teaching the word. Participants received different sets of instructions (counterbalanced):

Scenario 1: Parents were told the true purpose of the study, namely that we were investigating whether texting can impact word learning in children. Parents were told that they would teach 2 new words to their infants, before doing the word recognition task in the eye-tracker booth.

Scenario 2: Parents were told that we were interested in investigating how children learn new words from their parents and how parents feel while they teach a new word to their children (Konrad et al., 2021). Parents were told that they would teach 2 new words to their infants, before the word recognition task in the eye-tracking booth. We explained: "To not disturb the interaction between you and your child, we will be in the room next door and will ask you questions about how you feel during the teaching task via a smartphone."

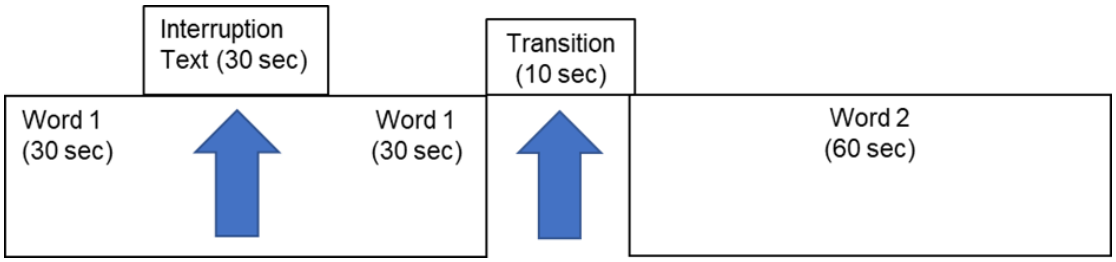
Warm-up play. Dyads were given a few minutes for unstructured free play period before the teaching phase.

Teaching phase. To initiate the teaching phase, the experimenter talked to the parent and instructed him/her to teach poma and blicket (order counterbalanced). Parents were given 60 sec to teach each novel word in the soundproof booth. Parents transitioned to the second word after they received the other toy/object from the experimenter instructing them to do so (see **Figure 8**). Another 60 sec teaching period followed and ended once the experimenter entered the booth. Parents were told that they could teach the words the way they want to and repeat the words as many times as they would like to. The whole

teaching session was recorded with a Canon video camera and children's behaviour was coded offline from videos.

Interrupted period. The experimenter sent a text to the phone during the middle of the teaching phase of word 1 or during the teaching phase of word 2 depending on the interruption condition. Following Konrad et al. (2020), when the parent heard the smartphone text notification, they picked up the phone and opened the message. They then were redirected to a Qualtrics Survey asking them how they currently felt, and where they were presented with a list of 5 different adjectives, each on a 5-point Likert scale ranging from "not at all" to "very" (e.g., "At the moment, I feel calm"). However, unlike Konrad et al. (2020), 5 adjectives and not 6 were used since our pilots revealed that it took approximately 30 sec to complete the survey and was therefore similar to the interruption period employed in the study by Reed et al. (2017). The survey concluded with a brief instruction for the parent, stating, "Thank you. You can now continue teaching the word." Parents had 30 sec more to teach that target word. As in Reed et al.'s (2017) study, the total duration of teaching time in the interrupted condition was 60 seconds, which was the same as in the duration in the control (uninterrupted) condition.

A



B

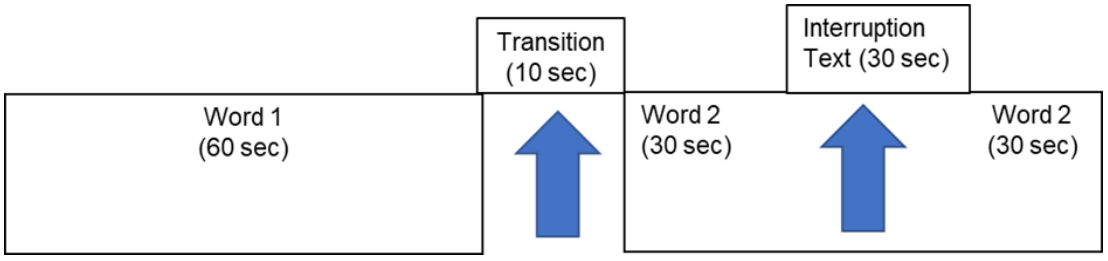


Figure 8: *Experiment 4.* (A) Teaching phase in interruption-first condition. (B) Teaching phase in interruption-second condition

Word recognition test. Following the teaching phase, children were taken to the eye-tracking booth and seated on a baby car seat or on their parent's laps. Two-second centering trials presented a smiley emoticon centrally on the screen before each test trial. Each noun was presented via 5-s trials, with its onset starting at 2500 ms so that it separated the pre-naming phase from the post-naming phase. For each trial, two images were played side-by-side with audio instructions (e.g., "Look at the poma"): one represented the target object and the other one showed a distractor object. In total, children saw 10 trials including 4 control trials, 4 target trials and 2 mutual exclusivity trials (see **Table 8** and more details below).

Table 8: *Experiment 4.* Order of Test Trials with Accompanying Audio Instructions

Trial	Type	Audio
1	Control	“Find penguin”
2	Control	“Find spoon”
3	Control	“Find apple”
4	Control	“Find elephant”
5	Target	"Find poma/blicket"
6	Target	"Find poma/blicket"
7	Mutual	"Now find jaben/zoke"
8	Mutual	"Now find jaben/zoke"
9	Target	“Find poma/blicket again
10	Target	"Find poma/blicket again"

Control trials. The pictures included 4 pairs of images with common animals/objects. Each picture showed a different object/animal (e.g., a ball and a penguin).

Target trials. Children were asked to find the object for each target novel word taught previously (e.g., “Find blicket”). Each trial showed the picture or the target and a distractor which was a new object (that children were unlikely to know the label of), and the order was counterbalanced. The target picture for *blicket* was a rolling pin and the distractor picture was a flute. The target picture for *poma* was a screwdriver and the distractor picture was a hair dryer.

Mutual exclusivity trials. Children were asked to find an object related to a new unfamiliar word (*jaben*, *zoke*). Each trial showed the picture of the target and a distractor which was one of the learned objects (rolling pin or screwdriver), and the order was counterbalanced. The target picture for *jaben* was a flute and the distractor picture was a rolling pin. The target picture for *zoke* was a hair dryer and the distractor was a screwdriver.

Coding. The eye-tracker recorded children' eye gaze data with a 120 Hz frequency. Children's knowledge of a particular word was indexed by higher looking times towards the target object than during the distracter in the post-naming phase, as compared to the pre-naming phase. The proportion of looking time (PLT) towards the target visual stimulus was calculated as a dependent variable: looks to target / (looks to target + looks to distractor).

Other dependent variables were the weekly average parental screen time estimates as directly reported by the parents based on their phone data as a measure of parental screen time, and the total time parents spent filling the Qualtrics questionnaire[6]. Moreover, the highest educational level achieved by either parent, ranging from 1 to 6 (low to high: from primary school to postgraduate degree) was used as the proxy for SES. The CDI scores were used as estimates of children's vocabulary skills.

[6] The parents' questionnaire answers about their feelings while teaching were not analysed as we are interested in the questionnaire interruption effect itself.

3.2.3 Results

The difference in the proportion of looking times at the target pictures (difference between PLT in the post-naming phase minus the pre-naming phase) was used as an indicator of the child's word learning. The 42 parental educational levels and 35 Income scores of deprivation (7 missing data) were used as indicators of the parents' SES. Finally, 37 CDI scores (5 missing data) and 36 parental screen time estimates (6 parents did not know their screen time) were collected. Outliers were defined as values with standardised *z*-scores greater than 1.5 or less than -1.5. Examination of total time spent teaching in the interrupted condition revealed 7 outliers: during the interrupted period, these parents (who were given Scenario 2, the cover story) took up the whole remaining time of the learning task to fill the questionnaire, so they were excluded for further analyses as they failed to provide data for the post-interruption period. After removing outliers, we had a final sample size of 35 children. **Table 9** below provides the descriptive data for the children's age, gender, parental education, income deprivation scores, CDI scores, plus looking times at the target variables.

Table 9: *Experiment 4.* Descriptive data of the whole sample

Means and standard deviations of the children's ages, parental education, IDS, and the variables of PLT (proportion of target looking time) during the different types of trials (control, target and mutual exclusivity)

	n	*M*	*SD*
Age (days)	42	540.76	21.52
Parental educational level	42	4.81	1.17
Income score of deprivation	35	0.10	0.06
Screentime (min per day)	38	102.00	72.84
CDI comprehension	37	43.49	16.82
CDI production	37	11.97	12.54
pre-naming control trials PLT	35	0.59	0.13
post-naming control trials PLT	35	0.61	0.15
difference of control trials PLT	35	0.03	0.03
pre-naming target words PLT	35	0.48	0.11
post-naming target words PLT	35	0.62	0.16
difference of targets word PLT	35	0.14	0.17
pre-naming mutual exclusivity PLT	35	0.43	0.20
post-naming mutual exclusivity PLT	35	0.43	0.20
difference of mutual exclusivity PLT	35	<-0.01	0.24

Word Learning

The PLT variables were not modulated by children's gender, age, income scores of deprivation, or parental education differences as evidenced by t-test and correlation analyses, therefore these factors will not be included in the analyses below.

First, to examine if children recognised the control words and learned the target words, one-way t-tests were run on the PLT difference against a test value of 0. Children correctly recognised the familiar words in control trials, $t(34) = 4.75$, $p < 0.001$, $d = 0.14$. For the target trials, the PLT difference was significantly different from 0 as well, $t(34) = 4.88$, $p < .001$, $d = 1.02$, showing that children successfully learned the target words. However, PLT difference was not different from 0 for the mutual exclusivity trials, $t(34) = -0.36$, $p = .972$, $d = 0$. When presented with a new word (*jaben*/*zoke*) paired with a now familiar object (the pin roller or the screwdriver), children did not successfully map it with the picture that had not yet been labelled.

Second, we ran a repeated measure ANOVA on PLT difference of the target words with Interruption as a within variable (interruption: present, absent), Order of Interruption as a between variable (1=interrupted for first word, 2=interrupted for second word), and Scenario as a between variable (1 versus 2). No significant main effects of Order $F(1,31) = 0.61$, $p = .442$, $\eta^2_p = .02$, nor Interruption $F(1,31) = 0.77$, $p = 0.39$, $\eta^2_p = .02$, were found. In particular, there was no interaction between these variables $F(1,31) = 3.02$, $p = .092$. This means that the order of the taught words did not impact children's word learning, and they did not look longer at the matching picture of the word taught during interruption than during no interruption. However, a significant main effect of Scenario was found, $F(1,31) =$

4.50, $p = 0.042$, $\eta^2_p = .13$ but no interaction between Scenario and the other factors. Children looked longer at the target words/pictures following scenario 1 ($N = 21$, $M = 0.20$, $SD = 0.14$) than scenario 2 ($N = 14$, $M = 0.06$, $SD = 0.05$); $t(33) = 2.25$, $p = 0.035$, Cohen's $d = 1.33$. Children whose parents were given scenario 1 (the true story) learned the target words better than those whose parents were given scenario 2 (the cover story) (see **Figure 9** below).

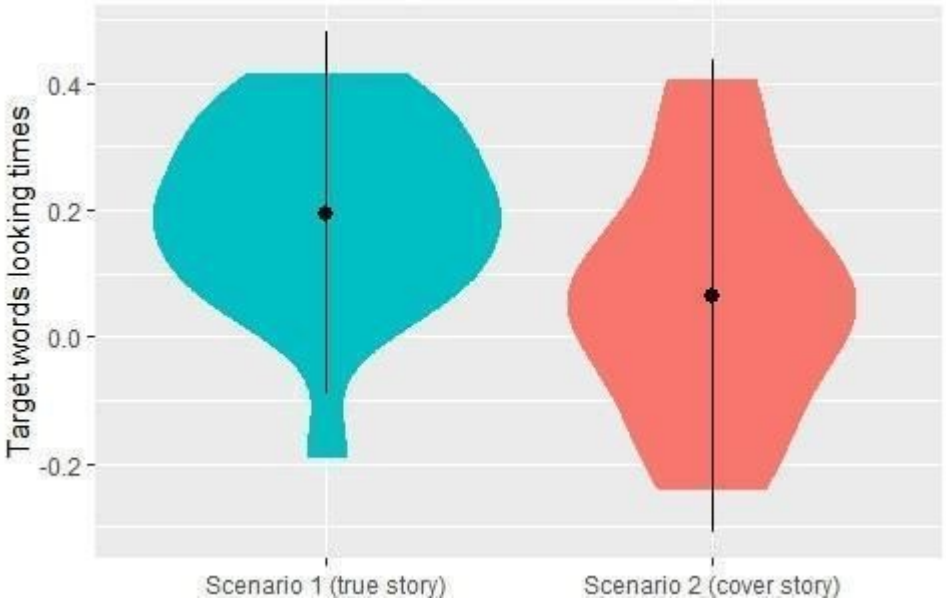


Figure 9: *Experiment 4.* Comparison of scenario types on children's word learning performance.

To investigate whether parents' self-reported phone use can be related to children's word learning in the interrupted and uninterrupted conditions, following Reed et al. (2017), parents were categorised as either light or heavy cell phone users based on a median split. An ANOVA on PLT difference with Interruption as a within factor (interrupted or uninterrupted, Order of Interruption as a between variable (1=interrupted for first word,

2=interrupted for second word), Scenario as a between variable (1 versus 2) and a between factor (light or heavy cell phone use) showed no main effects of Interruption, Order, Scenario, and phone use, and no interaction between those factors, $F(1,23) = 0.05$, $p = .82$, $\eta^2_p = .002$.

Plan of the follow-up analyses with a priori and a posteriori hypotheses

The phone text interruption did not have any effect on word learning, which contradicts our expectations based on Reed et al.'s (2017) findings. Therefore, we investigated first whether any of the following variables linked to parental behaviour were associated with children's word learning:

- the time parents spent on the phone to fill the questionnaire. *A priori* hypothesis: The more time parents spent filling the questionnaire, the less well children learned the interrupted word.

- the total number of word repetitions (from the parents). *A priori* hypothesis: The more times parents repeated the word, the better the children's performance at word learning.

- whether or not parents repeated the word while filling the questionnaire (during the interruption). *A posteriori* hypothesis: children whose parents repeated the word during the questionnaire would perform better on word learning than those whose parents did not.

- whether or not parents repeated the word in isolation. *A posteriori hypothesis:* children whose parents repeated the word in isolation learned the word better than those whose parents did not.

Second, to investigate why the different scenarios led to different learning, we looked at two parental behaviours: did parents in scenario 1 spend less time on the questionnaire than those in scenario 2? Did they repeat the target words the same amount in both scenarios, whether it was during the entire task or before/during/after interruptions? Our *a priori* hypothesis was that parents in scenario 1 would spend less time on the questionnaire and would repeat more the target words than parents in scenario 2.

Finally, we turned to children's behaviour during the tasks to examine whether their level of engagement and their reaction (social bid and/or room exploration) to the text interruption could explain their word learning performance and differ across the two scenarii. Although Reed et al. (2017) only assessed whether children walked away (=explored the room) when their parents were on the phone to examine whether children's reaction to the phone affected their word learning, we intended to provide a more exhaustive overview of their behaviour and examined their engagement with the task, their social bids and their exploration of the room before, during, and after the phone interruption. Our *a priori* hypotheses were:

- children's behaviour would be less engaged with the task during the questionnaire/interruption.
- children whose parents had scenario 1 would be overall more engaged with the task than those whose parents had scenario 2.
- children's word learning would not be affected by their behaviour during the task (see Reed et al., 2017).

Parents' behaviour and word learning

Although the total duration of each phase was monitored by the experimenter, as well as the moment the questionnaire was sent (or not, depending on the condition), parents could spend as little or as much time as they wanted on the questionnaire (with a maximum of 60 sec, although we excluded the 7 parents who spent the entire 60s doing so). We analysed first whether the questionnaire duration and/or the total number of repetitions had an impact on the children's word learning. No correlation was found between the duration of the questionnaire and the PLT of the interrupted word, r = -.09, p = 594.

Additionally, there was no correlation between the PLT of the interrupted word and the total number of word repetitions (including the number of repetitions during questionnaire/interruption), r = -0.16, p = .358, and no correlation was found between the PLT of the non-interrupted word and the number of word repetitions, r = -.05, p = .794. Also, the total number of the interrupted word repetitions did not correlate with the questionnaire duration, r = 0.14, p = .427. The total number of word repetitions excluding the number of repetitions during interruption did not correlate with the PLT either.

Next, a t-test was run to examine whether repeating the interrupted word while filling the questionnaire (during interruption) affected the PLT of the interrupted word. Children whose parents did not repeat the word while completing the questionnaire learned the interrupted word better (N = 20, M = 0.16, SD = 0.21) than children whose parents did repeat the word during the questionnaire (N = 15, M = 0.002, SD = 0.23); $t(33)$ = -2.12, p = .043,

Cohen's *d* = 0.71. It should be noted however that the number of times parents repeated the word during the interruption was not correlated with the PLT of the interrupted word (*r* = -.20, *p* =.25). Plus, no correlations were found between the number of word repetitions during the questionnaire completion and the total number of the interrupted word repetitions during the entire teaching task (*r* = .08, *p* = .661) nor with the total number of non-interrupted word repetitions (*r* = .22, *p* = .208). The fact that the number of word repetitions during the phone interruption did not correlate with the total number of word repetitions during the entire teaching task suggests that parents behaved differently when they were on the phone. During the questionnaire, parents purposely decided whether to repeat the word or not. This decision-making supports the notion that the word repetitions made during the questionnaire had a specific impact on the PLT for the interrupted word, and this impact was not due to chance or random behaviour. In summary, the way parents behaved regarding word repetitions during the phone interruption was distinct from their behaviour during the rest of the teaching task. Their purposeful choice regarding word repetitions during the questionnaire indicate that these repetitions had a deliberate effect on the PLT of the interrupted word, which was not coincidental.

As some parents did repeat the target words in isolation (e.g., "blicket"), while others inserted them in sentences (e.g., "use the blicket", "roll the blicket", "can you say blicket?"), we examined whether parents inserting the word in a sentence or not affected children's word learning. We created 3 groups: Group 1 (*N*=20) includes parents who repeated the word in isolation at least once in both the interrupted and non-interrupted conditions;

Group 2 (*N*=10) includes parents who never repeated the words in isolation in neither of the two conditions; and Group 3 (*N*=5) includes the parents who repeated the word in isolation at least once in one condition but not in the other condition. Group 3 was then excluded for the following analysis as these parents were not consistent in the way they repeated the word in isolation across the two conditions. A t-test was run to search for differences between Groups 1 and 2 on the PLT of the target words. Children whose parents repeated the word in isolation at least once (N = 20, M = 0.14, SD = 0.03) did not outperform children whose parents never repeated the word in isolation (N = 10, M = 0.11, SD = 0.07); $t(28) = 0.35$, $p = 0.734$.

In summary, whether or not parents repeated the words in isolation, how long they spent on their phone filling in the questionnaire, and how many times they repeated the words, did not affect children's word learning. However, surprisingly, children whose parents did not repeat the word while completing the questionnaire learned the interrupted word better than children whose parents did repeat the word during the questionnaire. This will be explored further when comparing the two scenarii (see section Scenario 1 vs Scenario 2 below).

Following this, we explored why the scenario type influenced children's word learning and investigated further the parents' behaviour.

Scenario 1 vs Scenario 2

To investigate why the different scenarios led to different learning, we looked at two parental behaviours: did parents in scenario 1 spend less time on the questionnaire than

those in scenario 2? Did they repeat the target words the same amount in both scenarios, whether it was during the entire task or before/during/after interruption?

Entire task

An independent t-test was run to look for scenario differences on the questionnaire duration. Results revealed that there were no significant differences on the questionnaire duration (in seconds) between the scenario 1 (*M* = 33.86, *SD* = 10.58) and the scenario 2 (*M* = 35.21, *SD* = 12.15); *t*(33)= -.34, *p* =0.74, *d* = 0.12.

During the entire task, the total number of word repetitions (including repetitions during the questionnaire) was analysed as a function of the scenario and interruption. An ANOVA was performed on the number of word repetitions with Interruption (interrupted or non-interrupted) as a within factor, and Scenario (Scenario 1 versus Scenario 2) as a between factor. There was a main effect of Interruption $F(1,33) = 6.05$, $p = .019$, $\eta^2_p = .16$. However, we found no main effect of Scenario, nor a significant interaction between Interruption and Scenario ($F(1,33) = 0.19$, $p = .665$, $\eta^2_p = .01$). During the entire task, the main effect of Interruption was due to parents significantly repeating more the interrupted word (including the number of the interrupted word repetitions while filling the questionnaire) (*M*= 15.97, *SD* = 1.12) than the non-interrupted word (*M* = 13.51, *SD* = 1.00); *t*(34) = 2.45, *p* = .020, *d* = 2.32.

Then, a similar ANOVA was conducted on the total number of repetitions when excluding the repetitions during the questionnaire, as a function of the scenario and interruption. No

main effects of Interruption or Scenario were found and there was no interaction between these variables. Thus, during the entire task, there were no differences on the total number of repetitions when the number of repetitions during the questionnaire was not included between the interrupted and the non-interrupted conditions.

These findings suggest that during the entire task, parents overall repeated more the interrupted word (only when the repetitions during the questionnaire completion were included in the total repetitions) as with the interruption period they would have extra time to repeat the interrupted word (overall 1min30s) compared to the non-interrupted word (overall 1min).

Before vs after interruption

Then, we compared the number of repetitions of the word during the first half (30 sec) of the learning task and during the last half of the task, as a function of the scenario and interruption. An ANOVA was conducted on the number of word repetitions with Period (first half or last half) and Interruption (interrupted or non-interrupted) as within factors, and Scenario (scenario 1 vs scenario 2) as a between factor. No main effect of Interruption was found, but there was a main effect of Period $F(1,33) = 16.50$, $p < .001$, $\eta^2_p = .33$ and an interaction between the Period factor and the Interruption factor $F(1,33) = 12.41$, $p = .001$, $\eta^2_p = .27$. However, there was no main effect of Scenario and in particular no interaction was found between Scenario and these variables $F(1,33) = 2.65$, $p = .113$, $\eta^2_p = .07$. In both scenarios, parents repeated more the target word during the first half than the last half (see **Figure 10** below).

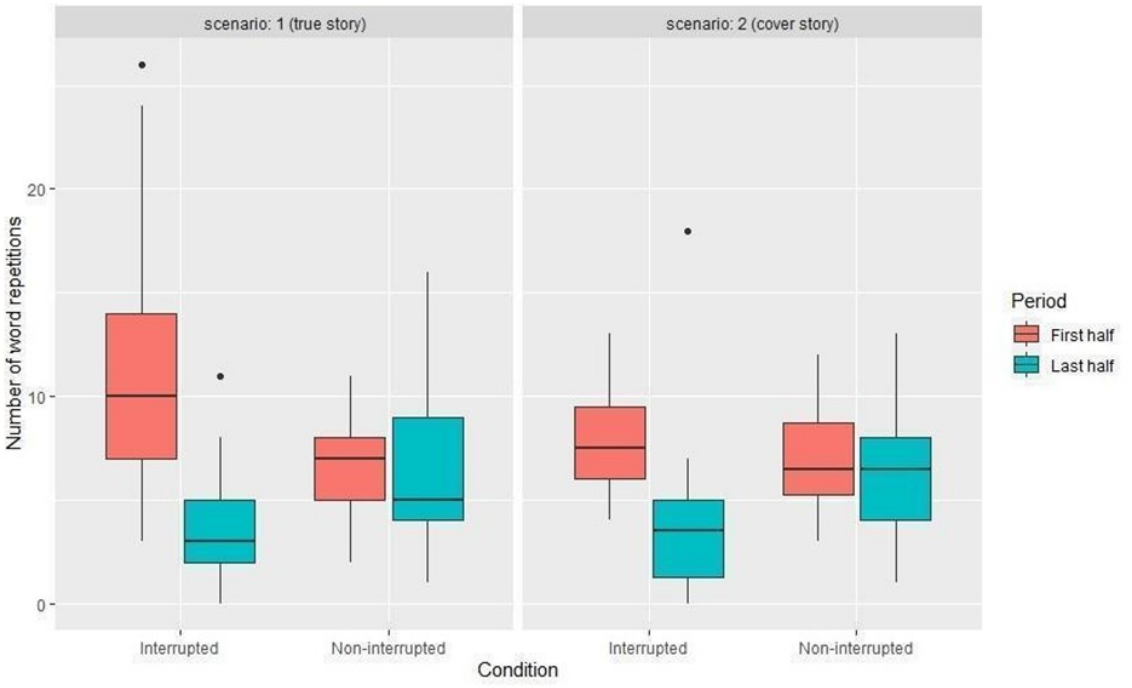


Figure 10: Experiment 4. Comparison of the number of repetitions between the first half and last half of the task between the scenarii.

Then, as the scenario type did not have an effect, a two-way ANOVA was performed on the number of word repetitions with Period (first half or last half) and Interruption (interrupted or non-interrupted) as within factors. There was no main effect of Interruption $F(1,34) = 0.001$, $p = .976$, $\eta^2_p < .001$. However, a main effect of Period $F(1,34) = 18.87$, $p < .001$, $\eta^2_p = .36$ and a significant interaction between the Interruption and Period factors were found $F(1,34) = 14.72$, $p = .001$, $\eta^2_p = .30$. Further analyses revealed that the main effect of Period was due to parents repeating on average more the word during the first half ($M = 8.52$, $SD = 3.45$) than during the last half of the task ($M = 5.30$, $SD = 2.90$); $t(34) = 4.34$, $p < .001$. Regarding the interaction, there were no differences on the non-interrupted word repetitions during the first half ($M = 6.83$, $SD = 2.70$) vs the last half of the task ($M = 6.69$,

SD = 4.06); *t*(34) = 0.24, *p* = 0.814, Cohen's *d* = 0.04. However, parents significantly repeated more the interrupted word during the first half (*M* = 9.57, *SD* = 5.36) than during the last half (*M* = 3.91, *SD* = 3.74); *t*(34)= 4.24, *p* <0.001, *d* = 1.22 (see **Figure 11**).

During the last half, it is likely that parents repeated less the interrupted word because they had less time to repeat it depending on the time they took to complete the questionnaire. In the last half of the task, parents had 26 seconds on average to repeat the interrupted word (range: 14–30s; *SD* = 9.40). Whereas in the first half of the task, they had a full 30 sec period to repeat the word.

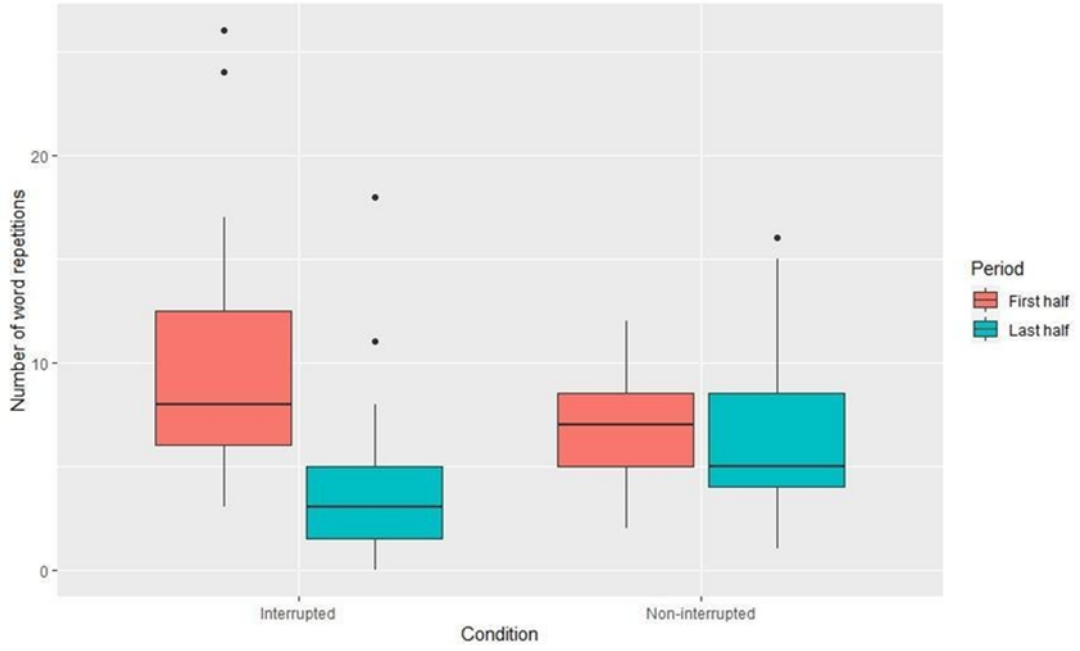


Figure 11 *Experiment 4.* Comparison of the number of repetitions of the interrupted word during the first half (30s) of the learning task and during the last half of the task.

During interruption

Next, a t-test was run to examine whether parents with scenario 1 repeated more the interrupted word during the questionnaire completion (interruption) than parents with scenario 2 (this number of interrupted word repetitions was not counted previously neither in the number of repetitions during the first half nor the last half of the task). Unexpectedly, parents who were given scenario 2 repeated more the word during the interruption (M = 4.43, SD = 3.39) than parents with scenario 1 (M = 1.19, SD =2.24); $t(33) = -3.09$, $p = .002$, d = 1.13) (see **Figure 12** below).

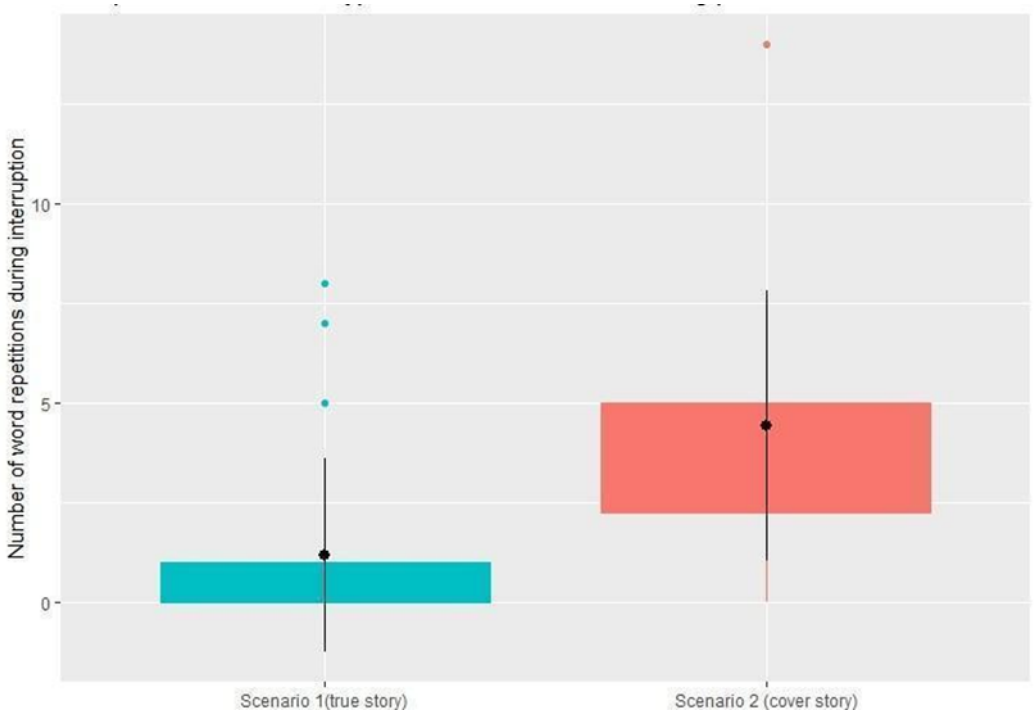


Figure 12: *Experiment 4.* Comparison of the number of word repetitions of the interrupted word during the questionnaire between the two types of scenarii

Furthermore, our results revealed that 71.4% of parents with scenario 2 continued repeating the interrupted target word while filling the questionnaire whereas only 23.8% of

parents with scenario 1 kept repeating the word. This is consistent with our previous results showing that children whose parents did not repeat the word while completing the questionnaire learned the interrupted word better than children whose parents did repeat the word during the questionnaire. These findings suggest that children of Scenario 1 (true story) might have performed better on the interrupted word learning because their parents did not repeat the word during the interruption. It might be possible that continuously repeating the target word while looking at the phone questionnaire and not actively focusing on the child and/or the toy could disrupt the child's ability to learn words effectively. For instance, the child might be confused as to why the parent is suddenly saying *blicke*t while looking at and holding a phone, whereas previously the parent has been saying *blicket* while looking at and holding the rolling pin.

Children's Behavioural Responses to Interruptions.

In the last part, we examined whether children's engagement (first part), and/or children making social bids and/or exploring the room (second part) were related to their word learning performance and differed across scenarii.

Children's engagement

Similar to Reed et al. (2017), we investigated whether children's reaction to the text interruption affected their word learning performance. Additionally, we chose to examine children's engagement with the word learning task. Two skilled research assistants independently scored children's behavioural responses on the word learning task during 3

phases (before, during and after the phone text distraction) on the following behaviours: whether they were engaged with the toy (includes intently looking at it and/or playing with it = holding it or touching it while looking at it), whether they were engaged with the parent (includes interacting with the parent by looking and/or vocalising sounds and/or speaking to him/her), and whether they showed a positive affect during the task (displaying facial expressions of joy such as smiles or vocalisations with a positive tone). A score of 1 was assigned to each behaviour exhibited. The children's engagement level with the task was the sum of those 3 scores for each phase (maximum score of 3). Inter-rater reliability agreement between coders was 85.92% and according to a Cohen's Kappa calculation, was strongly reliable, κ = 0.85. **Table 10** provides the descriptive data for the children's engagement with the task.

Table 10: *Experiment 4.* Descriptive data of the children's total level of engagement during the word learning task (*N* =35)

	M	*SD*
Engagement before the phone text	2.77	0.49
Engagement during the phone text	2.46	0.78
Engagement after the phone text	2.77	0.60
Overall engagement during the task	2.67	0.53

Note.

The engagement level is ranged from 1 to 3 (lowly engaged to highly engaged).

The overall engagement level is the mean of the three engagement levels (before, during, after phases).

First, we analysed whether children's engagement level during the task differed between Scenario 1 and Scenario 2. An ANOVA was conducted on the children's engagement scores as the dependent variable with the Phase during the task (pre, during, and post) as a within factor and Scenario as a between variable (scenario 1 vs scenario 2). There was a main effect of Phase $F(1,33) = 7.19$, $p = .001$, $\eta^2_p = .18$. However, there was no main effect of Scenario and in particular no interaction was found between Scenario and the Phase variable $F(1,33) = 1.56$, $p = .217$, $\eta^2_p = .05$. In both scenarii, children's level of engagement was similar.

Next, as the scenario type did not have an effect, an ANOVA was performed on the children's engagement scores as the dependent variable and the phase during the task (pre, during, and post) as a within factor. The effect of phase was significant $F(1,34) = 6.60$, $p = .002$, $\eta^2_p = .17$, showing that engagement varied across the three phases. Paired t-tests were run to determine which phase differed from which one, with results adjusted by Bonferroni correction (Abdi, 2007; significance value divided by 3 and adjusted to 0.017). Children were significantly more engaged before the phone text phase ($M = 2.77$, $SD = 0.49$) than during the phone text phase ($M = 2.46$, $SD = 0.78$); $t(34) = 3.19$, $p = 0.003$, $d = 0.48$. Moreover, children were significantly more engaged after the phone text phase ($M = 2.77$, $SD = 0.60$) then during the phone text phase, $t(34) = -2.75$, $p = 0.009$, $d = 0.45$. However, no differences were found between the engagement level before the phone text and after the phone text phases, $t(34) = .00$, $p = 1.00$, $d = 0$. Those results suggest that children were engaged with the task when parents were not on their phone to complete the

questionnaire. They recovered quickly from the phone text and re-engaged with their parents as much as before the phone text (see **Table 10** above).

Then, correlations were tested between the child's engagement level before the parent was on the phone (BPP), the engagement level when the parent was on the phone (PPP) and the engagement level after the parent was on the phone (APP). A significant positive strong correlation was found between BPP and PPP, $r = 0.65$, $p < .000$. Also, a strong positive correlation was found between BPP and APP, $r = 0.56$, $p < .000$. The less children were engaged with the task during BPP, the less they were engaged during PPP and APP, so throughout the duration of the task.

Finally, a correlation was conducted to examine whether the children's level of engagement with the task had an impact on their word learning. There were no significant correlations between the learning of the target words and the level of engagement during each phase and no significant correlation between the overall engagement of the child and the word learning scores, $r = 0.94$, $p = .590$. This suggests that the child's engagement with the task, at least as measured by our observations, did not affect their word learning. Additionally, no significant correlation was found between the duration of the questionnaire and the level of engagement with the task. The duration of the questionnaire (completed by the parent) did not impact the child's engagement.

Children's social bids and room exploration

Following this, we examined children's social bid and room exploration. Two skilled research assistants independently scored those children's behaviours. They scored whether the child made a social bid (attempting to get their parent's attention, physically or vocally, in a negative, positive or neutral way) during each of the three phases BPP, PPP and APP. A score of 0 was assigned for a child exhibiting no social bid and a score of 1 for a child making a social bid. Similarly, they scored whether the child explored the room during each of the three phases. A score of 0 was given when the child did not explore the room and a score of 1 was given when the child explored the room. Inter-rater reliability agreement between coders was 82.38% and according to a Cohen's Kappa calculation, was substantially reliable, $\kappa = 0.78$. Out of the total 17.62 % disagreement, 10.22% was specific to whether a child made a social bid.

Correlations were made to examine whether the child's exploration of the room and whether the child making a social bid can be related to their word learning performance.

Children's social bid

Out of the 35 children, 42.90% made a social bid to their parent during BPP, 77.10% during PPP and 60% during APP. There was an increase of 34.20% of social bid when the parents were on the phone, that is, when they were reducing their engagement with their child, and children were seeking more of their parents' attention after the phone text. Nevertheless, no association was found between social bid and the word learning scores. Those results suggest that the child's attention to the parent did not have any effects on word learning.

Then, we ran a repeated measure ANOVA on children's social bid with Phase as a within variable (during BPP, PPP, or APP), and Scenario as a between variable (1 versus 2). A main effect of Phase was found, $F(1,33) = 4.69$, $p = .013$, $\eta^2_p = .12$, showing that social bid varied across the three phases. Nevertheless, no main effect of the Scenario factor was found and in particular, there was no significant interaction between Phase and Scenario type $F(1,33) = 0.34$, $p = .714$, $\eta^2_p = .01$.

Next, as the scenario type did not have an effect, an ANOVA was performed on children's social bid with Phase as a within variable (during BPP, APP, or APP). A main effect of Phase was found, $F(1,34) = 4.80$, $p = .011$, $\eta^2_p = .12$. Then, paired t-tests were run to determine which phase differed from which one, with results adjusted by Bonferroni correction (Abdi, 2007; significance value divided by 3 and adjusted to 0.017). There were no differences on children's social bid between BPP and APP or between PPP and APP. However, children significantly made more social bid during PPP (M = 0.77, SD = 0.43) than BPP ($M = 0.43$, $SD = 0.50$); $t(34) = -3.43$, $p = .002$, $d = 0.73$, which is consistent with our previous results showing an increase of social bid during PPP due to the parents being on the phone, and thus reducing their engagement with their child.

Children's room exploration

During the task, it was observed that 60% of children explored the room during BPP and 62.9% during both PPP and APP. Moreover, no relation was found between the child's room exploration and the word learning scores which showed that whether the children explored the room during the task did not affect their word learning and that parents being on the

phone did not make them explore the room more. This was corroborated by a repeated measure ANOVA on children's exploration of the room with Phase as a within variable (BPP, PPP, or APP), and Scenario as a between variable (1 versus 2), showing no main effect of Phase nor Scenario. No interaction was found between the phase and scenario type factors $F(1,33) = 0.05$, $p = .833$, $\eta^2_p = .001$.

In summary, neither parents' behaviour in terms of number of word repetition, nor children's behaviour including children's level of engagement, social bids, or room exploration, did impact on children's word learning performance.

3.2.4 Discussion

Our study investigated whether parental phone texting can impact word learning in young children aged 17 to 19 months. It was expected that children would learn the non-interrupted word better than the word interrupted by a phone text. Also, we hypothesised that children whose parents knew the true purpose of the study (Scenario 1) would outperform those whose parents were given the cover story (Scenario 2). Our findings partially confirm our hypotheses: although children learned the interrupted and uninterrupted words equally well, the children with Scenario 1 outperformed children with Scenario 2 on word learning.

Word learning

Children successfully learned the target words in the two conditions, interrupted and non-interrupted. However, they did not succeed in the mutual exclusivity trials, that is, trials in which they were presented with a new object and a new label, together with the picture of the learned object. In this condition, they were expected to look longer at the new object upon hearing the new label, if they had inferred that this new label could not apply to the learned object given that it had a name (e.g., blicket). This is consistent with Reed et al. (2017), demonstrating that the word learning was probably not entirely robust in this situation. The use of the mutual exclusivity principle is typically seen as early as 17 months (Halberda, 2003) and our sample of 17 to 19 months old infants would be expected to demonstrate its use. However, as pointed out by Reed et al. (2017), considering that prior research has described toddlers as being conservative word extenders (e.g., Childers, 2011; Childers et al., 2012), the lack of children's application of the mutual exclusivity principle should not diminish toddlers' successful extension of the novel words at test in our study.

Phone text interruption

In contrast with Reed et al. (2017) who found that children did not show evidence of learning the interrupted word verbs while succeeding in the uninterrupted condition, our results showed that the interruption by the phone use did not impact children's word learning and that children learned equally well the non-interrupted and the interrupted words. Those contrasting findings could be due to several reasons. Their study involved a phone call

interruption where the parent talked to another interlocutor and not a phone text where the parent did not chat with someone else. Indeed, Reed et al. (2017) reported that when the mother was speaking on the phone to an invisible partner, she was no longer discussing the same topic that had been addressed in the conversation with her child which might have disrupted word learning as semantic contingency is important for learning (Kaiser & Roberts, 2013). Nevertheless, it is puzzling to observe in our study that children whose parents did not keep repeating the word during the phone interruption/questionnaire and thus did not maintain semantic contingent responses, learned the word better than children whose parents kept repeating the word. Furthermore, it is interesting that three quarters of parents with Scenario 1 (who knew the true aim of the study) did not maintain semantic contingency during the interruption whereas only one quarter of the parents with Scenario 2 (who didn't know the true aim) did. It is possible that parental knowledge of the purpose of the study affected their teaching attitudes towards their children including on their application of semantic contingency. Indeed, research has shown that context might influence mothers' contingency and teaching attitudes. For instance, Conti-Ramsden and Friel-Patti (1983) observed that mothers produced a higher proportion of contingent responses during free-play compared to semi-structured teaching sessions. Parents' behaviours and word learning in our current study will be discussed further below.

On the other hand, our results are consistent with Konrad et al. (2021) who did not find an impact of interruption by phone on children's (aged 19 months) learning. Similar to us, they

used a questionnaire completion as a phone text distraction, but their study included action imitation learning and not verbal word learning. Furthermore, similarly to Reed et al. (2017), and Konrad et al. (2021), the amount of screen use reported by the parents was not related to children's word learning in our study. Konrad et al. (2021) did not observe the amount of screen time to be associated with learning but found that individual differences in parental reports regarding their phone usage for various purposes was linked to learning performance. Interestingly, children's learning performance was lower when their parents reported a need to stay connected for work, a sense of security when their smartphone was close by, or occasional use of their phone to escape from their children. On the other hand, when parents reported that they found it easier to multitask, children's performance was higher. Their findings suggest that higher levels of reported dependence on smartphones by mothers were associated with overall lower imitation performance. According to a literature review conducted by Morris, Filippetti, and Rigato (2022), understanding the reasons why parents use their smartphones in the presence of their children is crucial in order to untangle the complexities associated with this behaviour because this may moderate the effect of technoference on children's language development. Technoference may not be a direct causal factor in language development but rather a moderating factor. For example, they quoted Torres, Radesky, Levitt, and McDaniel (2021) who found that parents who used their smartphones as an escape reported higher levels of parenting stress compared to those who did not engage in such behaviour. This suggests that difficulties in establishing a nurturing relationship may also contribute to parental smartphone use during family mealtimes, as highlighted by Radesky et al. (2018). It would be interesting to explore with

additional research, parents' attitudes towards their phones which might have indirect effects on their children's learning.

Our results also revealed that the number of word repetitions by the parent did not affect children's word learning, which is in line with Reed et al. (2017). Interestingly, parents significantly repeated the interrupted word more during the first half of the teaching phase than during the last half. Besides eventually having less time in the last half of the task to repeat the word, it might be possible that parents were still distracted by the preceding phone text and repeated the word less. Those findings could potentially imply that parents were then less engaged with the word learning task afterwards which supports previous findings that showed that maternal use of mobile devices was associated with fewer verbal interactions during structured and unstructured parent-child interactions (Kirkorian et al., 2009; Radesky et al., 2014).

Scenario type

We found that children whose parents knew the true purpose of the study (Scenario 1) learned the words better than children whose parents had the cover story (Scenario 2). What did parents do differently when given the two scenarios? Did they repeat the words more often in Scenario 1, and/or spend less time on the questionnaire? Actually, there were no differences on the number of word repetitions before and after the interruption, or during the overall teaching task, between the two groups of parents. Furthermore, there were no differences on the questionnaire duration between the two types of scenarios.

On the other hand, three quarters of the parents with Scenario 2 continued to repeat the target word during the interruption (while filling the questionnaire) whereas only a quarter of parents with Scenario 1 chose to keep repeating the target word. This means that most of the parents with Scenario 1 chose to only focus on the phone while completing the questionnaire. This is somewhat surprising as we would have expected parents of Scenario 1 to compensate for the interruption by continuing to repeat the target word during the interruption. This behaviour during the interruption had an impact on word learning: consistently, across the two scenarii, children whose parents did not repeat the word while completing the questionnaire learned the interrupted word better than children whose parents did repeat the word during the questionnaire. It might be possible that continuously repeating the target word while looking at the phone questionnaire and not actively focusing on the child and/or the toy could disrupt the child's ability to learn words effectively. For instance, the child might be confused as to why the parent is suddenly saying *blicke*t while looking at and holding a phone, whereas previously the parent has been saying *blicket* while looking at and holding the rolling pin. Parents often introduce basic terms through ostensive definition, which is pointing to and labelling an object, a behaviour that can facilitate children's word learning (Akechi et al., 2013; McGregor, 2008). Previous studies have shown that children's novel object learning was more successful when the experimenter followed children's pointing than when they ignored the object by redirecting children's attention (Begus, Gliga, & Southgate, 2014; Tomasello & Farrar, 1986). So, parents who did not repeat the word while filling the questionnaire might have employed an efficient strategy by only saying the target word when pointing to/touching the correct toy

object and not their phone. Thus, as pointed out by Konrad et al. (2021), examining parental responses to technoference and the role of parents' attitudes towards smartphones could serve as a useful proxy for studying patterns of technoference.

Moreover, as the total number of the word repetitions did not affect children's word learning performance, the results might imply that the quality of speech and the teaching style used by the parents to their child could be the influential factor, more than the quantity of speech – although we did not find evidence that repeating the word in isolation helped word learning. Previous research has shown that parents adjust their strategy to introducing new words to suit their child's approach to word learning (Adamson, Bakeman, & Brandon, 2015). The way parents teach novel words to young children plays a role in noun acquisition such as the use of ostensive definition, making reference to familiar terms to introduce a new word (Callanan, 1985), attempting to draw attention by linking words and objects together (Adamson et al., 2015).

Children's behaviour

In line with Konrad et al. (2021) and Reed et al. (2017), children's behaviour during the task did not affect their word learning and was not associated with parents' reported smartphone usage. Additionally, the duration of the questionnaire (completed by the parent) did not impact their engagement. After the questionnaire, toddlers were engaged with their parents as much as before the phone text, which is consistent with Konrad et al. (2021), and implies that children reengaged in the task once their parents stopped being on

the phone. Also, in our study, children had similar levels of engagement with the task in both scenarii. Nevertheless, our results imply that the parent's attention affected the child's behaviour. Indeed, children were significantly less engaged with the task and made more social bids when their parents were on the phone compared to before and after the phone distraction. This is line with previous research that indicates that children may interpret their parents' facial expressions as blank or emotionless when they are checking their phones, and this lack of expression, commonly referred to as a "still face," can evoke negative responses or aversive reactions from children (Adamson & Frick, 2003; Myruski et al., 2018). However, it is plausible that our findings, which indicate no negative effects of texting or still face on immediate word learning, align with our results that children were as much as reengaged with the task after the phone text interruption. Nevertheless, it might be possible for a longer term retention of learning to be hindered (Konrad et al., 2021).

Limitations and future research

The study has a few limitations. First, we found that the number of word repetitions and children's behaviour did not impact word learning but children whose parents did not repeat the word during the interruption learned the interrupted word better. These findings might imply that parental teaching and speaking styles could have been the influential factor in children's word learning. However, we cannot be certain as we did not investigate the different speaking styles factor besides whether parents inserted the words in sentences. Future studies investigating technoference effects on children's language should also assess

parental speaking style as it influences children's word learning (Adamson et al., 2015; Begus et al., 2014; Callanan, 1985; Tomasello & Farrar, 1986).

Second, we did not examine parents' behaviours (besides the word repetitions) nor parental responsiveness during the task such as eye contact, affect, and posture. Nevertheless, it might be possible that parents were less engaged with the word learning task after the text distraction (as they repeated the word less than before) but this did not affect children's word learning. Reed et al. (2017) only assessed maternal reactions of the phone interruption and found no effects on learning either. Additionally, Corkin et al. (2021) results showed no relation between joint attention and technoference, or between joint attention and children's vocabulary. Yet, they assessed joint attention during an interaction without technoference, and joint attention may mediate the effects of technoference on children's language (Morris et al., 2021). Indeed, Davidovitch et al. (2018) showed that technoference can reduce joint attention during parent-child interactions and thus can affect language development. Additional studies are needed to investigate whether joint attention can mediate the effects of technoference on language.

Third, we did not assess parental attitudes towards their phones and previous studies suggested that the reasons why parents use their smartphones in the presence of their children (e.g. a need to stay connected for work, a sense of security when their smartphone was close by, or occasional use of their phone to escape from their children) can mediate the relation between children's language and screen use effects (Konrad et al., 2021; Morris

et al., 2022). However, our findings might also point to a potential impact of parental attitudes towards their phones in word learning situations. Indeed, children whose parents were made aware of the potential phone distraction (Scenario 1) learned better the words than those whose parents were given a cover story (Scenario 2). It should be noted that parents were aware that the experimenter did not observe them in live/online while they were doing the teaching task with their children. Thus, the effect of parental attitudes on phone use and their role in word learning situations could be further explored. In addition, as pointed out by Konrad et al. (2021), further research is needed to investigate whether different forms of learning such as word learning or non-verbal action imitation are more impacted by different types of phone use such as phone calls or silent texting. Also, future studies could increase children's memory retrieval demands (from word or action learning) by introducing a time delay between the teaching and testing phase (Konrad et al., 2021).

Finally, we manipulated parental phone use in a lab situation which does not reflect realistic parental phone use habits (explicit word learning tasks are not typical of parent-child interactions). We did not find an immediate effect of phone use on children's (aged 17-19 months) word learning but that does not mean that parents' screen use cannot have a long-term effect on children's language skills. Whether parental screen use interferes with real-life opportunities for language development, and in children younger than 2.5 years (given the results by Mustonen et al., 2022), is an open question which we will explore further with Studies 1 and 2.

3.3 Studies 1 and 2 Introduction

Experiment 4 did not show an impact of parental phone text use on word learning, which contrasts with Reed et al. (2017) which revealed a negative effect of parental phone call use on children's word learning. However, these two studies investigated the effects of parental screen use on children's language learning skills in an artificial lab environment: the explicit word learning interaction is probably not a situation that occurs in most children's real-life experience. In fact, we showed that the instructions provided to parents had an impact on word learning, more than the text interruption itself, which suggests that what happens in a lab situation is not necessarily a fair reflection of real-life situations. Thus, we wondered whether parental screen time interferes with real-life word learning opportunities for children's language development.

Mustonen et al. (2022) found negative parental screen time effects on children's vocabulary. Finnish mothers of 164 children (aged between 2.5 and 4.1 years) filled a one-time questionnaire to report their children's screen time, as well as their own. Children's language skills were assessed using validated tests (e.g., Finnish Phonology test; Kunnari, et al., 2012, and the Finnish version of the MacArthur Communicative Development Inventories III; Stolt, 2023). The findings revealed a negative association between mothers' screen time and their children's vocabulary skills, but not phonological skills. The following studies expand on this study in several ways. First, we question whether this negative association could also be present in children younger than 2.5 years (given the results by Mustonen et al., 2022). Second, Mustonen et al. (2022) did not ask specifically the mothers

to estimate their screen time when their child is around, which would be a more direct test of the links between parental screen time and child language development.

We examined the relationship between real-life parental screen use, parental education and children's language development. Two studies were conducted to test whether toddlers' language development is correlated with parental use of their mobile phones. In this thesis, they are named Studies 1 and 2 as we investigated the effect of the screen time variable as it occurs naturally in real life without manipulating it, whereas in the previous experiments, the stimuli and factors such as technoference were manipulated in a controlled lab setting. The first study is an exploratory online survey where we originally collected data about children's word knowledge through parental questionnaires during a Babylab visit (for studies unrelated to this one), and later asked them to complete a questionnaire about their screen time habits. The second study included a more objective measure of language development by administering a standardised language test to children in the lab, and a more objective daily screen time recording. Our main hypothesis was that the more time parents spend on their cell phone, the lower the toddler's language development, once accounting for effects due to parental education. It must be noted that all data reported here were collected before we read about the study by Mustonen et al. (2022), who adopted a similar design with older children, and we will address in detail their findings as compared to ours in the General Discussion of Studies 1 and 2.

3.4 Study 1: Exploratory Study on parental screen time and children's vocabulary

3.4.1 Method

Participants

The two studies reported here were approved by the University of Plymouth Faculty of Health Ethics Committee. A total of 115 parents (96.5% were mothers) and their monolingual children (53 boys and 62 girls) aged 8 to 29 months (*M* = 16 months 11 days, *SD* = 5 months 12 days) were recruited from the Plymouth Babylab database. The two parents' highest educational levels were retrieved on a scale of 1 to 6 (low to high: from primary school to postgraduate degree), leading to an average parental educational level at *M* = 4.77, *SD* = 1.14.

Procedure

After reading and signing a consent form, parents were invited to complete a questionnaire about their screen time habits on all types of screens (phone, TV, computer, tablets, video games) (Table 1). These parents had previously come to the Babylab for a language study, and on that occasion, completed the Oxford Communicative Development Inventories (Oxford CDI) (Hamilton et al., 2000). The Oxford CDI is a list of words that are typical in children's vocabularies. Parents were asked to tick whether their child could understand and/or say each word. When the current research took place, parents were asked to take a survey regarding demographic background and behaviour on social media (most questions pertained to parental use of Facebook: Briazu, Floccia, & Hanoch, 2021). They also provided

estimates of their screen time (**Table 11**). On average, 6 months (*M* = 194 days, *SD* = 209 days) elapsed between the moment they filled the CDI and the survey.

Table 11: *Study 1.* Questionnaire to parents in Study 1

Questions administered to parents	Response options
"Let's talk about your time on screen when it is not for work. In a typical day of the week, overall, how many hours do you spend on a screen for leisure purposes? That can be on your phone but also on any other type of screen (tablet, computer, TV, playstation…)?"	1) Less than an hour; 2) 1 hour to 2 hours; 3) 3 to 4 hours; 4) 5 to 6 hours; 5) 7 hours or more
"In a typical day of the week, and only when your youngest child is around, how many hours do you spend on a screen for leisure purposes?"	1) 0; 2) Less than an hour; 3) 1 hour to 2 hours; 4) 3 to 4 hours; 5) 5 to 6 hours; 6) 7 hours or more
"In a typical weekend day, overall, how many hours do you spend on a screen for leisure purposes?"	1) 0; 2) Less than an hour; 3) 1 hour to 2 hours; 4) 3 to 4 hours; 5) 5 to 6 hours; 6) 7 hours or more
"In a typical weekend day, and only when your youngest child is around, how many hours do you spend on a screen for leisure purposes?"	1) 0; 2) Less than an hour; 3) 1 hour to 2 hours; 4) 3 to 4 hours; 5) 5 to 6 hours; 6) 7 hours or more

"In a typical week, how many hours does your youngest child spend away from you, at nursery/with grandparents/ childminder/school?"	1) 1-10; 2) 11-20; 3) 21-30; 4) 31-40; 5) 41-50
"How many hours on average does your youngest child sleep per night (including nap time)?"	1) 8 hours or less; 2) 9 hours; 3) 10 hours; 4) 11 hours; 5)12 hours; 6) 13 hours; 7) 14 hours; 8) 15 hours or more

3.4.2 Results

Data description and plan of analyses

To estimate screen usage, we calculated two different variables for each parent, DailyScreen and DailyScreenChildProp. DailyScreen is the time the parent reported to spend on screen during a day when not at work. DailyScreenChildProp is the proportion of time the parent spent on screen while the child was around (parent's answer from "when your youngest child is around, how many hours do you spend on a screen", see **Table 11** above) out of the total time with the child awake and not away from parent (that is, take away sleeping time and away time). These measures were weighted 5/7 for a weekday and 2/7 for a weekend day.

For example, for a parent who reported spending 2 hours per weekday on the phone and 3 hours on a weekend day, DailyScreen was 2.29:

(1) DailyScreen = (5/7) *2 hrs weekday screen time + (2/7) *3 hrs weekend screen time

If this parent reported spending 1 hour on their phone on a weekday while their child is around, and 30 min during a weekend day, with a child who slept 12 hours a day and was away for 2 hours per weekday, DailyScreenChildProp would then be 0.08, applying the following formula:

(2) DailyScreenChildProp = (5/7) *1 hr weekday while child around/ (24 hrs – 12 hrs sleep – 2 hrs away from parent) + (2/7)*0.5 hrs weekend while child around /(24 hrs-12 hrs sleep)

The reason why we chose to use two different variables is that DailyScreen might comprise time when there is no possibility of interaction with the child, for example when the child is napping. In contrast, DailyScreenChildProp is meant to capture only the moments where parents can interact with their child.

Two CDI scores (CDI comprehension and CDI expressive vocabulary) were used as indicators of the child's language development (expressed as proportion of words known out of the total number of words).

Hierarchical regressions were run on the CDI scores to examine in Step 1 which core variables (age, gender, and parental education) could predict children's vocabulary, followed in Step 2 by screen time variables which were added to those regressions. No corrections for multiple comparisons were made as comprehension and expressive vocabulary scores

measure different aspects of language, and the two measures of screen time were designed to provide a different construct.

Following this, a moderation analysis was run to examine at what level(s) of the age moderator the screen time-language association became significant.

Parental education was used to evaluate SES as it has been considered more accurately reported compared to other measures, and stable over time (Duncan & Magnuson, 2003). Friend et al. (2022) emphasised the importance of parental education in language development, showing a positive association between vocabulary size and maternal college completion. Krogh et al. (2021) also found a negative relationship between maternal education and infant screen time throughout the first year, starting as early as 2 months old. Parental education was indexed through the highest educational level achieved by either parent (e.g., Mäkinen, Laaksonen, Lahelma, & Rahkonen, 2006).

Analyses

Table 12 provides the descriptive data for the children's age (*N* = 115), gender, parental education, language scores and screen time variables. Parents reported spending overall 2h 44min on their phone, and on average 1h 20min per day on their phone while their child was around.

Table 12: *Study 1.* Descriptive data of the sample in Study 1

Means and standard deviations of the children's age, gender (53 boys and 62 girls), language scores, parental education and the 2 variables of screen time, DailyScreen and DailyScreenChildProp. The last three lines report the raw data from which DailyScreenChildProp is derived. "Screentime around child" expressed in hours is a directly reported parental estimate.

	M	*SD*
Child age (days)	497.92	163.95
Boys' age	486.68	162.15
Girls' age	495.45	162.69
Parents' education	4.77	1.14
CDI comprehension vocab (proportion)	0.36	0.33
CDI expressive vocab (proportion)	0.16	0.23
DailyScreen (overall screen time raw hrs)	2.74	1.58
DailyScreenChildProp	0.15	0.14
Screentime around child (raw hours)	1.34	1.31
Total daily sleep time (hours)	11.89	1.80
Overall daily time parent spent with child (hours)	8.94	3.25

Two-step hierarchical regressions were run on the CDI comprehension and CDI expressive vocabulary. At step 1, a regression model on comprehension scores with age, parents' education and gender led to a significant model (R^2 = .66, F(3,111) = 71.61, p < .001) with only age as a significant contributor (β = 0.81, t = 14.32, p < .001). On the expressive vocabulary scores, the same regression led to a significant model (R^2 = .54, F(3,111) = 40.67, p < .001) with again only age as a significant contributor (β = 0.71, t = 10.63, p < .001).

At step 2, the screen time variables were added into the regression models to investigate the screen time effects on the CDI scores. On the comprehension scores, when DailyScreen was entered into the model with age, gender and parental education, there was no significant improvement in variance (*F change* (4, 110) = 0.99, p = .32) and no significant contribution of DailyScreen (p = .32) (**Table 13**). Results were similar with DailyScreenChildProp (p = .93). **Figures 13 (a)** and **(b)** illustrate the relation between parental screen time and the comprehension scores.

Table 13: *Study 1.* Hierarchical regression on CDI scores (with Enter method) in Study 1

Variable	**B**	***β***	***t***	***p***
Step 1				
Children's age	0.002	0.81	14.32	<.001
Children's gender	0.04	0.06	0.99	0.33
Parental education	0.008	0.03	0.5	0.62
Step 2				
Children's age	0.002	0.81	14.33	<.001
Children's gender	0.03	0.05	0.84	0.40
Parental education	0.005	0.02	0.31	0.76
DailyScreen	-0.01	-0.06	-1.00	0.32

Step 1: $R^2 = .66$; Step 2: $R^2 change = .003$; $R^2 = .66$

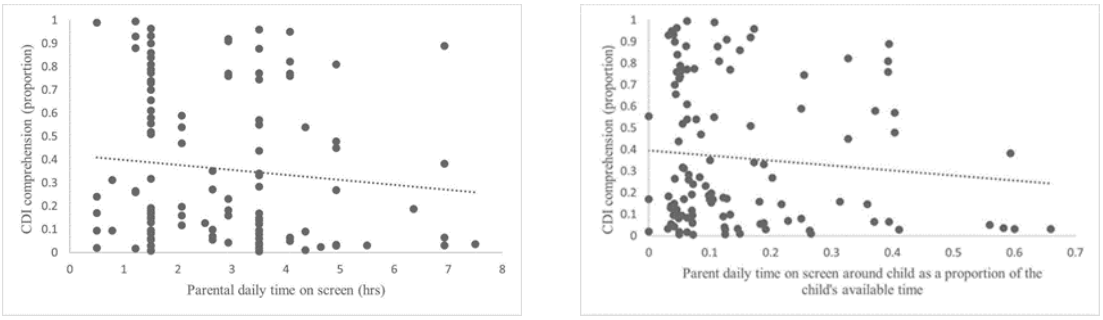


Figure 13: *Study 1.* Effect of Dailyscreen (a) and DailyScreenChildProp (b) on CDI comprehension scores

On the expressive vocabulary scores, when DailyScreen was entered into the model with age, parental education, and gender, there was no significant improvement in variance (*F change* (4, 110) = 2.36, *p* = .13) and no significant contribution of DailyScreen (*p* = .13).

Results were similar with DailyScreenChildProp (p = .77). **Figures 14 (a)** and **(b)** illustrate the correlations between parental screen time and children's production vocabulary.

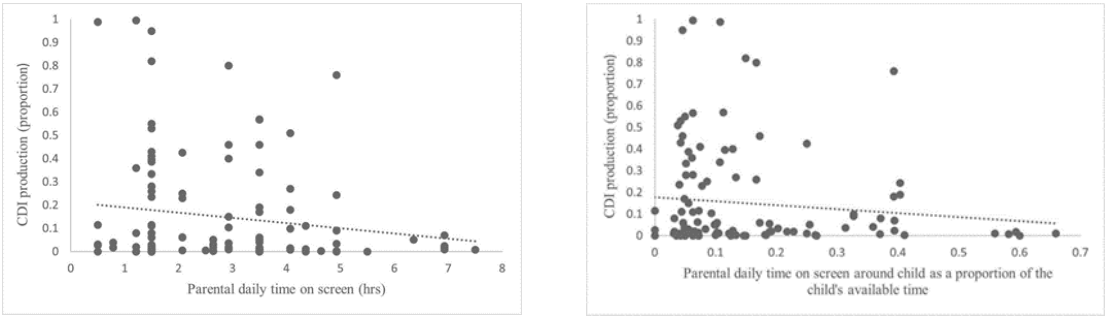


Figure 14: *Study 1.* Effect of Dailyscreen (a) and DailyScreenChildProp (b) on CDI production scores

A post-hoc power analysis was conducted using a linear regression on the reported comprehensive vocabulary scores with 115 participants. Only two predictors (age and DailyScreen) were used, as parental education and gender were found to have no effects on children's language. An R^2 value of 0.66 and a significance level of 0.05 were used. Using the "pwr" package in R, we calculated Cohen's f^2 effect size ($f^2 = R^2 / (1 - R^2)$) and estimated the power of our study to be 1 which shows a strong test power.

In the regression analyses on the whole sample, no parental screen time effects were found on children's language development. However, given that the acceleration in vocabulary is usually noticeable in the middle of the second year of life (e.g., Fenson et al., 2006), leading to a non-linear growth of the reported language scores with age, the association between parents' screen time and child's language development might depend on child's age.

Therefore, we ran a moderation analysis with the Johnson-Neyman technique (Johnson & Neyman, 1936), with children's age as a continuous variable. The moderation analysis did not show an association between DailyScreenchildprop and child language development, and children's age. However, the same analysis showed that DailyScreen had an effect on language development with children older than 16 months, in production (but not in comprehension). As shown in **Figure 15**, the Johnson-Neyman interval revealed that when the child's age is over 15.83 months, the conditional slope of DailyScreen on vocabulary production was statistically significant at the $p < .05$ level, with the range of observed values of age being [8.37, 29.67].

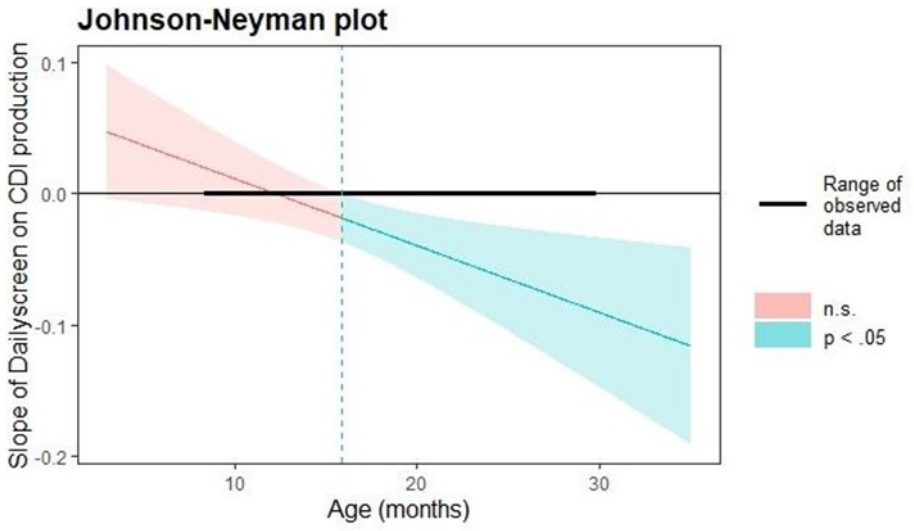


Figure 15: *Study 1.* Johnson-Neyman plot showing the association between parental screen time and child's expressive vocabulary as a function of age.

Based on these findings, we conducted further analyses on children who were older or younger than 16 months to explore the relationship between Dailyscreen and expressive vocabulary.

Children older than 16 months included 46 participants, 23 boys and 23 girls (*M* = 22.31 months, from 17.83 to 29.67). At step 1, a regression model on the CDI expressive vocabulary scores, forcing only age (as parental education and gender were found to have no effects on children's language) into the equation led to a significant model (R^2 = .10, *F*(1,44) = 5.0, *p* = .030).

When DailyScreen was entered into the model with age, it led to a significant model (*F change* (1, 43) = 4.52; R^2 = 0.19, *F*(2, 43) = 5.0, *p* = 0.011), explaining an additional 8.5% of the variance, with age (*p* = .035) and DailyScreen as significant contributors (*β* = -.29, *t* = -2.13, *p* = .039). **Figure 16** illustrates the negative relationship between parental screen time and children's production vocabulary, which corroborates the results from the Johnson-Neyman analysis.

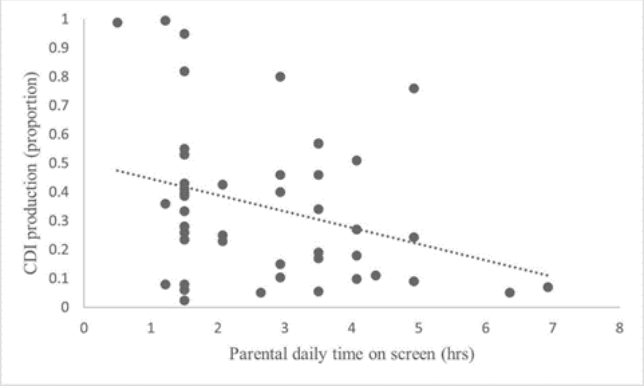


Figure 16*: Study 1.* Sub-group of children aged 16 months and above (N = 46). Relation between DailyScreen (parents' daily overall time on screen) and expressive vocabulary.

A similar analysis on the subgroup of children aged less than 16 months (*N* = 69) failed to show any contribution of DailyScreen on vocabulary scores, as hinted by the Johnson-Neyman plot.

3.4.3 Discussion

This first study aimed at examining a possible negative relationship between parental screen time and young children's vocabulary knowledge, possibly due to reduced exposure to language input and parent-child interaction opportunities. Using the parent-reported language questionnaire (CDI) as a measure of word knowledge and parental self-estimates of their own screen time, we did not find parental screen time to be a predictor of language development in children younger than 16 months. However, we found that in children older than 16 months, there was a trend for word production to be negatively associated with parental screen time, as measured by DailyScreen, that is, the amount of daily time spent on screen. This is consistent with Mustonen et al. (2022) who found a negative association between children's CDI expressive scores and mothers' overall daily screen time (children aged between 2.5 to 4 years). Additionally, in our study, the relationship between parental screen time and children's language development became only significant from 16 months of age which may indicate that parental cell phone use starts to have an impact on children's vocabulary during the second year of life, a stage of rapid vocabulary growth (Fenson et al., 2007). Possibly, a modulating effect of parental screen time before the age of 16 months might be hidden by a floor effect in vocabulary size. Indeed, toddlers approximately know 50 words when they are 18 months of age, after which they acquire between 20 and 21

months (Pine, 1995) and at 2 years old, they typically master between 200 and 500 words (Fernald et al., 2001).

Regarding the effect of the other variables, it is not uncommon to fail to find an effect of SES on the parent-reported language samples given that the typical Babylab visitors belong to the same middle-class population (Cattani et al., 2019; Gaber Abdel Wahab et al., 2021). However, we included this variable in our analyses, ensuring that any effect of parental screen time on language skills was not mediated by socio-economic factors.

This first study suffers from three main limitations: first, there was a high variation between the CDI completion date and the screen time survey's completion (6 months on average). It is possible that parental screen habits at time *t* are very different from time *t – 6 months*, introducing some uncertainty in the measurements. Second, the use of a parental report (CDI) to estimate children's language development might be biased in that study. Indeed, it is possible that parents who spend a high amount of time on their screens could under- or overestimate their child's language development. Finally, the measure of screen time used in this study might not be totally objective, as parents' recollection reports on screen time might not be very reliable (Radesky et al., 2020). To investigate this further, a second study was conducted with a more objective screen time recording and a more objective, face to face, language test, both performed at the same time. Furthermore, because Study 1 provided evidence that the effect of screen time on vocabulary skills was significant only with children aged 16 months and above, in this second study we focused on children aged 19 to 32 months as the British-English language assessment tool we chose to use can only be tested on children older than 19 months.

3.5 Study 2: Main Study on parental screen time and children's vocabulary, with a language test

In this study, we used a standardised face to face language assessment test, the WinG test (Cattani Krott, Floccia, & Dennis, 2019) to estimate toddlers' vocabulary knowledge. We also designed a 7-day survey for parents to report their phone usage on a daily basis, using data from their built-in screen usage app. By increasing the objectivity of both measures – language skills and parental screen time – we expected to replicate the finding from the first study that children's word knowledge would be negatively correlated with their parents' time on mobile devices.

3.5.1 Method

Participants

Ninety-five children were tested and the data from 15 children were excluded due to the non-full completion of the WinG test (4 online and 11 face-to-face participants). The final sample included 80 healthy monolinguals aged 19 to 32 months (42 boys and 38 girls) who were recruited from the Plymouth Babylab database (*M* = 22 months 29 days, ranging from 19 months to 32 months 4 days). Participating parents (79 mothers and 1 father) had educational levels at an average of *M* = 4.84, *SD* = 0.80. Thirty-two children did the experiment online due to Covid restrictions at the time of testing and 48 were invited to do it face-to-face in the Babylab.

Materials

For this study, an application (app) was built in which the parent reported their daily smartphone usage by answering a 6-item questionnaire every evening at the time of their choosing, typically after the child had gone to bed (see **Appendix B** and below for more details).

The Words in Game (WinG) test (Cattani et al., 2019) was used to test online and in-person young children's language development. For the video testing condition, it was administered online with Zoom using computer/laptop devices. It consists of 44 groups of 3 cards, 4 pre-tests and 40 experimental. Each set of 3 cards contains a comprehension card, a production card, and a distractor card. The comprehension and production tasks each contain 20 noun words and 20 predicate words. The 4 components each lead to their own standardised score and percentile for the number of correct answers that should be reached for each age and each gender. For this study, only the comprehension tasks for both nouns and predicates were administered with children aged 19 to 24 months, as recommended by the test guideline. For those who were 24 to 32 months old, following the WinG recommendations, the production task for nouns was additionally administered. The sessions were recorded for off-line coding, through Zoom for the video condition, and with a Canon video camera in the Babylab.

Procedure

After reading and signing the consent form, parents installed the app on their phone, and with the help of the Apple/Android built-in screen time function, completed the daily screen usage questionnaire. Once the screen time week recording was completed, they were invited for their child to complete the WinG test (Cattani et al., 2019), either online during the pandemic lockdown, or in the Babylab.

For the online Zoom session, the WinG cards were set standing against a cardboard box on a table, so that they would be visible on the child's screen. The researcher was sitting in a chair behind the table and a laptop was placed in front of the table, facing the picture cards. The child was in a room at home and sat in front of the electronic device using Zoom, with the parent sitting nearby. For the face-to-face condition, the WinG cards were set upon a table, with two chairs adjacent to each other on the table (for the child and the experimenter), with the parent sitting beside their child.

The WinG test was administered in line with the instructions from the WinG manual (Cattani et al., 2019). However, for the WinG test online with Zoom, children could not touch or take the cards to indicate their choice. Instead, they were asked to point to their computer's screen at the correct card. The session was video recorded, and the child's answers were scored according to their hand gesture and/or eye gaze going to the right, middle or left card. The WinG test started with 2 pre-tests of 3 cards each to give the child practice of what was required for the game. The 3 cards were presented in a random order in a line in front of the child, one comprehension, and 2 distractor cards. The children were first asked to point out or touch which one was named (comprehension card); once they pointed to one

of the cards, it did not matter if it was the right one. Then, the comprehension and distractor cards were taken away to move on to the next set of cards. This was repeated on the 20 experimental noun cards, the 2 pre-test sets for the predicate condition and all 20 experimental predicate cards. For children aged 24 months and older, a production subset on 20 experimental noun cards was done in parallel to the comprehension subset. After a child had responded to a comprehension request, the comprehension target picture and one of the distractor cards were removed from the table. The remaining distractor card was used for the production test. The administrator asked the child to name the picture (see diagram in **Figure 17**). Praise was always regularly provided, irrespective of the child's answers.

*Comprehension: "Where is the cat?"

*Production: "What is this?"

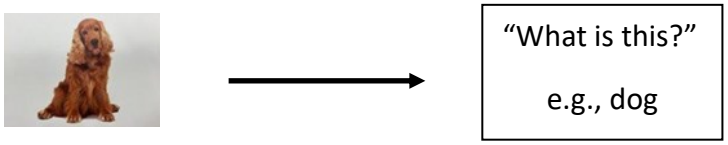


Figure 17: *Study 2.* Diagram of the structure of the WinG (Cattani et al., 2019).

The WinG test was performed after parents had provided data regarding screen time recording and lasted around 30 minutes. The data computed from the WinG were children's percentile scores for noun comprehension, predicate comprehension (for 80 children) and noun production (for 30 children who were 24 months and above) as calculated from the standardised scores in the WinG manual.

3.5.2 Results

Data description and plan of analyses

From the parents' screen time questionnaire, those daily measures were collected and averaged across the week:

- (A1) parent's total screen time as indicated by their phone summary data.
- (A2) time that the parent and the child spent together on the phone if applicable.
- (A3) time that the child was on the parent's phone on her/his own.
- (A4) time that the parent was on the phone during the child's nap.
- (A5) time that the parent spent on the phone while they were not with their child.

The time that the parent spent on the phone while the child was around, was calculated each day as follows and then averaged across the week:

(3) $D1 = A1 - (A2 + A3 + A4 + A5)$

This measure of parental screen time while the child is around – as opposed to the reported estimate in the first study – was thought to capture a more objective measure, because we did not ask parents directly for this estimate.

From these data, DailyScreen and DailyScreenChildProp were calculated as follows, to be as close as possible from the measures used in Study 1. The first measure, A1, is very similar to DailyScreen, that is, the time the parent reported spending on screen during a day. However, here it is a true capture of the screen time, whereas in Study 1 parents were asked to focus on leisure time. DailyScreenChildProp is the proportion of time the parent spends on screen while the child is around out of the total time with the child awake and not away from the parent (that is, take away sleeping, and away time). Because we did not retrieve the child's napping time nor night sleeping time information (due to experimental error), we used the mean of the sleep time (of children aged 19 months and older) from Study 1, since the variation per day (hours) was very low in that sample (N = 44, M = 12.09, SD = 1.44).

(4) DailyScreenChildProp = D1/ (24 hrs – 12.09 hrs - time away from parent)

On the whole sample, parents reported spending overall 2h 46min on their phone and spent on average 1h 19min per day on their phone while their child was around.

Table 14 provides the descriptive data for the children's age, gender, parental education, WinG scores, and screen time variables.

Table 14: *Study 2.* Descriptive data of the whole sample in Study 2

Means and standard deviations of the children's age, gender, parental education, WinG scores and the screen time variables.

	n	*M*	*SD*
Age (days)	80	698.00	91.82
Boys	42		
Girls	38		
Parents' education	80	4.84	0.80
WinG nouns comprehension (percentile)	80	34.94	22.70
WinG predicates comprehension (percentile)	80	38.06	21.25
WinG nouns production (percentile)	30	42.17	28.00
Overall Screentime raw Hrs (DailyScreen A1)	80	2.76	1.06
SharedTimeHrs (A2)	80	0.12	0.23
ChildOnParentPhoneHrs (A3)	80	0.04	0.09
ParentDuringNapHrs (A4)	80	0.73	0.64
ParentAloneHrs (A5)	80	1.20	1.11
ChildAwayFromParentHrs	80	2.42	1.81
Screentime around child Hrs (D1)	80	1.32	0.81
DailyScreenChildProp (prop)	80	0.14	0.08
Time parent spent with child (hours)	80	9.50	1.81

The main dataset was complete: there was no missing data on the WinG scores (but only 30 older children completed the production part of the WinG), or on the screen time measures.

Hierarchical regressions were first carried out to investigate at Step 1 which core factor(s) (education and mode of administration of the test) predicted the WinG language comprehension scores, followed by the screen time variables and age at Step 2. Age was included in Step 2 only because it was not expected to explain any variance in WinG scores at Step 1, given that it is already included in the calculation of standardised scores. However, at Step 2 it might explain variance related to screen time effect on WinG scores. Gender was not included in Step 1 for the same reason, and not in Step 2 because we had no reason to expect a child gender effect on screen time values. Similar analyses were run on the WinG production scores. The Bonferroni correction (Abdi, 2007) for multiple comparisons was applied for noun and predicate comprehension scores as they are both measures of comprehension. Following this, a moderation analysis was run to examine whether children's age moderates a screen time-language association.

WinG comprehension scores

A regression model on the WinG noun comprehension scores with parental education and the mode of the WinG administration (online or face-to-face as a dummy variable) led to a significant model ($R^2 = 0.19$, $F\ (2, 77) = 8.71$, $p < 0.001$) with the mode of the test as the only significant contributor ($\beta = -0.36$, $t = -3.49$, $p < 0.001$). On the WinG predicate comprehension scores, a regression model forcing those same variables led to a significant

model (R^2 = 0.11, F (2, 77) = 4.87, p = 0.010) with again the mode of test as the only predictor (β = -0.29, t = -2.69, p = 0.009).

Given that the mode of the WinG administration has a significant effect on the comprehension language scores independent t-tests were conducted to compare the WinG scores from children tested face to face versus online. Online children performed significantly better on the noun comprehension task (n = 32, M = 45.47, SD = 22.05) than face-to-face children (n = 48, M = 27.92, SD = 20.47); t (78) = 3.59, p = .001. Similarly, on the predicate comprehension task, children who did the language test online outperformed (n = 32, M = 45.94, SD = 21.08) those who did it face-to-face *(n* = 48, M = 32.81, SD = 19.90), t(78) = 2.79, p = .007. It should be noted that parents' and children's characteristics (parental education, DailyScreen and DailyScreenChildprop measures, children's gender) were similar in both samples. Surprisingly, parents did not spend more time on their phones during COVID-19 lockdowns, at least not in this sample. Children tested online were significantly older than children who came to the Babylab, but the WinG standardised scores integrate the age.

Then, Step 2 analyses were conducted to examine the effect of the screen time variables and age on the WinG test scores. On the WinG comprehension noun scores, when DailyScreen and age were entered into the model with parental education and the mode of the language test, there was no significant improvement in variance (F *change* (2, 75) = .12, p = .89). Similar results were observed when adding DailyScreenChildProp (p = .24) and age (p = .77) to the model.

A post-hoc power analysis was conducted using a linear regression on the WinG noun scores with 80 participants. Three predictors (parental education, mode of the test, and DailyScreen) were used. An R^2 of 0.19 and a significance level of 0.025 were used. Using the "pwr" package in R, we calculated Cohen's f^2 effect size ($f^2 = R^2 / (1 - R^2)$) and estimated the power to be 0.92 which showed a strong test power.

Similarly, on the WinG predicate scores, at Step 2, when adding DailyScreen and age to the model, there was no contribution of DailyScreen (p = .35) or age (p = .29). Similar results were observed when adding DailyScreenChildProp (p = .10) and age (p = .24) to the model.

WinG production scores

A hierarchical regression model on the WinG noun production scores with the parental education variable at Step 1 (as all those scores were collected in a face-to-face situation) led to a significant model (R^2 = 0.21, F (1, 28) = 7.51, p = 0.011), showing that the highest the parental education level is, the more words children produce. At Step 2, when adding DailyScreen and age into the model, there was no improvement in variance (*F change* (2, 26) = 1.20, p = .32) and only parental education as a contributor (p = 0.028) with no contribution of DailyScreen (p =.73) or age (p = .16). Similar results were observed when adding DailyScreenChildProp (p = .27) and age (p = .17) to the model.

Additionally, a post-hoc power analysis was conducted using a linear regression on the WinG nouns production scores with 30 participants. Three predictors (parental education

DailyScreen, and age) were used. An R-squared value of 0.28 and a significance level of 0.05 were used. Using the "pwr" package in R, we calculated Cohen's f² effect size ($f^2 = R^2 / (1 - R^2)$) and estimated the power to be 0.76.

Moderation analyses were conducted on the WinG scores (noun and predicate comprehension, and noun production subsets) with the Johnson-Neyman technique (Johnson, & Neyman, 1936), with children's age as a continuous variable. Contrary to Study 1, the results did not show an association between parental screen time (neither DailyScreen nor DailyScreenChildProp) and child language development, and children's age (for an example, see **Figure 18**).

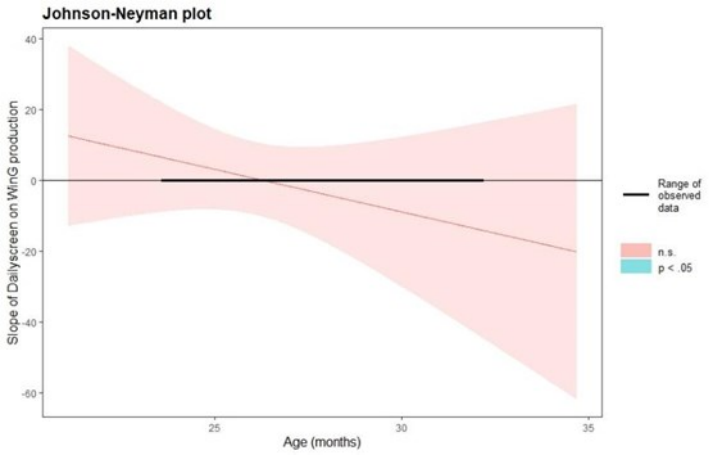


Figure 18: *Study 2.* Johnson-Neyman plot showing the association between DailyScreen and child's expressive vocabulary as a function of age.

3.5.3 Discussion

Study 2 was designed to test for a negative association between parental screen time and children's vocabulary knowledge, by using more objective measures of language skills and screen time estimates than in Study 1. However, contrary to the results of the first study and those of Mustonen et al. (2022) who focused on older children (2.5 years old and above), we found no indication of an effect of parental screen time on children's language development. We will address the methodological choices of our second study and the different findings of Studies 1 and 2 in the general discussion. We would like to point out here the unexpected finding that the WinG language test scores collected online were higher than those collected face-to-face, although parents' and children's characteristics (parental education, screen time and children's gender) were similar in both samples. The limitation of Study 2 in this regard, is that due to COVID restrictions, children completed the task in two different settings and thus it is difficult to consider this data as one homogeneous measure of language skills.

3.6 General Discussion of Studies 1 and 2

In two studies, we examined the possible links between parental screen time and children's vocabulary development in the early years. In the first, exploratory study, we found evidence of a negative correlation between parental time on screen and children's productive vocabulary as assessed by a parental questionnaire (CDI; Hamilton et al., 2000), but only for children aged 16 months and above. This corroborates Mustonen et al.'s (2022) findings that language abilities in Finnish children aged 2.5 to 4.1 years are negatively associated with

their mothers' time on screen. To increase the sensitivity for both measures of language skills and screen time, we ran a second study with children aged 19 to 32 months where parents were asked to complete a daily survey of their screen activity and provide in-built phone data, and where a face-to-face validated language test was administered (WinG; Cattani et al., 2019). However, we found no evidence of an effect of parental cell phone use on children's vocabulary in Study 2, which contradicts results from Study 1 and Mustonen et al. (2022). In what follows, we discuss the choices of dependent and independent variables across our study and that of Mustonen at al. (2022), in an attempt to reach the most reasonable conclusion about the whole results.

Reliability of parental screen time estimates

How reliable are our estimates of parental screen time in the second study, as compared to Study 1 and Mustonen et al. (2022)? First, parents reported spending overall 2h 44 min daily on screens in Study 1, and 2h 46 min on their phone in Study 2, which is consistent with eMarketer findings that the average US adult spent 2h 55min on a smartphone in 2019 (Wurmser, 2019). Note that in Mustonen et al. (2022), the average screen use of mothers was 5h 34min, which is considerably more than what we have observed in the UK and could partially explain why mothers' time on screen has a significant impact on language skills in the Finnish sample. In Study 1 and in Mustonen et al. (2022), parents were asked to estimate their own screen time with two questions only: one about a weekday and one about a weekend day. In contrast, in Study 2, we relied on averaging daily recordings over a week from the in-built phone software. It is interesting that the estimates did not vary much between an objective report of daily screen (Study 2) averaged across 7 measurements, and

a more subjective one based on two questions (Study 1). The standard errors in the two studies show that we may have been more precise in Study 2 (SE 0.12, versus 0.15 in Study 1), but not to the extent that parents' responses were significantly shifted.

The second measure of screen time we used in our two studies is the time spent on the phone while the child is around, which was not used in Mustonen et al. (2022) who only captured the overall daily time spent on screen. It was introduced as a direct measure of how parental screen might interfere with children's language learning. Indeed, if parents spend time on their screen while the child is asleep or while they are in daycare – which is what was included in the daily screen time in Mustonen et al.'s (2022) study or in our overall screen time measure – then this time does not count towards missed interaction opportunities. In Study 1, parents estimated spending 1h 20min on their phone while their child is around, but this amount might be underestimated as the questions parents were asked then involved all types of screens. In Study 2 where only cell phone usage questions were asked, parents reported spending 1h 19min on their phone around their child which corroborates the findings from Corkin et al. (2021) who reported that New-Zealand parents spend 1h 28min on their phone around their child of 18 to 25 months. In that study, parents completed a questionnaire about their own screen time and the screen media usage of their children on both weekdays and weekends, along with general questions about their technology use. Our parental screen time estimates are very similar to their findings, and might reflect realistic daily screen time habits of parents. However, the fact remains that using individual estimates might introduce some uncertainty about the reliability of the measure, and future research may include other screen time measurement tools that do

not rely on parental report to minimise the effect of bias in self-report. Stanford University developed the Human Screenome Project where they examined screen time by capturing screenshots of participants' phones every 5 seconds (Reeves, Robinson, & Ram, 2020); this could be used as a future, bias-free, measure of parental screen time.

Sensitivity of language assessment tools

In Study 1 and in Mustonen et al. (2022), children's language was assessed via language-specific adaptations of the CDI (Oxford CDI: Hamilton et al., 2000; Finnish CDI-III: Stolt, 2023), and an effect of parental screen time on language skills was found in both cases. While we used a CDI that only contained a vocabulary part, Mustonen et al. reported both the vocabulary scores and the general language abilities scores from the CDI-III. However, their analyses show that it is mainly the vocabulary skills which were related to parental screen time, since the general abilities scores only explained an additional 2% of the variance as compared to the vocabulary scores alone. In Study 2, we used a stantardised face to face test (WinG), which assesses receptive and productive vocabulary, and surprisingly did not find evidence of an effect of parental screen time, although it seems that we were targeting the area of language most likely to be affected by parental screen time (based on Mustonen et al.). It is interesting to note that Mustonen et al. (2022) also used face to face tests, that is, the receptive part of the Reynell Developmental Language Scales III (RDLS-III) (Kortesmaa et al., 2001), the Finnish Phonology test (Kunnari et al., 2012) and the Finnish Morphology test (Lyytinen, 1988)) but found no association between parental screen time measures and any of these tests. There are therefore three possibilities to explain the overall pattern of results: (1) standardised scores are not sensitive enough in

this type of research, (2) parental questionnaires capture information that face to face tests do not, and (3) the effect of parental screen time is only to be found on word production, and Study 2 lacked the power to reveal it (only 30 children out of 80 were old enough to complete the production part of the WinG).

Regarding the first possibility, inspection of Tables 11 and 13 shows that there is more variability in CDI scores (e.g. in production, mean = 0.16 and STD = 0.23) than in WinG scores (in noun production, mean = 42.17 and STD = 28.00), which is also what was observed by Mustonen et al. (2022) for the CDI-III productive vocabulary scores (mean = 67.18 and STD = 16.77) as compared to the RDLS-III (mean = 103.97 and STD = 12.51). These differences are expected: the WinG and RDLS-III scores are standardised, which brings age-related variability down. But in the case of the CDI, even though raw scores show higher individual variation, age is entered in regression models, absorbing the variance which is encompassed in standardised scores. Inspection of standard deviations in Mustonen et al. (2022) and in the current study are reassuring that our samples were representative of children across the spectrum of language skills, whatever tool was used to measure these.

Regarding the second possibility, namely that parental questionnaires capture different information than face to face tests, it is well documented that CDIs do suffer from occasional biases. Roberts, Burchinal and Durham (1999) observed that mothers from disadvantaged backgrounds may under-report their 30-month-old child's vocabulary and grammatical skills, possibly because working mothers had little opportunities to engage with their child. In contrast, and more typically observed, is the finding that parents from lower SES areas tend to over-report their child's language skills, and in particular in word comprehension

(e.g., Feldman et al., 2000). In the current study and in Mustonen et al. (2022), the sample was highly skewed towards highly educated parents, minimising the possibility of a SES-related over-reporting bias as seen for example in Bavin et al. (2008). However, there remains the possibility that, similar to what was observed by Roberts et al. (1999), the mothers who spend more time on their phone would be under-reporting their child's vocabulary skills, simply because they would miss on the opportunities to engage with their children. That was the main reason why we opted for a face-to-face test in Study 2, and it remains a valid point to explain why the effect of parental screen time on language skills is found with parental questionnaires, but not with face-to-face tests. If that is correct, then there is a real possibility that there is actually no significant impact of parental screen time on children's language development. If we take the estimates of parental screen time from Study 2 at face value, what we observe is an average daily activity of about an hour per day during which parents use their phone while their child is around. That does not necessarily mean that children miss out on parental interaction: it could be that parents choose to use their phone mostly when children are engaged in solitary play, or with siblings. It could also be that parents compensate for missed opportunities by providing richer interactions when not on their phone. Finally, it could be that an average of one hour of lost interaction per day per se does not result in a meaningful long-term impact on language skills. A growing body of research shows that children from some rural non-western communities get exposed to considerably less child-directed speech than is typically measured in urban and/or western families, and yet acquire language within the same milestones (e.g., Cristia, Dupoux, Gurven, & Stieglitz, 2019). As pointed by Cristia (2021) in her systematic review of

the prevalence of child-directed speech across cultures, “it is possible that urban children’s remarkable reliance on child-directed input is a side effect of its very salient prevalence” (page 12). In other words, when child-directed speech is less prevalent, children use other sources of information to acquire language knowledge, such as third-party interactions, or become more reliant on cues found in non-child directed speech. Parent-child verbal interaction is not always the main source of information that children rely on to acquire language, and therefore a potential reduction of these interactions due to screen time might not significantly influence children’s language.

Last but not least, the pattern of results might be due to the fact that parental screen time mainly affects expressive vocabulary skills. Recall that in Study 1, the effect of parental screen time was found only in CDI production scores, but not on comprehension scores. In Study 2, again there was no effect on word comprehension as assessed by the WinG, but also no effect on word production, which could be due to the fact that only 30 out of 80 children were old enough to complete the production part of the test. In Mustonen et al. (2022), the association with parental screen time was found with the CDI-III, which only assesses word production. When assessing syntax (with the FMT, the total score of the CDI-III and the RDLS-III), or receptive vocabulary (with the RDLS-III), then no association with parental screen time was found. Altogether, these data paint the picture of a very specific effect of parental screen time on children’s language skills: lexical production seems to be mainly affected, and only from the age of 16 months as suggested by Study 1. This age-related effect might stem from the fact that mother-child verbal interactions significantly increase from 1 year to 2 years and a half (Clarke-Stewart & Hevey, 1981) and parents

spending more time on their phones could impact the quantity of communication with their children.

Limitations

A first limitation to this study is the relatively small sample of older children who could be assessed with the production part of the WinG in Study 2. In our defence, this study was undertaken before we came across Mustonen et al. (2022), which made it clearer that we were less likely to find an effect in receptive skills than expressive skills. After Study 1 and without the knowledge brought by Mustonen et al. (2022), we assumed that the use of a face-to-face test was going to augment the sensitivity of all measures, including receptive lexical skills.

A second limitation is that we still may have not captured the most objective measure of the time parents spend on their phone while their child is around. However, we believe that we reported a realistic outcome as our time estimates were similar to Corkin et al.'s (2021). So far, the only effects of parental screen time on child language development were obtained with the overall screen time measure (Study 1 and Mustonen et al., 2022), not with the time spent on screen while the child around. It will be interesting to get a better understanding of what this means: either we must find a way to get a more accurate measure of this screen time measure, or we must understand why overall screen time alone has an effect on child language. As mentioned before, overall screen time captures moments where there are no loss opportunities for parent-child interactions (e.g., the child is napping), so if it does have an impact on child language skills, while time spent on screen with the child around does

not, it means that it is not the lack of opportunities per se that is responsible for the protracted language skills. Other factors could then be investigated, such as parental style and general engagement, that might lead to parents being more likely to spend more time on screens in general.

Conclusion

We have examined the possibility that parental screen time might negatively predict toddlers' vocabulary knowledge in two studies. Such a finding would have an important impact for recommendations to families and Early Years professionals, beyond the more traditional area of research which suggests that children's excessive screen time is detrimental to various aspects of their development. Such a negative correlation was found in the first study for children aged 16 months and older and for word production, but not in the second, despite using more objective and precise measures of both parental screen time and children's language skills. The possibility of different results between the two studies might be due to variations of parental screen time measures which needs to be addressed in further studies, but also, in the light of the results from Mustonen et al. (2022), might point to an effect circumscribed to productive vocabulary, and from the second half of the first year of life. At this point, we can only reasonably conclude that the association between the parental screen time and early language skills is not robust, at least not in the population that we have studied here. Further research will need to focus on getting a better understanding of the dynamics between parental screen use and children's activities.

4 Chapter 4: Screen Time and Children's Emotional Development

This chapter aims at examining the potential associations between children's and parental screen time and children's empathy. To provide a comprehensive context, we will begin with a summary on the developmental trajectory of children's emotional understanding from birth to 3 years old (based on the thesis introduction). This summary will serve as a foundation for understanding the significance of empathy assessment in children aged 3 years. Building upon this, we have chosen to measure cognitive empathy and social attention, considering the latter as a precursor to empathy (Bons et al., 2013). We will focus onto a study by Noten et al. (2019) which assessed 3-year-old children's cognitive empathy and social attention, outlining their methodology and findings. Following this, we will present a summary review of previous studies that have explored the relationship between screen use and children's socio-emotional development. Finally, we will introduce our current experiment, which aims to explore the potential links between children's and parental screen time and children's empathy. We will provide an overview of our experiment's objectives and its significance in contributing to the existing literature on children's socio-emotional development and screen time effects.

4.1 Literature Review

Infants at 4 months start to understand themselves as separate from others (Sachs et al., 2019), and at 1 year they become aware that emotions are often directed towards people

and objects (Phillips et al., 2002). At 2 years, they begin to develop cognitive empathy which involves the ability to understand the emotions of other people (Blair, 2005; Smith, 2006) and can categorise and label facial expressions based on emotions (Widen & Russel, 2008). By 3 years old, children are able to feel sympathetic and empathetic with others' emotions via perspective taking and can describe the causes of emotions in a situation (Grazzani et al., 2018; Harter & Whitesell, 1989; Wellman & Banerjee, 1991). It should be noted that previous research has shown that children and adults are better at identifying happiness than sadness (e.g., Guarnera, Hichy, Cascio, & Carrubba, 2015; Leime, Neto, & Torro-Alves). Additionally, it seems that children are more accurate and quicker at recognising positive emotions compared to negative emotions (Camras & Allison, 1985; Widen & Russell, 2003).

In the Noten et al. (2019) study, to investigate the relationship between empathy and aggression, cognitive empathy (assessed via children's own verbal responses) and social attention were measured. Social attention which can include attention to faces, voices, and body movements, is necessary to recognise people's emotions and can be considered as a precursor of empathy (Bons et al., 2013). In their study, social attention was defined as attention to faces. Three-year-old children were shown short emotional content clips (e.g., a happy child opening a gift, a sad child flushing a dead goldfish) and their social attention was assessed with eye tracking. Due to its sensitivity and objectivity, eye-tracking serves as an effective method for evaluating visual attention in young children (Zantinge et al., 2017). The relative total fixation duration to the face was used as an index of social attention, that is, the time that the child was looking at the face as a proportion of the total time that the face was visible on the screen and displaying emotion (Noten et al., 2019). Then after each

emotional video, children were asked what type of emotions (e.g., happy, sad, scared) the main character felt in the clip. The coding of answers involved recognition of emotions which was indexed as cognitive empathy. It was found that children attended more to the face in the sad clip than in the happy clip, and that they recognised equally well the happy and sad emotions. Therefore, Noten et al.'s (2019) study suggested no association between cognitive empathy and social attention in children of 3 years old. In contrast, Yan et al. (2017) found that social attention was positively associated with 5-year-olds' empathy. Their study investigated how children's (aged 5 to 6 years) behavioural and perceptual reactions to expressions of facial pain relate to their empathy. To assess children's empathy, they used a translated version of the 6-item (e.g., I feel sorry for other kids who don't have toys and clothes) self-reported questionnaire from Zhou et al. (2003). Social attention was measured through eye-tracking in an odd-one-out visual paradigm task (Kryrsko & Rutherford, 2009) which consists of searching for the emotional facial expression among neutral expressions (Yan et al., 2017). For their analyses, based on the self-reported questionnaire scores, they divided the participants into 2 groups: low empathy and high empathy groups. Children high in empathy performed better at the odd-one-out task than those low in empathy. It should be noted that Noten et al. (2019) and Yan et al. (2017) included different age groups (3 vs 5 years old) and their methodological differences in their studies might also explain their contrasting findings on social attention. Noten et al. (2019) only focused on facial happiness, sadness and fear expressions whereas Yan et al's (2017) main goal was to examine facial pain. Yan et al. (2017) used reaction time and accuracy as indexes of social attention with a visual search task where children had to find the emotional cartoon facial expression by

pushing a joystick to the corresponding direction. In contrast, Noten et al. (2019) assessed social attention through children's relative fixation duration to non-cartoon faces of clips showing emotional content.

In summary, the links between social attention and cognitive empathy are not clearly established with conflicting results in the literature, and it will be part of this current experiment to explore whether social attention can be associated with cognitive empathy and screen time.

Screen time and empathy

A report from Ofcom which analysed children's and parents 'media use and attitudes, reported that children aged 3 to 17 years old still watch live TV but are more likely to watch streaming services: 78% watched services like Netflix compared to 47% in 2021. The report also stated that six in ten children aged 3-17 played games online in 2021 (Ofcom, 2022). Screen use such as computer and video gaming has been negatively linked to poor interpersonal interactions and social competence in adolescents (Kowert et al., 2014; Lemmens, Valkenburg, & Peter, 2011), which can lead to a decrease in the development of emotion understanding (Karstad et al., 2015).

A few studies investigated whether screen time can be associated with the development of empathy in children. Skalicka et al. (2019) showed that increased screen time at age 4 was associated with poorer levels of emotional comprehension at age 6. Additionally, the presence of TV in a child's bedroom at age 6 predicted a decline in emotional comprehension by the age of 8.

Another study by Uhls et al. (2014) found that children aged 11 to 13 years who regularly used phones, TVs, and computers performed worse at reading human emotions than those who spent five days without screen time. Indeed, children were split into 2 groups: one experimental group who went to camp with no access to technology for five days and one control group who carried on with their regular daily routines, which included engaging in activities such as using screens. Both groups were shown pictures of faces at the start and the end of the five-day study session, and they were asked to name the emotions they represented. According to the results, children who attended camp outperformed those who had access to their media devices on reading facial expressions of emotion.

Other factors that can influence children's emotional development include socio-economic background (Gershoff, Aber, Raver, & Lennon, 2007; Hartas, 2011; Mayer, 2002). For instance, in Hartas' (2011) study, the socio-economic measures included the family income and the maternal educational qualifications. Children's socio-emotional competence were obtained from teacher ratings at the end of the first year of primary school (age of 5 years). Teacher ratings involved assessment scales on children's social and emotional progress. By conducting ongoing observations throughout the first year and taking into account the accomplishments outlined in the Early Learning Goals and guidance for the Foundation Stage (Hartas, 2011; Qualifications and Curriculum Authority, 2000), the study revealed that family income and maternal educational qualifications influenced children's scores in social/emotional development.

It should be noted that although SES can be evaluated using various indicators such as parental education, parent occupation, and family income. Among these measures, parental

education has been regarded as a more reliable and consistent assessment method compared to others, as it is considered to be more accurately reported and stable over time (Duncan & Magnuson, 2003). Additionally, a literature review from Brito and Noble (2014) reported that parental education was more linked to cognitive stimulation in the home than family income (Evans & English, 2002; Hoff-Ginsberg & Tardiff, 1995). Similarly, Eilertsen et al. (2016) found that parental education was a stronger predictor than house income on children's cognition. They examined the association between SES and cognitive function in 255 children aged 8 to 12 years. The parents reported their educational level and household income, and children's cognitive function was assessed with a Norwegian translated WISC-III, a standardised test of intelligence (Wechsler, 1991). Findings revealed that parental education was the only significant individual contributor of cognitive functioning. Therefore, parental education will be included as a proxy for SES in our current study investigating children's empathy and screen time.

Many studies on children's development investigated the effects of children's screen time instead of focusing on the parents' (Corkin et al., 2021). However, like children, parents themselves seem to spend more time on mobile devices such as cell phones and tablets, which may impact their interactions with their children. Zhou et al. (2002) revealed that parental warmth in interactions is positively related to children's empathy, especially for older children (aged 11 years). A reduction of parents' involvement when interacting with their children might negatively affect the development of empathy in children. Additionally, due to spending time on the phone, parents might be less socially and emotionally active with their children (Kildare and Middlemiss, 2017). Nabi and Wolfers (2022) examined

whether parental screen activities can be associated with children's (aged 5 to 12 years) general emotional intelligence, empathy, and emotional regulation skills. Four hundred parents were given a questionnaire about their own media use and their co-use with their children. To measure children's emotional intelligence, scales derived from the conceptualisation of emotional intelligence (Salovey & Mayer, 1990) were filled by the parents (e.g., "My child knows when s/he is happy", "My child exhibits emotional control by emphasising positive and deemphasising negative emotion"). Additionally, parents reported their children's emotional regulation from Shields and Cicchetti (1997)'s checklist (e.g., is impulsive; displays exuberance that others find intrusive or disrupting. To assess children's empathy, they were also asked to complete a seven-item empathic subscale of the Davis (1983) reactivity index that Nabi and Wolfers (2022) adapted. For example, an item could include "I would describe my child as a pretty soft-hearted person". Their findings demonstrated that parents' use of mobile devices was negatively related to children's general emotional intelligence. However, their results did not show parental media use to be associated with either children's empathy or emotional regulation. One of the limitations of this study could be that children's emotional development and parental media use were not objectively assessed as they both were reported by the parents.

4.2 Experiment 5: Screen Time and 3-year-old's Empathy

4.2.1 Introduction

To the best of our knowledge, previous research on screen use and children's empathy only included children above 4 years old. As children aged of 3 years are capable of showing cognitive empathy (Grazzani et al., 2018; Pons et al., 2004), the current study investigated whether children's and parental screen time can be related to 3 years old children's empathy development. It explored and assessed 3 emotion-related variables: social attention (defined as attention to faces in this experiment) with eye tracking, and cognitive empathy (emotion identification and emotional explanation accuracy) measured via children's own verbal responses. It was expected that screen time would be negatively correlated with children's cognitive empathy and social attention, and that children's cognitive empathy would be positively associated with social attention. As in Noten et al. (2019) study, children were shown emotional videos and eye tracking was used to assess social attention to emotional faces. However, unlike Noten et al. (2019), cartoon faces were preferred to real ones as research demonstrated that when presented with images for a brief duration, the ability to accurately identify emotions was improved for images that were more "cartoonised" compared to images that were photorealistic (Kendall et al., 2016). As social attention is a prerequisite of empathy (and not an index), we explored whether it can be linked to cognitive empathy and to screen time. Following Noten et al. (2019), questions about the recognition of emotions (happiness or sadness) of the individuals in the clips were asked to measure cognitive empathy - emotion identification in children. Questions about why the character in each clip was feeling happy/sad were asked to index cognitive empathy

- emotional explanation accuracy. Finally, questionnaires about the child's and parents' screen time were given to the parent to measure their regular screen use. When comparing screen time questionnaires from Study 1 (one-time filled questionnaire) and Study 2 (daily questionnaire for one week), it was found that they both had similar screen time estimates; therefore, in the current experiment, we decided to use a one-time questionnaire for parents to complete on their visit at the Babylab.

4.2.2 Method

An a priori power analysis was conducted using "Sample Size Calculators for designing clinical research" (Kohn & Senyak, 2021) to determine the minimum sample size necessary to test the study hypothesis. Results revealed the required sample size to achieve 80% power for detecting a medium effect, at a significance criterion of $\alpha = .05$, was $N = 29$ for $r = 0.50$, $\beta = 0.20$.

Participants

The study protocol was approved by the University of Plymouth Faculty of Health Ethics Committee. A total of 37 parents (36 were mothers) and their monolingual children (12 boys and 25 girls) aged 33 to 45 months (M = 40 months 1 day, SD = 2 months 24 days) were recruited from the Plymouth Babylab database. The highest of the two parents' highest educational levels on a scale of 1 to 6 (low to high: from primary school to postgraduate degree) were retrieved and the average parental educational level was 4.71. The families were all residents of Plymouth and its surroundings and had signed up to the Babylab to consider taking part in any proposed study.

Materials

During their visit at the Babylab, the parents first completed a demographic questionnaire to collect information about the family SES. Parental education was used as a proxy for SES.

At the same time, they were asked to complete a questionnaire about their own daily screen time (cell phone use) and their children's screen time (TV/streaming watching, gaming) (see **Appendix C**). The following daily estimate times on a weekday and on a weekend were retrieved from the parents' answers: the total time the parent spends on their phone, the time he/she spends on phone while the child is around, the time the child spends watching videos and/or gaming alone, and the time the child spends watching videos and/or gaming with family. The times were calculated and weighted 5/7 for a weekday and 2/7 for a weekend day to obtain weekly daily average screen time measures: overall parental screen time, parental screen time while child around, children's alone screen time, children's screen time with family.

Video clips. In total, 12 video clips (6 different non-emotional and 6 different emotional clips: 3 happy and 3 sad) from animation movies were used. As in Noten et al. (2019) study, the neutral video with no emotional or social content played calm music and non-narrative animations. The emotional clips had social content (e.g., a happy child receiving a gift, a sad person saying goodbye to his horse as the horse is released into the wild). The neutral and emotional clips lasted respectively approximately 60 seconds and 50 seconds each (Noten et al, 2019; van Zonneveld et al., 2017; Zantinge et al., 2017).

Eye-tracking. Following Noten et al. (2019), eye tracking was used as an accurate method for evaluating young children's visual attention, indexing social attention to emotional faces. A TOBII 300 eye tracker was used to gather gaze information within a specific area of interest (AOI) (Tobii Technology, Danderyd, Sweden). By corneal reflection techniques, the *x* and *y* position of the eye on the screen was recorded at 120 Hz. The video clips were shown on a screen that was positioned approximately 70 cm from the child's eyes. Dynamic AOIs for the whole face were drawn manually with a 1 cm margin around the face. To prevent overlap, mouth and eyes were included in the AOIs, in terms of accurately distinguishing the face (Noten et al., 2019; Zantinge et al., 2017). Prior to commencing the experiment, a five-point calibration procedure was performed. The relative total fixation duration to the face was used as an index of social attention (Noten et al, 2019) which is the percentage of the time the child was looking at the emotional face out of the total time that the face was clearly showing emotion on the screen as coded by the author and checked with a skilled research assistant (16.24s, 12.47s and 15.12s respectively for the 3 happy clips; 13.24s, 24.83s, and 17.77s respectively for the 3 sad clips). For example, during a clip, if a child looked at a face for 10s while it is showing a happy expression, and the total time where the face is expressing happiness in this clip is 16.24s, the percentage of social attention would be (10/16.24) *100 ~ 62%.

Procedure

After providing informed consent, parents were first asked to complete 2 questionnaires (demographic and screen time), prior to the eye tracking session.

Social attention to emotional faces

For the first part of the experimental task, the child sat on the parent's lap, watching 12 video clips (6 different non-emotional clips and 6 different emotional clips) on the eye-tracking monitor. Following Noten et al. (2019), each neutral video was immediately followed by a video clip with emotional content. The neutral clip was always presented first, and to avoid order effects, the order of the emotional clips was counterbalanced.

Cognitive empathy

For the last part of the task, the child, parent and experimenter were sat in another room with a computer screen and the child was shown the same 6 emotional clips as in the previous part. Following each emotional video clip, the child was asked to identify the emotion that the main character in the video felt: "Is the main character (e.g the little girl) happy or sad?" and "Why is the character happy/sad?". This procedure was performed for each emotional clip, so in total six times. While the child was engaged in the experimental task, the parent was asked to not interact with the child.

The children's answers for the emotion recognition of the clips and the quality of the explanations for the causes of the emotions were coded (both emotion recognition and quality of explanation were taken as measures of cognitive empathy).

Emotion recognition was assessed by asking the child whether the main character in the clip felt happy or sad. If the child did not give an answer when first asked, he/she was prompted (in our sample, all children gave a response for the emotion recognition). If the child gave an incorrect answer, he/she was not corrected. Then, regardless of the answer (even if it

was not correct), he/she was asked to explain based on their response. Answers were coded as 0 when the emotion was not recognised accurately, and 1 when the emotion was correctly identified (Noten et al., 2019). Then scores were converted as proportions of correct answers out of the total answers. The happiness recognition score (ranging from 33.33% to 100%) was the proportion of recognition of happiness for the happy content and the sadness recognition score (ranging from 33.33% to 100%) was the proportion of recognition of sadness for the sad content. One total emotion recognition score was calculated as the average of the happiness and sadness recognition scores.

If the child did not give an answer when first asked about the cause of the emotion, he/she was prompted. The coding of the quality of the explanations for the causes of the emotions was the sum of two scores and the maximum possible total score was 2. The first score ranged from 0=no explanation of emotion provided to 1=explanation of emotion provided. The second score ranged from 0=irrelevant explanation provided (explanation not logical given the story) to 1=relevant explanation provided (e.g., the girl is sad because Sulley is leaving). For the second score, if a child identifies an emotion incorrectly but then gives a logical explanation given the story, the answer counts as a relevant explanation and gets a score of 1. For example, if the child says that the girl is happy (which is incorrect, she is sad) because she's hugging Sulley (which is the case in the clip), the answer is scored 1.

Similarly, as above, scores were then converted as percentages of the quality of the explanations. The explanation of happiness accuracy score (33.33% to 83.33%) and sadness accuracy score (16.67% to 83.33%) were calculated. One total emotion explanation accuracy score was calculated as the average of the happiness and sadness accuracy scores.

4.2.3 Results

For the cognitive empathy analyses, the whole sample (*N*=37) was used. For the attention to emotional faces (social attention) analyses, children were excluded for showing no interest in watching the videos (*N*=2), for a lack of eye-tracking calibration (*N*=5), or for relative fixation duration being extreme values (4 and 12%) identified by the SPSS explore function (*N*=2). For the social attention data, the final sample is made of 28 children.

Demographic data and descriptive statistics for social attention, cognitive empathy (emotion identification and emotion explanation accuracy), and screen time are shown in **Table 15** below.

***Table 15**: Experiment 5.* Sample characteristics

	N	*M*	*SD*
Parents	37		
Education		4.71	1.64
Daily average total time on phone (hrs)		3.10	1.50
Daily average time on phone when child around		0.27	0.61
Children	37		
Daily average time TV watching/gaming alone		1.35	2.03
Daily average time TV watching/gaming with family		2.16	0.96
Cognitive empathy - identification (%)		88.74	13.06
Happy		93.69	15.39
Sad		83.78	24.37
Cognitive empathy – description (%)		47.22	38.93
Happy		49.10	41.75
Sad		45.50	38.22
Social attention (%)	28	57.71	21.39
Happy		54.26	25.00
Sad		61.15	23.18

Preliminary analyses

In this section, we examined whether age, gender and parental education would explain variance in emotion-related and screen time variables, using linear regressions.

A linear regression model was made on social attention, forcing age, gender, and parental education. It led to a non-significant model (R^2 = .10, F(3, 23) = 0.85, p = 0.48) with no significant contributors. Similar results were observed on cognitive empathy - emotion identification (R^2 = .03, F(3, 31) = 0.28, p = 0.84) and emotion explanation accuracy (R^2 = .09, F(3, 31) = 1.06, p = 0.39). It should be noted that the absence of parental educational level effect on these variables is not surprising given that the typical Babylab visitors belong to a relatively narrow range of largely middle-class population (Abdelwahab et al, 2021; Cattani et al, 2019).

Then, regressions were made to examine whether age, gender and parental education could predict screen time. A linear regression model was made on overall parental screen time, forcing age, gender, and parental education. It led to a non-significant model (R^2 = .08, F(3, 26) = 0.71, p = 0.56) with no significant contributors. Similar results were observed on parental screen time with child around (R^2 = .08, F(3, 26) = 0.73, p = 0.54), and children's screen time (children's screen alone (R^2 = .04, F(3, 31) = 0.44, p = 0.73) and children's screen with family (R^2 = .06, F(3, 31) = 0.63, p = 0.60)).

The children's age, gender, and parental education level were not significantly associated with neither the emotion-related nor the screen time variables. Regarding age, this is consistent with Nelson et al. (2011) who suggested that from age 3 to 4 years, many aspects of children's emotional development were highly stable such as emotion knowledge which

involved labelling emotions, affective perspective taking and identifying causes of emotions. Also, Aksan and Kochanska (2005) found evidence of high stability from 33 to 45 months of children's early conscience including moral emotion. Therefore, age, gender and parental education variables were not included in further analyses.

Plan of analyses

First, analyses were made on each of the 3 emotion variables (social attention to emotional faces, cognitive empathy – emotion identification and cognitive empathy – emotion description accuracy) to examine whether the type of emotion (happiness vs sadness) modulates these variables. Then, correlations were run between those three emotion variables, as well as between the four screen time variables (overall parental screen, parental screen use while child around, children's alone screen and children with family screen). Following this, to investigate our main hypothesis that screen time can be negatively associated with children's empathy development, correlations were run between the screen time variables and the children's emotion variables.

Social attention

Children spent a similar amount of time looking at the sad faces (M = 61.15%, SD = 23.18) than the happy faces (M = 54.26%, SD = 25.00); $t(27) = 1.64$, p =.113. There was a positive correlation between the relative total fixation duration to the happy faces and the sad faces, r = 0.58, p = .001, showing that attentive children tended to be attentive for both types of videos.

Cognitive empathy - emotion identification

There were no differences between the recognition of happiness (*M* = 93.69%, *SD* = 15.39) and sadness when asked to identify the emotion in videos (*M* = 83.78%, *SD* = 24.37); *t*(37) = 1.93, *p* = .062. Recognition scores were overall very high, indicating possibly a ceiling effect, and as can be seen on Figure 19, scores were at ceiling for all ages (see **Figure 19** below). No association was found between recognition of happiness and recognition of sadness, *r* = -0.20, *p* = .240.

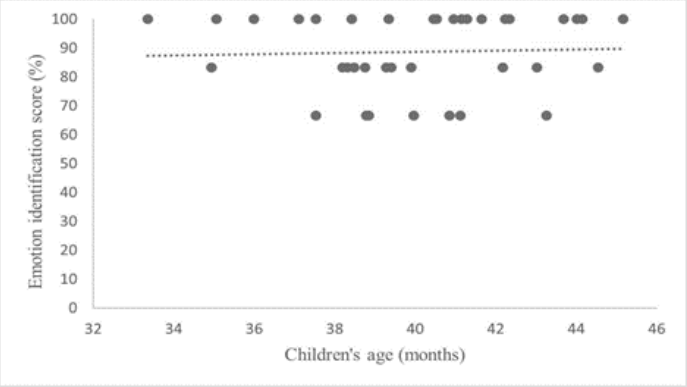


Figure 19: *Experiment 5.* Emotion identification scores as a function of age.

Cognitive empathy - emotion explanation accuracy

Regarding explanation accuracy, no difference was found between happiness (*M* = 49.10%, *SD* = 41.75) and sadness *(M* = 45.50%, SD = 38.22); *t(*36) = .97, *p* = .34. Explanation accuracy for the happiness emotion was positively correlated with the sadness emotion explanation, *r* = 0.84, *p* < .001, suggesting that children who were correct on one type of emotion tended to be correct on the other type.

Correlations between the children's emotions variables

For the results of the correlation between the emotion identification and emotion explanation accuracy variables, the Bonferroni correction was applied as they are both measures of cognitive empathy. Thus, the significance threshold was adjusted to 0.025. No significant correlations were found, neither between social attention to emotional faces and cognitive empathy - identification, r = -0.91, p = .645; nor between social attention and the cognitive empathy - explanation accuracy, r = -0.23, p = .907; and nor between the two measures of cognitive empathy, r = 0.16, p = .345.

Screen time

No significant correlation was found between children's screen use with family, and children's screen use alone, r = 0.28, p = .088. However, overall parental screen use was positively correlated with parental screen use while child around, r = 0.48, p = .006. The more parents spend time on their phones overall, the more they spend time on their phones while their children are around. Also, there were no associations between the parental screen time variables (neither overall nor while child around) and children's screen time variables (neither alone nor with family). As it was suggested that the impact of parents' screen time on their children's screen time differs depending on whether it is a weekday or a weekend (Jago et al., 2014), we examined the differentiation of screen time between weekdays and weekends but found no relation either between parental or children's screen time.

For the following correlations between children's screen time and children's emotion variables, the Bonferroni correction was applied as both children's screen use alone and children's screen use with family are measures of children's screen time. Thus, the significance threshold was adjusted to 0.025. Similarly, for the correlations between parental screen time and children's emotion variables, the Bonferroni correction was applied as overall parental screen use and parental screen use while child around are both measures of parental screen time. The significance threshold was adjusted to 0.025.

There were no significant correlations between the social attention to emotional faces and the screen time variables. Also, there were no associations between cognitive empathy - identification and the screen time variables. However, there was a negative moderate correlation between the emotion explanation accuracy with children's alone screen use, r = -0.40, p = .014 (see **Table 16** below). The more children spent time on screens alone, the less accurate they were when explaining the emotions in the video clips (see **Figure 20** below).

Table 16: *Experiment 5.* Correlations between the children's emotions variables and the screen time variables

Variable	Social attention	Emotion identification	Emotion explanation accuracy
Overall parental screen time	-0.035	0.16	-0.164
Parental screen time w/child around	0.172	0.082	0.042
Children alone screen time	-0.074	-0.035	-.404*
Children screen time w/family	0.039	-0.036	-0.028

* Correlation is significant at the 0.025 level (2-tailed).

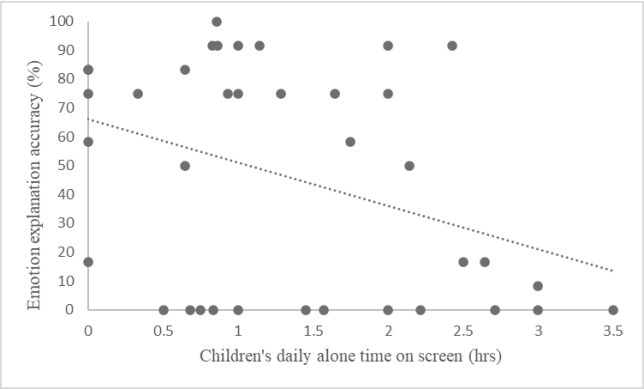


Figure 20: *Experiment 5.* Relation between children's daily alone time on screen (TV watching, streaming, gaming) and their emotion explanation accuracy on the videos.

4.2.4 Discussion

Our study investigated whether children's and parental screen time can be related to children's empathy development. It was expected that screen time would be negatively correlated with children's cognitive empathy and social attention, and that children's cognitive empathy would be positively associated with social attention. Our findings showed that one measure of screen time correlated negatively with one measure of empathy: children's alone screen use, and emotion explanation accuracy. No other screen time variables, or empathy variables (social attention and emotion recognition) showed any sign of association. It is important to note that neither children's social attention nor cognitive empathy (emotion identification and emotion explanation accuracy) scores increased with age in our study.

Social attention

There were no differences in social attention between the happy and sad contents which contrasts with Noten et al. (2019) whose participants attended less to the non-animated happy clips than to the sad clips. Previous studies suggest that negative facial expressions may capture attention for a longer period compared to positive expressions as they require more attentional resources than happy faces (Eastwood, Smilek, & Merikle, 2003; Srivastava, & Srinivasan, 2010). Indeed, it was found that happy expressions might be more quickly recognised as the integration of happy facial features are easier to process such as the mouth of happy faces being a highly salient visual feature (Calvo and Nummenmaa, 2008). Thus, sad expressions would require more time for the integration of facial features involved in configural processing (Calvo, Nummenmaa, & Avero, 2010).

However, Yan et al. (2017) who also used cartoon faces, did not find any differences between happiness and sadness on children's fixation duration, but they found differences between pain and the other emotions. Differences in methodology, such as variations in the length of the task, the timing, or the content of the stimuli, have been proposed as potential reasons for contrasting findings on social attention to emotional faces (Cooper & Langton, 2006; van Berlo et al., 2020). In our study, children might have equally looked at happy and sad videos because of the attraction to animated videos. Indeed, the Ofcom (2022) reported that younger children until the age of 8 were more likely to watch cartoons and animation movies than non-animated content. Observing animated shows is a popular pastime for children, and one of its appealing aspects is the inclusion of various imaginative events (Li et al., 2021).

Cognitive empathy – emotion identification

At first sight, children recognised equally well the happiness and sadness emotions in the clips which is in line with Noten et al. (2019) but, contrary to our results, their findings did not indicate a ceiling effect in children's emotion identification. Other research suggests that children can recognise positive emotions more accurately than negative ones (Camras & Allison, 1985; Guarnera et al., 2015; Widen & Russell, 2003). In our study, the emotion recognition task might have been too easy (as demonstrated by the ceiling effect), resulting in little variance in the data. Our own subjective appreciation of the video clips used by Noten et al. (2019) suggested no obvious differences between our cartoon clips and their inaminated clips. However, Noten et al. (2019) included emotion intensity coding in the scoring of children's emotion identification. Children were requested to justify their emotion identification by using emoticons to demonstrate the level of intensity associated with the mentioned emotion. The emoticons represented emotions with no intensity, low intensity, or high intensity. If children correctly identified the emotion but did not obtain a high score for the emotion intensity, it might have been why all of them did not have very high scores of emotion recognition.

Consequently, the fact that we did not find a relationship between social attention and cognitive empathy is difficult to interpret: if one variable displays low variability, a correlation would necessarily be non-significant.

Cognitive empathy - Emotion explanation accuracy

There were no differences between the explanation accuracy for happiness and sadness which support our results that children recognised equally well happy and sad contents. Noten et al. (2019) found a difference of social attention between happiness and sadness, but it is important to note that it is unclear whether there was a difference on children's emotion explanation accuracy as they did not take into account the valence of emotions in their analyses of emotion explanation. We might have failed to observe a difference between the happy and sad emotions for several reasons. It could be that parents verbally described happy and sad emotions equally to their children at home. Lagattuta and Wellman (2002) recorded parent-child interactions at home with six families (language samples taken between the ages of 2 and 5 years) and reported that parents and children, on average, discussed negative and positive emotions at equivalent rates. Interestingly, they found that discussions about the causes of emotions were significantly more prevalent in conversations regarding negative feelings compared to conversations about positive feelings. More research is needed with larger samples to explore whether the valence of emotion can have different effects on children's level of explanation. However, it should be noted that the absence of differences between sad and happy was not a critical finding that could impact the rest of our analyses to investigate parental screen time effects on children's cognitive empathy.

Children's emotion explanation accuracy was not associated with their cognitive empathy identification which is not in line with previous studies suggesting that children's language and conversational abilities can both directly and indirectly impact their empathetic

responses and behaviours (Ornaghi, Conte, & Grazzani, 2020; Ornaghi et al., 2017). Nevertheless, as previously mentioned, a ceiling effect of the emotion recognition task scores might also explain the absence of a relationship between this variable and emotion explanation accuracy.

Screen time

Parents reported spending 3h 06min on the phone which is consistent with eMarketer findings that the average US adult spent 2h 55min on a smartphone in 2019 (Wurmser, 2019). Parents estimated spending only 16 min on the phone while their child is around which is consistent with a previous study by Corkin et al. (2021) which reported that parents spend 10 min daily on their phone. On the other hand, this estimate does not corroborate with Study 2 which reported that parents spend 1h19 min daily on their phone while their child is around.

It is possible that the self-report might have been biased and parents who spend a high amount of time on their screens could underestimate their screen time while with their child. Additionally, parents reported that their children overall spend 3h30 min daily on screens which is consistent with Madigan et al. (2019) who reported that 3-year-olds overall spend 3h57 min on screens. Also, children were reported to spend 81 min alone on screens which is consistent with Neumann (2015) who reported that children of 2 years old spend 80 min alone.

In our study, parental screen time and children screen time were not correlated, which is surprising given that parental overall screen time was positively correlated with parental

screen while child around. Indeed, as the more time parents spend on their phone and the more time children spend on it with them, we would have expected a positive association between children's screen use on other devices (e.g., TV, tablets) and parental cell phone use as well. Moreover, we did not find an association between children's alone screen time and children screen with family. These results were not expected as families can be considered screen viewers as a whole and our results contrast with previous studies which have indicated parental screen time to be positively correlated with children's screen time (e.g., Carson & Janssen, 2012; Jago, Sebire, Edwards, & Thompson, 2013), and children co-screen use with parents has been associated with increased children's alone screen time (Latomme et al., 2018).

We might have not observed an association between parental screen time and children screen time for several reasons. It might be because we did not accurately measure screen time, especially, parents' estimations of their phone use when with their child might have not been reliable as a majority of parents tend to underestimate the amount of time they spend using smartphones when they are in the presence of their children (Kelly & Ocular, 2021). Indeed, as previously mentioned, parents estimated spending daily only 16 min on the phone while their child is around which is not consistent with 1h16 min daily reported in Study 2 which might have been a better estimate as a weekly diary was completed by the parents. The diary also included more detailed requested information such as the time that the parent was on the phone during the child's nap, the time that the parent spent on the phone while they were not with their child, the time that the parent and the child spent together on the phone, and the time that the child was on the parent's phone on her/his

own. In this Experiment 5, it is possible that the self-reports might have been biased and parents could under- or overestimate their children's screen time and/or their own.

Relation between empathy skills and screen time

First, no effects of parental nor children's screen time on children' social attention were found, which does not support our hypothesis. Indeed, as social attention is necessary to recognise people's emotions and is considered to be a precursor to empathy (Bons et al., 2013), we expected a negative correlation between social attention and screen time, if screen time does significantly impact on the learning opportunities in the early years.

Second, on one hand, our findings revealed that children's alone screen use was negatively correlated with their ability to accurately explain the reasons behind facial emotions. Our results might suggest that the negative effect of children's alone screen use on their emotion explanation accuracy may be attributed to them developing less verbal ability and language skills due to their time on screen. This is consistent with previous research on the adverse effects of children's alone screen use on their language skills (Birken et al., 2017; Chonchaiya & Pruksananonda, 2008; Corkin et al., 2021; Madigan et al., 2020; Zimmerman et al., 2007). For instance, Chonchaiya and Pruksananonda (2008) assessed children's (aged 15 to 48 months) TV viewing and language development by reviewing language milestones and the Denver-II screening test (e.g., "a 3- year- old should know how to talk well enough for strangers to understand most of the time"). Children who viewed TV by themselves were 8.47 times more likely to experience language delay than children who engaged in TV

watching alongside their caregivers. Indeed, when children spend more time on non-interactive media, where they are merely observers and not in active participation, the number of parents-child interactions may necessarily decrease, thus inducing negative effects on the children's language development (Morris et al., 2022). On the other hand, our results indicated that children's screen use was not associated with their cognitive empathy - emotion identification which contrasts with our hypothesis and previous findings (Skalicka et al., 2019; Uhls et al., 2014).

Third, there were no parental screen time effects either on children's empathy recognition skills which does not support our hypothesis. Indeed, it was expected that a reduction of parents' involvement when interacting with their children due to parental screen use might negatively affect the development of empathy in children (from 9 to 11 years old) as reported by Zhou et al.'s (2002) longitudinal study. However, our findings are consistent with Nabi and Wolfers' (2022) which suggested that parental media use and children's (aged 5 to 12) empathy were not related. Moreover, there might be no effects of parental screen time on the empathy of children as young as 3 years old. Indeed, Zhou et al. (2003) reported that parental warmth in interactions was positively related to children's empathy, especially for older children aged 11 years. As children of 11 years old spend more time on screens (Ofcom, 2022) than 3-year-old's and as research on empathy in children has demonstrated significant developmental progress in the tendency to share the emotions of another person or to empathise (Wilson & Cantor, 1985), parental screen time effect might be more likely to be found on older children. Nevertheless, as previously mentioned, a ceiling effect might explain our null results.

Furthermore, no parental screen time effects on the children's emotion explanation accuracy were found which is not in line with our hypothesis as parental phone use can affect the quality and quantity of time parents spend with their children, with consequences on children's language and socio-emotional development (Kildare and Middlemiss, 2017). We indirectly measured children's verbal fluency and therefore to some extent we would have expected our findings to be in line with Mustonen et al. (2022) where parental screen time was negatively associated with children's expressive language skills. Nonetheless, their study focused on children' expressive vocabulary knowledge, which might not be linked to children's verbal and emotional explanation abilities. This aspect of parental screen time effects on children's verbal abilities requires further investigation and remains an open question.

Limitations and future research

The current study adds to the literature by examining cognitive empathy, social attention and both parental and children's screen time. However, it is important to acknowledge certain limitations within the study. For instance, the children's high emotion recognition scores suggest a task-related ceiling effect, indicating that the emotional content videos chosen may not have been sufficiently challenging to elicit a diverse range of cognitive empathy responses from the children. This could potentially explain why no significant associations were found between children's cognitive empathy - emotion identification, social attention, and screen time. To address this limitation and obtain more accurate measurements of cognitive empathy - emotion identification, future research should consider utilising more challenging and diverse video stimuli that can effectively capture

variations in children's cognitive empathy abilities. By incorporating more demanding emotional content, future studies can better assess the potential links between children's empathy, social attention, and screen time. Indeed, as accuracy for identifying emotion on more “cartoonised” than photorealistic images is enhanced (Kendall et al., 2016) with children with a similar age range, using inanimated videos like Noten et al. (2019) might be preferable as they observed a difference between the happy and sad conditions. Another possibility would be to reduce the amount of speech on videos, to provide less cues and context and make the identification of the emotional content more challenging. Finally, introducing a graded response scale as Noten et al. (2019) did might help reduce the ceiling effect.

Another limitation is the potential bias in the parental report of their estimate of their own use of screens and their children’s. Parents’ and children's screen time could potentially be underestimated, as the measurement of screen time relied on parents' recollection reports, which may not be entirely reliable (Radesky et al., 2020). Although parents were asked to report their daily screen time based on phone data, accurately capturing the screen time habits of both parents and children can be challenging, particularly when parents had to estimate their phone usage while their child was present. To mitigate the impact of self-report bias, it might have been preferable to use a similar screen time measure tool as in Study 2 where a weekly diary was used and retrieved more detailed information such as the time that the parent was on the phone during the child’s nap and the time that the child was on the parent’s phone on her/his own. In this Experiment 5, our screen time estimates, especially when the parent is on the phone while their child is around, might not have been

as accurate as a one-time filled questionnaire was used. Future research could incorporate alternative screen time measurement tools. One such tool is the Human Screenome Project developed by Stanford University, which involves capturing screenshots of participants' devices at regular five-second intervals, providing a potential unbiased method to measure parental screen time (Reeves, Robinson, & Ram, 2020).

Moreover, this study only focused on smartphone usage when examining parental screen time, neglecting other forms such as TV, video games, and computer screen time. To address this limitation for future research of the impact of screen time on empathy development in children, a possible approach could be to adopt the method used by Rasmussen et al. (2020). They conducted pilot trials where they objectively assessed screen time across various devices in households with children aged 4-14. The researchers installed an application on smartphones and tablets, placed TV monitors on each TV in the household, and implemented tracking software on every PC. Subsequent studies could explore the relationship between the daily screen time habits of all family members (including different types of screens like TV, tablets, cell phones, computers, and gaming devices) and children's empathy development.

Furthermore, while a correlation between children's alone screen use and cognitive empathy - verbal ability was detected in our study, it is important to recognise that correlation does not imply causation. It is crucial to consider other unmeasured factors that could potentially influence children's empathy development. For instance, variables including parenting styles, parental warmth, and expressiveness (Denham & Kochanoff, 2008; Zarra-Nezhad et al., 2015, Zhou et al., 2003) play significant roles in shaping a child's

empathy skills. These factors may interact with screen time as well (Kildare and Middlemiss, 2017), making it essential to account for them in future studies to better understand the relationship between screen use and empathy development. Future research is necessary to examine a broader range of factors in order to obtain a more nuanced understanding of the various influences on children's empathy beyond screen time alone. Children's alone screen time might have a negative impact on children's development and according to the guidelines from the Word Health Organization (2019), children between 2-5 years old should have no more than 1 hour of high-quality screen time per day, and even this limited screen time should be supervised and accompanied by interactions with caregivers.

5 Chapter 5: General Discussion and Conclusion

The main aim of this thesis was to examine the possibility that parental screen use might be negatively related to toddlers' language and emotional development. Most of research in the field of social media has focused on examining the effects of their use by children and teenagers, rather than exploring the effects of parents' screen time (Corkin et al., 2021). However, caregivers themselves are observed to spend significant amounts of time on mobile devices such as cell phones and tablets. They may have become less socially and emotionally active (Kildare and Middlemiss, 2017), which may impact the quality and quantity of interactions they have with their children. A few previous studies (e.g., Mustonen, Torppa, & Stolt, 2022; Nabi & Wolfers, 2022; Reed, Hirsh-Pasek, & Golinkoff, 2017) have investigated how parental screen time can be linked to the language and emotional development of young children. Nabi and Wolfers (2022) did not find a connection between parental media use and empathy or emotional regulation in children aged 5 to 12 years. However, parents' use of mobile devices was negatively related to children's general emotional intelligence. Moreover, the findings of Reed et al. (2017) suggested that parental distraction through phone calls (manipulated in a lab setting) had a negative impact on word learning in 2-year-old children. Additionally, Mustonen et al. (2022) reported a negative association between mothers' screen time (in real life) and the vocabulary knowledge of their children aged 2.5 and 4 years. Taken together, it remains an open question as to whether parental screen use, especially in the presence of the child,

interferes with real-life opportunities for language and emotional development, particularly in children younger than 2.5 years.

The primary goal of this thesis was to investigate the relationship between parental screen use and young children's development. However, as the start of the thesis coincided with the COVID-19 pandemic, we had to adapt a few paradigms to online testing, as a preparatory work for the main investigation. This dissertation therefore raised the following questions, sequentially: Can online experiments with children provide valid data? Is there a relationship between parental screen use and children's language skills? Is there an association between children's and parental screen time and children's emotion recognition?

The first three experiments, written as part of a paper under revision at the time of writing (Nguyen, Fitzpatrick, & Floccia, in press), tested online adaptations of word recognition through preferential looking (Exp 1), word learning in a Switch task (Exp 2) and language assessment (Exp 3). In Experiment 1, previous lab-based findings were replicated (e.g., Vihman et al., 2007) and showed robust word recognition in children, with a minimal rate of attrition or data loss, i.e., less than 5%, in contrast to some previous lab-based studies. In Experiment 2, infants significantly learned a new word which is consistent with previous in-lab (e.g., Yoshida et al., 2009) and online research (Bulgarelli & Bergelson, 2022) involving the Switch task. Experiment 3 demonstrated that children can perform well on a language assessment test administered online with high levels of engagement and responsiveness throughout the task.

Experiments 1 and 3 showed there can be high levels of engagement for young participants when tested in the home environment. Indeed, we found that instead of being distracted

by their surroundings, children remained engaged for the duration of the experiment. In Experiments 1 and 2, effect sizes were not only replicated, but were much higher in magnitude. These experiments results provide encouraging support for adapting infant paradigms to online testing and provided a reassuring framework for undertaking data collection online, when that was needed (Experiments 1, 2, and 3).

Experiment 4 and subsequent studies or experiments address the main question of this thesis, which is whether parental screen time affects early child development. In Experiment 4, we did not find an immediate effect of parental phone text use on children's (aged 17-19 months) word learning in a lab situation. However, children whose parents knew the true purpose of the study ("we were investigating whether texting can impact word learning in children") learned the words better than children whose parents had the cover story ("we were interested in how parents feel while they teach a new word to their children").

In Study 1, we did not find parental screen time (in real life) to be a predictor of language development in children younger than 16 months. Nevertheless, in children older than 16 months, there was a trend for word production to be negatively associated with the amount of daily time that parents spent on screen outside of work.

In Study 2 which was conducted with more objective measures of screen time and children's vocabulary, we found no indication of an effect of parental screen time (in real life) on children's (aged 19 to 32 months) language development.

In Experiment 5, our findings showed that one measure of screen time correlated negatively with one measure of cognitive empathy: children's alone screen use, and emotion

explanation accuracy. No other screen time variables (overall parental screen time, parental screen time when the child is around, and children's screen time with family), or empathy variables (social attention and emotion recognition) showed any sign of association.

Can online experiments with children provide valid data?

With some modifications to lab-based procedures (Experiments 1 and 2), the IPL and Switch tasks successfully remotely collected eye movement data and provided solid replications of established results. Children were highly engaged during the tasks of Experiments 1 and 3 which might be due to them feeling more comfortable and at ease in their home, as suggested also by Tsuji et al. (2022). Testing online is not without its limitations such as children's attention might fade throughout the online session (Experiment 2). Tsuji et al. (2022) quoted Chuey et al. (2021), Shields, McGinnis, and Selmeczy (2021) who recommended to keep the tasks short and to elicit regular responses from children with synchronous tasks to monitor children's engagement. Another limitation is that certain types of paradigms might not be adaptable depending on the age. Lapidow et al. (2021) showed that children's age significantly influences their performance in an online setting. Notably, older children performed better compared to younger children. These results differ from what would typically be observed in a lab or in-person setting. Furthermore, while our research did not show any direct influence of parents on their children's behaviour during testing, it is crucial to acknowledge the limitations of our experiments regarding control over the testing environment and parental behaviour during the process. To address this concern, we recommend providing explicit instructions to parents, including visual aids like

instructional images or videos whenever feasible, to guide their behaviour and emphasise the significance of adhering to these guidelines. Nonetheless, conducting additional online testing could help determine the necessity of such instructions during both at-home and in-lab testing scenarios. The insights gained from this further investigation could then be used to refine and optimise the instructions given to parents during in-lab testing sessions.

More replication studies are required before generalising beyond Experiments 1, 2, and 3 that testing online is suitable for other infant paradigms and other infant populations. For instance, paradigms designed to evaluate children's knowledge of syntax could be effectively implemented in online testing, such as the *elicited production* that explores whether young children possess abstract knowledge of a particular structure (e.g., Ambridge, 2011). Similarly, paradigms aimed at assessing socio-emotional regulation in infants can be modified for online experiments. McElwain et al. (2022) successfully validated an online visit protocol that assessed the behaviour of mothers and infants during the SFP. Their findings indicated that during online visits, mothers and children produced similar proportions of facial expressions, vocalisations, and directions of gaze as observed in lab visits, including comparable behavioural changes in infants across different episodes of the SFP.

<u>Is there a relationship between parental screen use and children's development?</u>

Experiment 4, Study 2, and Experiment 5 consistently failed to demonstrated associations between parental screen use and children's (aged 17 to 45 months) language and emotional development. They do not support our hypothesis that parental screen usage would be

negatively related to young children's development, presumably because they would affect the quality and quantity of parent-child interactions. These findings contrast with the exploratory Study 1, where we found a link between parental screen time and children's vocabulary knowledge, for infants aged 16 months and above. However, this association was not fully robust across age and screen time measure, and measures of screen time and children's vocabulary were less objective than those used in Study 2. To the best of our knowledge, Studies 1 and 2 are ones of the first studies to investigate the association between parental screen time (especially when the child is around) and the language development of children younger than 2.5 years, observed in real life, and not in a lab situation (see review by Morris et al., 2022).

For Experiment 4, we decided to replicate and extend Reed et al. (2017)'s study but using texting distraction instead of phone calls, to better represent the main communication activity most people engage with on their phone. In another depart from Reed et al. (2017), we manipulated the instructions provided to parents (one half was given the true purpose of the study, the other half the cover story), and provided a more in-depth analysis of the parents' and children's behaviours. Results from Experiment 4 showed that the interruption by the phone use (manipulated in a lab) did not impact children's word learning which contrasts with Reed et al. (2017) who found that parental phone use can negatively impact children's word learning. Our results are consistent with Konrad et al. (2021) who did not find an impact of interruption by phone text on children's (aged 19 months) learning either. Interestingly, we found that children whose parents knew the true purpose of the phone use in the experimental task (that is, find out whether texting can impact word learning in

children) learned the words better than children whose parents did not know the true purpose of the phone use ("we will text you a questionnaire link to see how you feel while you teach something new to your child"). In line with Konrad et al.'s (2021) findings and Morris et al. (2021) who reported that technoference may not be a direct causal factor in language development but rather a moderating factor, our results might imply that parental responses to technoference and attitudes towards smartphones can moderate the effect of technoference on children's language development. More than the genuine effect of screen time, these findings point to the strong impact of parental attitude towards mobile phone use in word learning situations.

Experiment 5 revisited the link between parental screen time and children's emotional development with a more objective empathy assessment than Nabi and Wolfers (2022) who relied on parents' reports, and with younger children. We assessed social attention (considered a precursor to empathy; Bons et al., 2013) with eye tracking, and cognitive empathy was measured via children's verbal responses. We found that parental screen time was not related to children's cognitive empathy, consistent with Nabi and Wolfers' (2022). This might suggest that there might not be an effect of parental screen time on the empathy of children as young as 3 years old. Zhou et al. (2003) reported a positive association between parental warmth in interactions and children's empathy, particularly among older children aged 11 years. Given that 11-year-olds tend to spend more time on screens (Ofcom, 2022) compared to 3-year-olds, and considering the significant developmental progress in empathy observed in children (Wilson & Cantor, 1985), it is more plausible to find the effects of parental screen time on older children rather than younger ones.

Our findings answer some of the questions addressed at the beginning of this work. Nevertheless, several related questions remained unanswered, and our results have raised avenues that need to be explored in future research.

How could screen use affect early development?

First, it might be more possible to find the effects of parental screen time on language skills in older children rather than younger ones. Indeed, when they turn 2 years old their vocabulary knowledge is much higher ranging from 200 to 500 words, whereas at around 18 months of age, they approximately know 50 words (Fernald et al., 2001). Possibly a floor effect in their vocabulary skills might hide any modulating effect of parental screen use.

Second, a limitation of this work is the relatively small sample of older children who could be assessed with the word production task of the WinG in Study 2. In our defence, this study was conducted prior to our awareness of Mustonen et al.'s (2022) findings. Their research clarified that it was less likely to observe an effect in receptive skills compared to expressive skills. Before incorporating the insights from Mustonen et al. (2022) who used a parent report on the CDI and based on the outcomes of the exploratory Study 1, we had assumed that using a face-to-face test would enhance the sensitivity of all measurements, including receptive lexical skills.

Third, so far, only parental screen effects were observed on child's language development when considering the overall screen time measure (as seen in Study 1 and Mustonen et al., 2022), not specifically the time spent on screens while the child is around. It is intriguing to

acquire a better understanding of this phenomenon: we need to either find a more accurate way to measure screen time in this context or comprehend why overall screen time alone affects child language. If overall parental screen time impacts child language skills while time spent on screens with the child present does not, it implies that the deficiency of opportunities itself is not solely responsible for delayed language skills. Nevertheless, using individual estimates may introduce too much uncertainty regarding the reliability of the measure. In future research, alternative screen time measurement tools that minimise bias in self-report could be employed. For instance, Stanford University's Human Screenome Project has developed a method of examining screen time by capturing screenshots of participants' phones every 5 seconds (Reeves, Robinson, & Ram, 2020). This unbiased approach could be considered as a measure of parental screen time in future studies. Similarly, Morris et al. (2021) mentioned a pilot study by Yuan et al. (2020) who used a passive mobile sensing app to objectively measure parental screen time over 14 days. With the Minuku prototype app (Chang, 2016) that collects app usage statistics and history every 5 sec, the researchers were able to download screen on/off status, the name of the most recently used app in the foreground, and the time when that app was last used. They concluded that using app-based mobile sensing shows potential for achieving precise evaluation of mobile media usage by parents and children, making it a straightforward way to improve the accuracy of measuring mobile media and health-related activities. Indeed, they believe that this approach could prove valuable for researchers seeking to assess the duration, timing, and content of mobile phone usage in clinical or behavioural research.

Fourth, all our studies but one (Study 1) solely focused on smartphone usage when examining parental screen time, disregarding other forms of screen time such as TV, video games, and computer usage. This might have underestimated the time spent by parents on screens, and explained partially why Study 1, where we asked parents to report any type of screen type, may have provided evidence of a negative association with language skills. To address this limitation and enhance future research on the impact of screen time on child development, a potential approach could involve adopting the method employed by Rasmussen et al. (2020). In their pilot trials, they objectively assessed screen time across various devices in households with children aged 4-14. The researchers installed a specific application on smartphones and tablets, placed TV monitors on each TV in the household, and implemented tracking software on every PC. Subsequent studies could investigate the correlation between the daily screen time habits of all family members, encompassing different types of screens such as TV, tablets, cell phones, computers, and gaming devices, and their influence on children's language and emotional development.

Fifth, we did not assess the reasons why parents use their phones, and previous studies have indicated that the reasons behind parents' smartphone use in the presence of their children (such as the need to stay connected for work, a sense of security when their phone is nearby, or using the phone occasionally as an escape) can mediate the relationship between children's language skills and the effects of screen use (Konrad et al., 2021; Morris et al., 2022). Hence, it would be valuable to further explore the impact of parental attitudes towards phone use and their role in children's development to establish for certain whether a causal relationship between parental phone use and language outcomes exists. Moreover,

as pointed out by Konrad et al. (2021), a more in-depth examination of the family's media environment is necessary to investigate whether young children become accustomed to the effects of technoference.

Sixth, it is crucial to take into account other unmeasured factors that could potentially influence children's language and emotional development. Variables such as parental interactions with children, the home literacy environment, parental teaching and speaking styles, parental warmth and expressiveness have been identified as significant factors in shaping a child's language and empathy skills (Adamson et al., 2015; Denham & Kochanoff, 2008; Liebeskind et al., 2014; Rowe, 2012; Zarra-Nezhad et al., 2015, Zhou et al., 2003). These factors may also interact with screen time (Kildare and Middlemiss, 2017), highlighting the importance of considering them in future studies to gain a better understanding of the relationship between screen use and children's development.

Seventh, in our research, and in Mustonen et al. (2022), the sample was highly skewed towards highly educated parents. It would be interesting to conduct studies on the effects of parental screen use on children’s language and emotional development with a more diverse sample. Indeed, a few observational and qualitative studies that included families with diverse ethnicity and socioeconomic backgrounds (Elias et al., 2020; Garg, 2021) have reported that the phone use influenced parent-child interactions. Parents who used their mobile phones at playgrounds and eateries often positioned their bodies away from their child and they maintained little to no eye contact with them (Elias et al., 2020). It was observed that parents' smartphone usage differed between their behaviour at home and when they were out with their children (Garg, 2021). When parents are outside the home,

they may interact less with their children, as the external environment itself such as a playground can entertain the child (Morris et al., 2021). Elias et al. (2020) did not take into consideration whether the families' SES or cultural background influenced parents' phone use and parent-child interactions. However, Garg (2021) revealed that various aspects of parents' lives, including their household responsibilities, cultural values, and beliefs (both inherited from their parents and influenced by their socioeconomic status and work life), as well as their living arrangements (such as having access to private spaces), played a significant role in shaping their choices regarding technology use for their children. More specifically, those who shared their living spaces with their children due to financial constraints or small residential units used technology more frequently while caring for their children compared to parents with private time and spaces. The parents' cultural background, learned values, and attitudes towards technology and its impact on their children also influenced their technology practices. Garg (2020) reported that in all these cases, parents were driven by a desire to uphold their values and preserve specific beliefs about technology use. Thus, as SES can contribute to parental screen use and can also be a predictor of children's language and emotional development (Hart and Risley, 2003; Hartas, 2011), further studies with a more diverse sample could provide deeper insights into how socioeconomic factors interact with parental screen use and their combined effects on children's language and emotional development.

Finally, according to the review by Morris et al. (2021), most of the research on parental screen use and language development has been cross-sectional, which means that it does not allow for observing the unfolding effects of parental screen use on language over time.

They quoted that while some studies have shown that parents may be less responsive to their child during one-time observations (e.g., Elias et al., 2020), these single data points may not accurately represent regular occurrences of technoference. Therefore, it is important to conduct longitudinal research to examine the long-term impact of parental phone use on children's language and emotional outcomes. Morris et al. (2021) reported that a few longitudinal studies that have assessed parental phone use in children from birth to age 5 have taken into account a broader range of devices beyond mobile phones as sources of technoference and have explored non-language outcomes such as child behaviour or attachment (Coyne et al., 2022; McDaniel & Radesky, 2018). Interestingly, Coyne et al. (2022) found different results when comparing cross-sectional and longitudinal data. Indeed, in the cross-sectional analysis, they discovered a positive association between parental media use during feeding and levels of parent/infant dysfunction. However, when they examined this relationship longitudinally, they observed that media use during feeding was associated with lower levels of parent/child dysfunction. Their findings underscore the necessity for longitudinal studies that effectively capture the ongoing effects of parental screen usage and children's development. Data from longitudinal research would document whether recommendations should be made to parents to remind them to limit their own screen time so as to not hinder their child's development. This research might provide vital evidence for healthcare professionals to use, to help towards their recommendations for parents.

Final conclusions

The aim of this dissertation was to investigate a possible relationship between parents' screen usage and children's development. Regarding this investigation, our data provide four major pieces of information concerning young children:

- Firstly, there seems to be no robust parental phone text impact on children's learning in a lab situation (Experiment 4). Consistently, Konrad et al. (2021) found no effect of phone text either whereas Reed et al. (2017) revealed that parental phone call negatively impacted children's word learning.

- Secondly, our results might suggest that parental responses to technoference and attitudes towards smartphones may moderate the relationship between parental screen use and children's development (Experiment 4) which is in line with Konrad et al. (2021) and Morris et al. (2021) reports.

-Thirdly, there seems to be no robust effect of parental screen time on the language development of children younger than 2.5 years, when parental screen time is measured consistently (Studies 1 and 2).

- Finally, there seems to be no relationship between parental screen time and young children's empathy (Experiment 5), which is consistent with Nabi and Wolfers (2022).

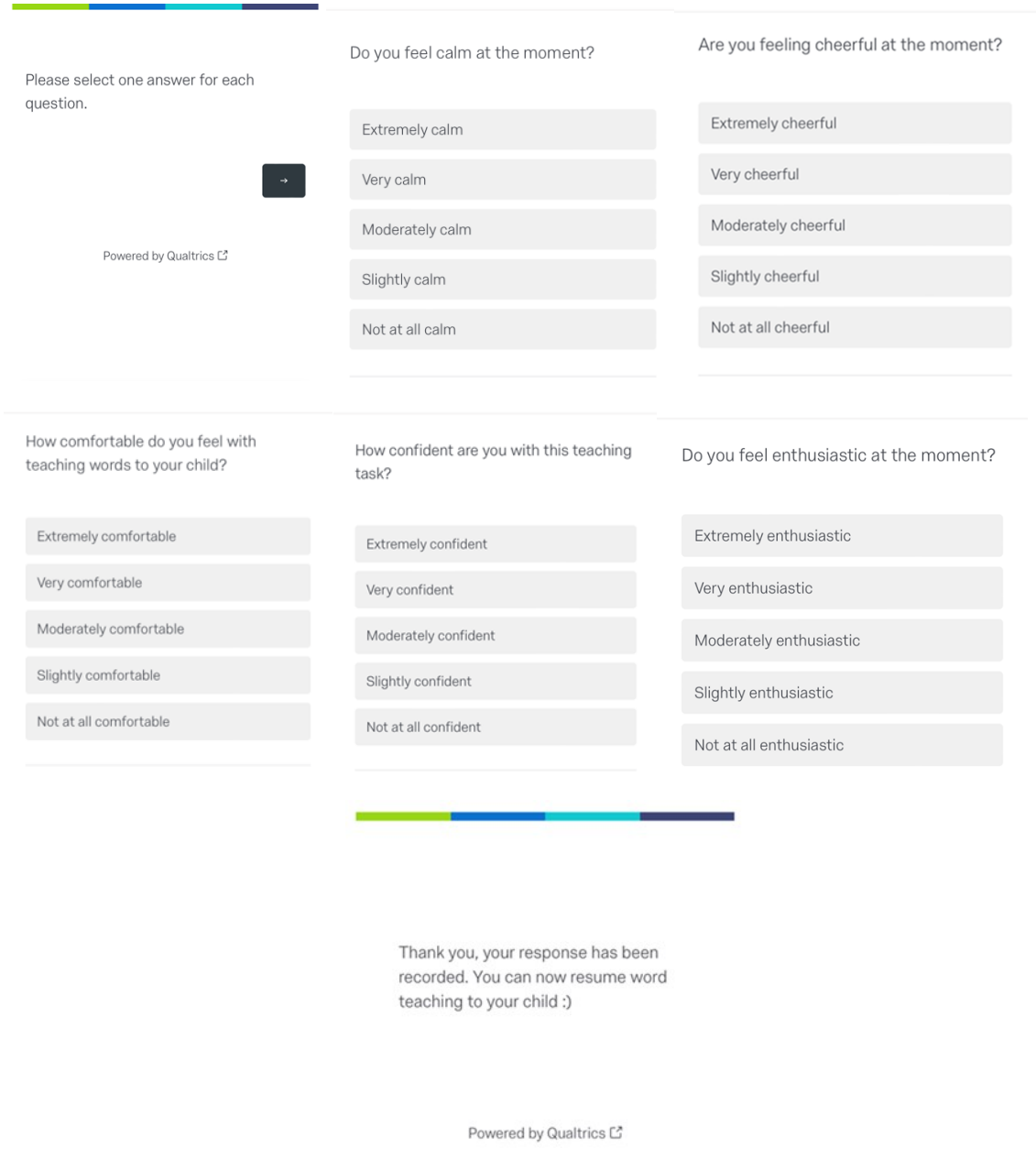
From this research, can we derive recommendations to parents about their screen time? At this point, we cannot provide an answer, and can only reasonably conclude that the association between parental screen time and early language is not robust, at least not in the population that we have studied here. Contrary to Mustonen et al. (2022) who propose

for healthcare professionals to recommend restricting mothers' overall screen time as they believe that it can lead to poor language abilities in children, we did not find a negative effect of parental screen use on children's development, and we cannot recommend whether parents should limit the time they spend on their phones.

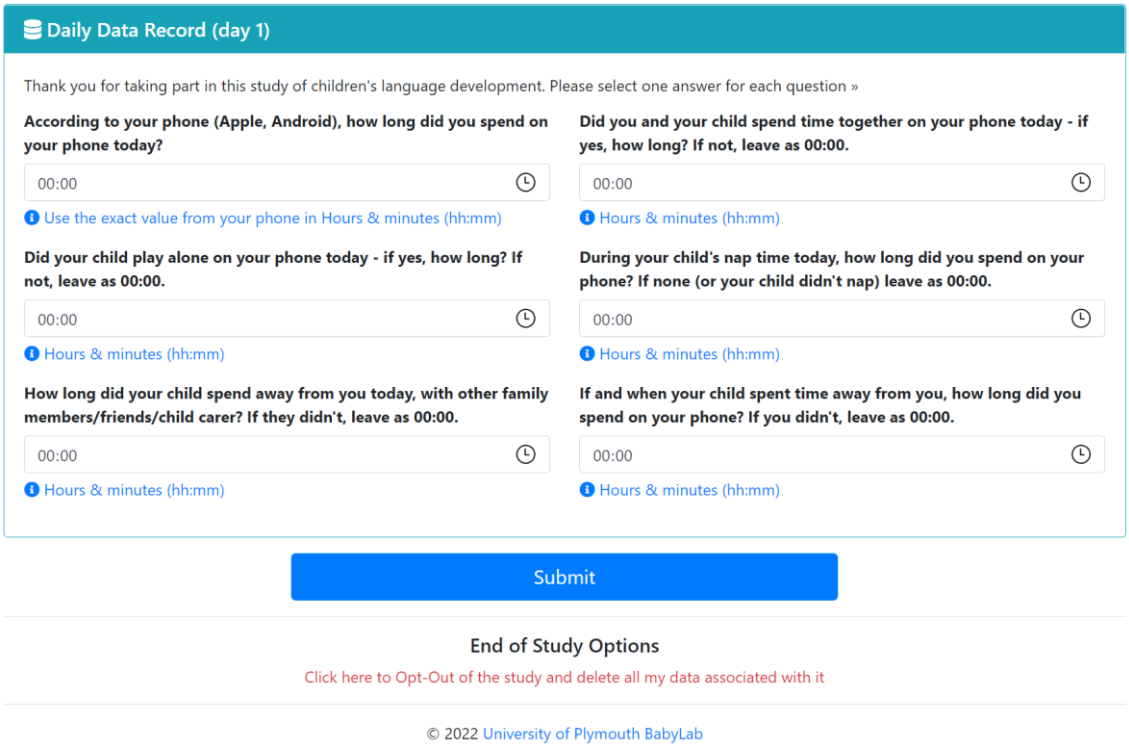
To sum up, only Study 1 which was an exploratory study, showed an association not fully robust across age and screen time measure. Our other findings suggest no relationship between parental phone use and young children's language and emotional development (Experiments 4 and 5, Study 2). In contrast, together with other recent studies (e.g., Konrad et al., 2021), our results contribute to the suggestion that parental screen usage itself may be a moderator in children's development and not a causal factor (Experiment 4). Future investigations should be pursued to explain more precisely why and how parents use electronic devices such as mobile phones during interactions with their children, might directly influence early language and emotional development.

6 Appendices

Appendix A: Experiment 4. Qualtrics questionnaire given to parents.



Appendix B: Study 2. Daily screen time questionnaire given to parents.



Daily Data Record (day 1)

Thank you for taking part in this study of children's language development. Please select one answer for each question »

According to your phone (Apple, Android), how long did you spend on your phone today?

00:00

Use the exact value from your phone in Hours & minutes (hh:mm)

Did you and your child spend time together on your phone today - if yes, how long? If not, leave as 00:00.

00:00

Hours & minutes (hh:mm)

Did your child play alone on your phone today - if yes, how long? If not, leave as 00:00.

00:00

Hours & minutes (hh:mm)

During your child's nap time today, how long did you spend on your phone? If none (or your child didn't nap) leave as 00:00.

00:00

Hours & minutes (hh:mm)

How long did your child spend away from you today, with other family members/friends/child carer? If they didn't, leave as 00:00.

00:00

Hours & minutes (hh:mm)

If and when your child spent time away from you, how long did you spend on your phone? If you didn't, leave as 00:00.

00:00

Hours & minutes (hh:mm)

Submit

End of Study Options

Click here to Opt-Out of the study and delete all my data associated with it

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Appendix C: Experiment 5. Screen time questionnaire given to parents

UNIVERSITY OF PLYMOUTH FACULTY OF HEALTH : PSYCHOLOGY

QUESTIONNAIRE FOR PARENTS

Name of Principal Investigators

Professor Caroline Floccia, Delphine Nguyen (PhD student)

Title of Research

Children's screen time and empathy development

Thank you very much for filling this questionnaire. If you have any questions, you can contact us at plymouthbabylab@plymouth.ac.uk

Please, read and answer the following questions.

Name of the parent/caregiver: _____

Name of the child:

Children's screen time

1) On a typical **weekday**, how much time does your child spend watching TV/online streaming/gaming (on any electronic devices) **alone**? Please give an estimate in hours and minutes.

2) On a typical **weekday,** how much time does your child spend watching TV/streaming/gaming (on any electronic devices) **with family member(s)/friend(s)**? Please give an estimate in hours and minutes.

3) On a typical **weekend day**, how much time does your child spend watching TV/online streaming/gaming (on any electronic devices) **alone**? Please give an estimate in hours and minutes.

4) On a typical **weekend day,** how much time does your child spend watching TV/streaming/gaming (on any electronic devices) **with family member(s)/friend(s)**? Please give an estimate in hours and minutes.

Parents' screen time

1) In a typical **weekday,** according to your phone, how much time (in total) do you spend using your mobile phone?

2) If applicable, in a typical **weekday,** how long do you spend on your phone **when your child is around you**?

3) In a typical **weekend day**, according to your phone, how much time (in total) do you spend using your mobile phone?

4) If applicable, in a typical **weekend day**, how long do you spend on your phone **when your child is around you**?

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