

Point, Walk, Talk: Links Between Three Early Milestones, From Observation and Parental Report

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Around their first birthdays, infants begin to point, walk, and talk. These abilities are appreciable both by researchers with strictly standardized criteria and caregivers with more relaxed notions of what each of these skills entails. Here, we compare the onsets of these skills and links among them across two data collection methods: observation and parental report. We examine pointing, walking, and talking in a sample of 44 infants studied longitudinally from 6 to 18 months. In this sample, links between pointing and vocabulary were tighter than those between walking and vocabulary, supporting a unified socio-communicative growth account. Indeed, across several cross-sectional and longitudinal analyses, pointers had larger vocabularies than their nonpointing peers. In contrast to previous work, this did not hold for walkers' versus crawlers' vocabularies in our sample. Comparing across data sources, we find that reported and observed estimates of the growing vocabulary and of age of walk onset were closely correlated, while agreement between parents and researchers on pointing onset and talking onset was weaker. Taken together, these results support a developmental account in which gesture and language are intertwined aspects of early communication and symbolic thinking, whereas the shift from crawling to walking appears indistinct from age in its relation with language. We conclude that pointing, walking, and talking are on similar timelines yet distinct from one another, and discuss methodological and theoretical implications in the context of early development.

Keywords: motor development, pointing, language acquisition, infancy, early milestones

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As infants approach their first birthday, they make observable gains in development across domains. At around 12 months of age, infants achieve three notable milestones: pointing, walking, and talking. These abilities are concrete manifestations of remarkable improvements in social, physical, and linguistic development, readily observed by scientists and parents alike. Despite occurring across seemingly disparate domains, these skills emerge close together in time, leading researchers to posit that there is an interactive relationship among them. The nature of such relationships is critical to theories of

early development because developmental psychology aims to understand and characterize growth and learning both within and across domains.

Particularly robust ties have been found between social and linguistic development. For instance, Booth, McGregor, and Rohlfing (2008) found that gestural cues, especially sociopragmatic cues like pointing, facilitate word learning in toddlers. Supporting this, in a meta-analysis of over 700 children from 25 studies, Colonnese, Stams, Koster, and Nool (2010) found clear

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links between pointing and language, for both comprehension and production of points and words, concurrently and longitudinally. Moreover, pointing delays are diagnostic for language delay; Lücke and colleagues (Lücke, Grimminger, Rohlfing, Liszkowski, & Ritterfeld, 2017a; Lücke, Ritterfeld, Grimminger, Liszkowski, & Rohlfing 2017b) have found that children who use fewer index finger points at 12 months are at greater risk for language delay one year later. Furthermore, pointing is a viable target of intervention: LeBarton, Goldin-Meadow, and Raudenbush (2015) found that infants who were instructed to point more during an intervention subsequently increased their pointing with caregivers and showed increases in vocabulary.

But why are pointing and language interrelated? Theoretical accounts have raised several possibilities, which generally focus on the referential and symbolic nature of pointing (e.g., Werner & Kaplan, 1963) and its capacity to elicit social interaction and/or shared attention with caretakers (e.g., Camaioni, 1997; Kishimoto, Shizawa, Yasuda, Hinobayashi, & Minami, 2007; Tomasello, Carpenter, & Liszkowski, 2007). Under such accounts, infants' points serve as symbols of their interest, and the pointing acts elicit joint attention. In longitudinal studies, a few months after pointing at objects, infants begin to talk about those same objects (Iverson & Goldin-Meadow, 2005). Relatedly, older infants appear to use pointing specifically to elicit labels from caregivers (Lucca & Wilbourn, 2018), presumably to facilitate verbal discussion of the same objects. Such robust relationships between pointing and language led Butterworth (2003) to quip that "pointing is the royal road to language" (p. 9). Thus, previous research provides convergent support for a theoretical view wherein early pointing and talking are intrinsically linked, rather than just simply tending to co-occur.

Although pointing and walking haven't been explored to the same degree as pointing and talking in infancy, a few studies suggest a possible relationship between these skills as well. Clearfield and colleagues found that the transition from crawling to walking (measured longitudinally from 9.5–14 months) is linked to increased social bids, including points (Clearfield, 2011; Clearfield, Osborne, & Mullen, 2008). Similarly, Walle (2016) found that as infants gained experience walking, both infant- and parent-initiated joint engagement (e.g., pointing) increased. Other research finds that locomotor experience may be linked with gaze and point comprehension, though the authors underscore that these results, "although statistically significant, [are] not robust" (Campos et al., 2000, p. 165). More concretely, pointing and walking may be linked through the affordances and opportunities walking facilitates: Because pointing requires a free hand, walkers have more chances than crawlers to point while on the move (cf. Iverson, 2010).

Finally, evidence has been mixed for a potential link between language and motor development. Several early studies highlight a lack of relationship between motor skills like walking onset and communicative skills like early gesture and language (Bates, 1979; Bloom, 1993). In contrast, Iverson (2010) argued that a general maturational account does not explain developmental results across the motor and language domains, highlighting several specific aspects of motor and language development that do appear to be tightly (and perhaps causally) coupled, for example, rhythmic arm movements and reduplicated babble, recognitory gestures and vocabulary, object mouthing and early vocalizations. Relatedly,

beginning to walk appears to qualitatively change the way infants interact with objects and people (Karasik, Tamis-Lemonda, & Adolph, 2011), as well as the way that mothers respond to object-sharing social bids (Karasik, Tamis-Lemonda, & Adolph, 2014). These results and others led Iverson to propose that while motor advances are neither necessary nor sufficient for language development, they are "normally participatory" (Iverson, 2010, p. 255): On that view, motor advances open new vistas and opportunities for social and communicative interaction.

In line with this theoretical position, walking in particular has been argued to confer language benefits in infancy. In recent work, walkers have been reported to have significantly larger vocabularies than crawlers, in both longitudinal and age-held-constant designs (He, Walle, & Campos, 2015; Walle & Campos, 2014), though not in all samples (Walle & Warlaumont, 2015). Just as having a hand free for pointing may give walkers a social advantage, being able to see further, bring themselves and objects to caretakers, and see caretaker's gaze may give walkers an advantage in their vocabulary development as well.

Thus, previous empirical and theoretical work has aimed to test and explain connections between gesture and language, gesture and motor skills, or motor skills and language. However, no work to date has considered pointing, walking, and talking in concert, despite their temporal proximity around the first birthday. Beyond the theoretical views discussed above (i.e., the symbolic and social links between pointing and talking, and the language-learning affordances provided by having free hands and lines of sight in upright posture), it is worth noting that two kinds of views are anecdotally quite prevalent among parents. First, parents may believe that a given child is gifted or delayed across the board. Second, parents may believe that when infants are improving in one domain, they are stagnant in another (i.e., that because their child is improving in language, walking is on hold or vice versa). On one hand, the evidence reviewed already previews the complexities of operationalizing any given ability as monolithic or representative of related skills. On the other hand, certain clinical diagnoses (e.g., Down syndrome, Williams syndrome, cerebral palsy, intellectual disability) highlight the tendency of certain skills to "hang together" across development. The present study lets us examine this range of explanations and connections among this set of early milestones within typical development, cross-sectionally and longitudinally, through parent report and observational data.

Summarily, the primary aims of this work are to examine links between the onset of pointing, walking, and talking and to test the influence of walking and pointing experience on infants' early vocabulary. To address this, we collected both naturalistic observations and parental report for early language, motor, and social skills in the context of a longitudinal study investigating early word learning.

Parental Report Versus Observational Data

Collecting both observational data and parental report lets us not only tackle our primary aim, but also facilitates a methodological contribution: comparing results garnered by these two data sources. While pointing, walking, and talking are overtly detectable behaviors, the standards of evidence used for each vary, even among researchers. For pointing, studies diverge in whether first-

finger extension alone qualifies as a point or if reaches are included (Lüke, Grimminger, et al., 2017a) and whether different underlying intentions are taken into account (e.g., declarative vs. imperative; Colonnese et al., 2010). For walking, criteria differ regarding how many unsupported steps or what distance must be traversed (Corbetta & Bojczyk, 2002; He et al., 2015). For talking, the phonetic, referential, and communicative criteria differ (e.g., what differentiates early words from babble, whether imitations are true words, if a clear referent is necessary or sufficient, how many phonemes need to be adult-like and how to quantify this, etc.; Vihman & McCune, 1994).

Complicating matters further, infants' abilities are often assessed via parental report, either concurrently or retrospectively. Even with standardized concurrent assessments (e.g., The MacArthur-Bates Communicative Development Inventories [CDI]: Words and Gestures; Fenson et al., 1993), administrative practices vary between studies and between labs (Frank, Braginsky, Marchman, & Yurovsky, 2019). Parents may complete the form in different environments (at home vs. in a lab vs. at a museum) and with varying levels of instruction or available clarification from researchers. Some parents may fill out the same questionnaire multiple times while some complete it only once, and questionnaires may or may not be presented as part of a battery of other tests.

Moreover, parents may interpret scientists' intended criteria differently, based on over- or underestimates of their child's precocity. Thus, when certain attainments are measured by researcher observation and others by parental report, biases may emerge unnoticed, even if individual measures are robustly validated. The secondary aim of the present work is to compare observational versus parentally reported measures for each of these three milestones. Researchers rarely have access to both longitudinal observations and parental-report over the same time span, leaving few chances to compare results across data collection methods. We ask whether these different measures tell the same developmental story.

Current Study Motivation and Predictions

Researchers concern themselves with accurately capturing infants' milestones because pointing, walking, and talking are important indicators of progress along a larger developmental trajectory. Intuitively, the emergence of each of these three skills indicates that infants can access new strategies to engage with the world. Pointing lets infants relay to parents that there is a distal object or event that is interesting or desirable, walking lets infants get around effectively with free hands and clear lines of sight, and talking turns the infants into participatory conversational partners. Thus, first points, steps, and words show that infants have gained new, qualitatively different skills from what they had before.

Moreover, once these skills emerge, they persist and improve rapidly (Adolph et al., 2012; Colonnese et al., 2010). For instance, while infants continue to crawl after they have taken their first steps, they frequently practice walking until they can do so efficiently, at which point they switch to walking as their main method of locomotion (Adolph, Robinson, Young, & Gill-Alvarez, 2008). Once they've started pointing and talking, they may not point and talk at *any* or *every* possible opportunity, yet their gesture and word productions increase steadily (Fenson et al., 1993). Indeed, clinicians often use delays in these milestones as indications of a potential need for

follow-up (Cyrułnik, Fee, De Vivo, Goldstein, & Hinton, 2007; Sauer, Levine, & Goldin-Meadow, 2010).

Finally, given the roles these new skills play, clear and concrete notions of the onset of each skill can improve standardized operationalizations across researchers, allowing study results to be more informatively compared. For these reasons, we focus on infants' first demonstrations of their new, emergent abilities in the current work, while also including analyses of how experience with pointing and walking may influence language development.

Based on the previous literature, we have four specific predictions. First, we predict pointers will have larger vocabularies than nonpointers (Colonnese et al., 2010). Second, we predict walkers will have larger vocabularies than crawlers (He et al., 2015; Walle & Campos, 2014). Third, we predict that the links between pointing and language will be stronger than between pointing and motor or motor and language, based on the theoretical accounts and body of empirical data cited above. Finally, we predict that parental and observational measures will be well aligned (via correlation and/or estimated age of onset); while parental reports will likely contain more measurement noise, we anticipate this will be mitigated by the larger samples of data parents have to draw on relative to our observational measures (Fenson et al., 1994; Libertus & Landa, 2013). In sum, we examine whether the first overt markers of improving mobility, social engagement, and linguistic communication are tied to each other, both through the lens of parental report and through recordings of infants' spontaneous behavior.

Method

This study was a secondary analysis on data collected for a project on noun learning in infants acquiring English (reported elsewhere; Bergelson, Amatuni, Dailey, Koorathota, & Tor, 2019; Bergelson & Aslin, 2017). As detailed below, while our reported measures include all word classes, for the observational data, we noted infants' first words of any kind, but only annotated concrete nouns thereafter. It is worth briefly clarifying this noun focus. While nouns are not the only early words that infants produce, they do dominate the early English lexicon (Dale & Fenson, 1996; Frank et al., 2019) and early noun production tracks very closely with overall early vocabulary. Moreover, many nouns in the early input refer to objects, which are also relevant targets of pointing and social bids (e.g., Karasik et al., 2014; Bates, 1979). Thus, while our noun-centric observational dataset constrains the scope of this work, nouns and objects are prevalent in the early skills we investigate here. Unless otherwise stated, our analyses use infants' reported overall vocabulary and their observed noun vocabulary.¹

Participants

Infants were recruited as they approached 6 months of age from a database of families from local hospitals or through Baby Lab outreach via family and friends. Caregivers provided consent on behalf of their infants at an initial lab visit for the

¹ Indeed, as we'd expect, reported noun and overall vocabulary were extremely highly correlated in this dataset, based on the MacArthur-Bates Communication Development Inventory ($r = 0.99$ for productive, $r = 0.99$ for receptive). Correlations of this magnitude help mitigate concerns about the limitations of this approach.

larger yearlong study through a process approved by the University of Rochester Research Subjects Review Board (title: Visual and Linguistic Aspects of Early Word Learning, protocol number: 51231). Given the broader aims of the yearlong study, the project sought to recruit 48 infants over the 8-month enrollment (some of the substudies, not relevant here, split infants into three groups; 16 is the standard n for the relevant analyses in those studies). While 46 participants originally enrolled, two dropped out in the early stages of the project, leaving 44 in the final sample. All infants were full term (40 ± 3 weeks), had no known vision or hearing problems, and heard $\geq 75\%$ spoken English; 75% of mothers had a bachelor's degree or higher, and 95% of infants were White. The families were enrolled in a yearlong study beginning at 6 months, which included monthly audio- and video-recordings in the home (from 6 to 17 months, $n = 12$ of each), as well as in-lab visits every other month (6–18 months, $n = 7$) and monthly surveys (6–18 months, $n = 13$ of each); see Table 1. Participants were compensated \$340 for the yearlong study.

Procedure

Home recordings. Researchers visited infants' homes each month from age 6 to 17 months to video-record infants for one hour. Two cameras (Looxcie 2, Looxcie, Inc., 22 g each) were placed on infants' heads via a hat or headband. One camera was oriented slightly down and the other was oriented slightly up to best capture infants' field of view (verified via Bluetooth with an iPad/iPhone during setup). If infants seemed unlikely to keep the camera gear on, the primary caregiver was asked to wear a camera on a headband as well. A camcorder (Panasonic HC-V100 or Sony HDR-CX240) on a tripod was placed in the corner of the room parents believed they would primarily spend time in during recording. Parents were asked to move this camcorder if they changed rooms. After set-up, experimenters left for one hour.

In addition to the video recordings, audio recordings captured up to 16 hours of infants' language input each month. Parents were given small audio recorders (Language Environment Analysis [LENA]; LENA Foundation, Boulder, CO) and infant vests with

LENA-sized chest pockets. Parents were asked to put the vest with the recorder on their child from when they awoke to when they went to bed (except for naps and baths). Parents were permitted to pause the recorder anytime but were asked to minimize such pauses. These audio recordings were collected on a different day from the video recordings.

Parents could approve data-sharing with other authorized researchers on audio-video release forms collected after each month's home recordings. Released recordings will be available via Databrary and/or Homebank by request. See Table 1 for a summary of the frequency and timespan of lab visits and home recordings. Further details about the methods used to collect and analyze these home recordings have been reported elsewhere (see Bergelson et al., 2019).

Lab visits. In-lab visits every other month included eye-tracking studies of word comprehension (cf. Bergelson & Aslin, 2017). While these word comprehension studies are outside of the scope of the current analysis, the video recordings of children during those studies were examined here along with the home recording data (cf. Bergelson et al., 2019) to estimate the age of point onset, detailed below. During these eye-tracking studies, the child was seated on their caregiver's lap facing a computer screen. Children saw two images on the screen while their caregiver heard a sentence labeling one of the images (e.g., "Do you see the ball?") and repeated the sentence to their child. Although neither parents nor children were instructed to point at the computer screen, this context often elicited spontaneous points from many of the children (31 of 44 or 70.5% of first observed points occurred during these in-lab videos).

Data Aggregation

Observed measures. Research assistants annotated the first observed instances of pointing and walking from infant video-recordings. For pointing, research assistants watched videos from the home visits and the in-lab eyetracking sessions to find the earliest attested instance of pointing, operationalized as index finger extended with communicative intent. We chose to consider a gesture communicative if the infant pointed at something (e.g., a

Table 1
Infant Mean Age (Months;Days) and SD (Days) at Each Home Recording (Audio and Video), Lab Visit, and Reported Measure

Month	Video-recordings	Audio-recordings	In-lab visits	CDI	Motor Questionnaire
6	6;4 (3.3)	6;7 (3.8)	6;2 (3.7)	6;5 (4.2)	6;5 (4)
7	7;2 (2.3)	7;5 (3.1)	NA	7;7 (4.8)	7;7 (4.7)
8	8;3 (2.7)	8;5 (2.5)	8;2 (2.7)	8;5 (4.2)	8;5 (4.4)
9	9;2 (2.7)	9;4 (3.5)	NA	9;6 (4.9)	9;6 (4.9)
10	10;3 (2.7)	10;4 (3.6)	10;2 (2.8)	10;6 (6.6)	10;6 (6.8)
11	11;2 (2.7)	11;5 (2.8)	NA	11;9 (7.6)	11;8 (7.4)
12	12;3 (2.7)	12;5 (2.9)	12;3 (2.2)	12;7 (5.9)	12;8 (6.9)
13	13;2 (2.7)	13;5 (3)	NA	13;10 (7.6)	13;10 (7.5)
14	14;2 (2.6)	14;4 (3)	14;2 (2.7)	14;7 (7.4)	14;7 (7.4)
15	15;2 (2.5)	15;5 (3.3)	NA	15;10 (9)	15;10 (9)
16	16;3 (2.7)	16;4 (3.1)	16;3 (2.8)	16;8 (7.4)	16;8 (7.5)
17	17;2 (3)	17;5 (3.3)	NA	17;9 (7.1)	17;9 (7.1)
18	NA	NA	18;2 (3.7)	18;10 (9.4)	18;9 (9.5)

Note. CDI = MacArthur–Bates Communicative Development Inventory: Words and Gestures; NA = not applicable. In-lab visits only occurred every other month.

toy or a screen) without touching it (e.g., poking the toy or touching their face). We limited points to single-finger extensions, which have been more strongly tied to sociocognitive development and language delay than whole-hand points/reaches (Liszkowski & Tomasello, 2011; Lüke, Grimminger, et al., 2017a). We include demonstrative and imperative points.

For walking, research assistants watched the monthly home video-recordings to find the first instance of infants taking three or more steps unassisted (the same criterion used by the reported walk measure below).² The first month that an infant pointed or walked was counted as the infant's onset date for that skill. A second researcher coded point and walk onset for 10% of children ($n = 4$ randomly chosen infants for each observed measure). Interrater reliability was high, with 87.50% agreement for month of pointing onset (weighted Cohen's $\kappa = 0.90$) and 100% agreement for walking onset ($\kappa = 1$).

Infants' first words were identified in either audio- or video-recordings. In an initial annotation pass for the larger project, trained research assistants annotated each child's first five words in any lexical category and all concrete nouns thereafter (based on the larger goals of the yearlong longitudinal project centered around nouns). To be considered a word, infants' productions had to meet at least two of the following three criteria: (a) the vocalization is an attempted match of the phonetic shape of an adult word, as evaluated by at least two listeners; (b) the word occurs in an appropriate context, for example, naming an object in the room; (c) an adult in the recording confirms the child's word, either through repetition of the intended word or by using the child's utterance to continue a conversation. Annotators in this first pass were instructed to be generous in their interpretation, capturing all possible infant words.

Following the first annotation, a researcher with postgraduate training in phonetics and child language assessed whether each word fit at least two out of three of the wordhood criteria above. When there was doubt, this researcher conferred with a second phonetically-trained child language researcher. 100% of removed words were agreed upon by both researchers.³ Thus, all child productions analyzed below were determined to indeed be words by at least two people using the wordhood criteria above; all data will be available by request for further verification by interested readers.

The first month that contained an annotated word spoken by the infant was used as the observed age of talk onset. While the first five words we annotated could come from any lexical class, all concrete nouns were annotated for all months. These were aggregated into monthly type-counts used as a vocabulary proxy to compare walkers versus nonwalkers and pointers versus nonpointers. See Table S1 in the online supplemental materials for each infant's first word.

Reported measures. Parents completed monthly vocabulary/gesture checklists (CDI: Words and Gestures; Dale & Fenson, 1996) and motor questionnaires (Libertus & Landa, 2013; Walle & Campos, 2014). These were completed in hardcopy or digitally; all surveys after the initial visit came prefilled with the previous months' responses.⁴

Reported point onset was calculated as the first month in which parents selected *sometimes* or *often* (rather than *never*) for the CDI item "points (with arm and index finger extended) at some interesting object or event."

For walk onset, we used the brief locomotor questionnaire used by Walle and Campos (2014), kindly shared by Dr. Eric Walle (Personal communication, December 12, 2014). This questionnaire asked whether the child had begun belly crawling, hands-and-knees crawling, cruising, or walking. The reported walking age was calculated based on parents' answer to "Has your baby started walking without holding onto anything/anyone, for at least three steps at a time?" Parents could indicate yes or no, and if they indicated yes, could further specify an exact date or the part of the month (beginning, middle, end of the month; coded as the 5th, 15th, and 25th, respectively). We used the exact date ($n = 21$) or part of the month ($n = 23$) to round to age in months to maintain consistency across measures for analysis (as we only have month-level granularity for pointing and talking onsets). While we also collected the Early Motor Questionnaire gross motor scale (Libertus & Landa, 2013), which has several questions related to walking (e.g., walking with arms upright, in a line, etc.), the locomotor questionnaire gave us greater precision for walk onset and was therefore used for the present analyses.

For talk onset, we used either the first month that a parent checked off a vocabulary item on the CDI as "understands and says" or selected *sometimes* or *often* (rather than *never*) in response to "Some children like to go around naming or labeling things, as though proud of knowing the names and wanting to show this. How often does your child do this?"—whichever came first. Reported vocabulary counts were aggregated from the CDI (both receptive and productive).

Final dataset. The full aggregated data contained age at point, walk, and talk onsets and monthly vocabulary totals, from both researcher-observed and parent-reported sources. Given the overarching cross-sectional and longitudinal structure, we can assess how many infants were pointers, walkers, and talkers each month. Over the yearlong study, all infants transitioned in these abilities. See Table 2 for mean onset ages of pointing, walking, and talking in our sample.

By design, data collection centered around the date infants turned one month older from 6 months onward, and generally occurred within one week of this date (see Table 1). For analysis, we round to the month the visit or survey was completed for all measures except reported walk, the only measure where parents provided either the exact age or age at a 10-day granularity, as described in the preceding section. Finally, we note that there was a small quantity of missing data across our measures. We were missing one of 528 (0.19%) at-home videos, six of 308 (1.95%) in-lab videos, one of 528 (0.19%) audio recordings, 27 of 572 (4.72%) CDIs, and 27 of 572 (4.72%) motor questionnaires. These missing datapoints were omitted for a given infant for a given month.

² Using a criterion of ≥ 10 rather than three unassisted steps did not change the pattern of results.

³ Unfortunately, the number of words removed during this cleanup was not recorded. However, subsequent phonetic transcription, that is, a level of detail beyond the wordhood assessment analyzed here, was conducted and attained high reliability. See the online supplemental materials for details.

⁴ This methodological decision was made to reduce the paperwork burden for our families. We do not believe it affected the validity of our survey data; our participants' average reported productive vocabulary is in line with WordBank norms (~ 50 th percentile) at each timepoint (Frank, Braginsky, Yurovsky, & Marchman, 2017).

Table 2
Infant Mean Age (Months;Days) and SD (Days) of Pointing, Walking, and Talking

Method	Point	First word (any)	First word (noun)	Walk 10 steps	Walk 3 steps
Reported	10;6 (74.4)	11;0 (68)	11;28 (73.7)	NA	11;28 (58.1)
Observed	11;23 (69.4)	11;18 (55.9)	12;18 (61.5)	13;4 (56.1)	12;30 (56.8)

Note. NA = not applicable.

Results

Analysis Approach and Plan

We used R to generate this article, along with all figures and analyses.⁵ All code is on GitHub.⁶

To begin, we address the question of how much parents and researchers agree about infants' milestone achievement by comparing reported and observed point, walk, and talk onsets, and for overall child vocabulary beyond talk onset. Next, we determine whether the onset of one skill predicts the onset of others by computing zero-order correlations between the age of onset of each milestone. We then address the role that point or walk status may play in vocabulary growth. We examine pointers versus nonpointers and walkers versus crawlers on monthly vocabulary at specific ages, using the annotated noun productions from home recordings as observed vocabulary, and both receptive and productive CDI scores as reported vocabulary. We then model the longitudinal relationships between point and walk status and vocabulary. Finally, we analyze vocabulary as a function of point and/or walk experience while controlling for age.

Shapiro–Wilk tests revealed that many of our measures (all observed milestone ages and reported talk onset) significantly differed from a normal distribution ($ps < .05$); we thus used nonparametric statistics for zero-order correlations (e.g., onset of pointing and talking) and central-tendency comparisons (e.g., age of first point in reported vs. observed data). In these cases, we used Kendall correlations to assess the strength of the relationships and two-sample Wilcoxon's tests to assess whether observed and reported onset ages differed significantly.⁷ We also conducted several multilevel regressions probing the role of age and point/walk onset or experience on vocabulary. Given the high skew in vocabulary (all $ps < .05$ by Shapiro–Wilk test), we use log-vocabulary in these regression models.

Observed Versus Reported Data

First, we examine the relationship between the observed and reported onset of each milestone, via correlations and comparisons of estimated onset of each skill (see Figures 1 and 2). By testing for correlations, we can assess whether parents and researchers agree on the relative timing of a given skill for infants in this sample. By testing for differences in estimated onset, we can see whether parents and researchers both find that skill X begins at age Y across the group.

The correlation between observed and reported point onset was weakly positive and marginally significant (Kendall's $\tau = 0.22$, $p = .07$). Pointing onset was observed later than it was reported ($M_{\text{difference}} = 1.64$ months, $p = .001$, Cohen's $d = 0.59$). The correlation between observed and reported walk onset was strongly positive, and statistically significant ($\tau = 0.82$, $p < .001$).

For walking, as for pointing, observed onset ages were significantly later than reported ones ($M_{\text{difference}} = 0.91$ months, $p < .001$, Cohen's $d = 1.18$). Thus, we find strong evidence that parental report of walk onset was correlated with observational data, weaker evidence for the same effect in pointing data, and a 1- to 2-month lag between when researchers observed these skills and when parents reported them, consistent with a sparsity of observational sampling for these behaviors.

For talk onset, we saw a different pattern in observed versus reported data. Here, parental and research assessment of age at talk onset did not differ statistically ($M_{\text{difference}} = 0.59$ months, $p = .17$, Cohen's $d = 0.22$), however, they also were not correlated ($\tau = 0.14$, $p = .24$). See Figures 1 and 2. That is, reported and observational data gave the same estimated age of talk onset, but this age estimate in each data-type did not correlate across the group.

Given that we continued to measure infants' accruing noun vocabulary, we further calculated correlations between observed and reported noun vocabulary each month beginning at 12 months, just after the average age of reported first noun: 11.76 months. We used an adjusted p -value threshold of $p < .008$, given that we conducted six nonindependent correlation tests (12–17 months). Observed and reported vocabulary correlated significantly for all months after 12 ($ps < .008$); see Figure 3. Note that for this analysis we compared reports and observations of only the nouns in the child's vocabulary, because after a child's first five words, we only recorded their object words.⁸ Thus, beyond infants' very first words, observation and parental report for vocabulary correlated quite robustly.

Pointing, Walking, and Talking Onsets

Before looking at how infants' vocabulary was affected by point or walk status, or their experience with those abilities, we probed for relationships in the timing of the emergence of each skill. That is, we computed zero-order correlations between age of onset for each skill, within each data type (i.e., reported or observed). None of the skill onset ages were correlated (point-

⁵ R (Version 3.5.2; R Core Team, 2017) and the R packages *broom* (Version 0.5.1; Robinson, 2018), *knitr* (Version 1.21; Xie, 2015), *papaja* (Version 0.1.0.9842; Aust & Barth, 2017), and *tidyverse* (Version 1.2.1; Wickham, 2017).

⁶ Please visit <https://github.com/CharlotteMoore927/PointWalkTalkPublic> for access to our GitHub repository.

⁷ For simplicity (and given the lack of agreement on appropriate effect size computation in non-parametric statistics), we include Cohen's d as our measure of effect size, despite occasionally violating its assumptions.

⁸ It is not trivial to match observed words with items on the CDI directly. For instance, *hummus*, *basket*, and *backpack* are all words we observed infants producing that are not on the CDI. Here we simply total the number of attested noun types across observed and reported data each month.

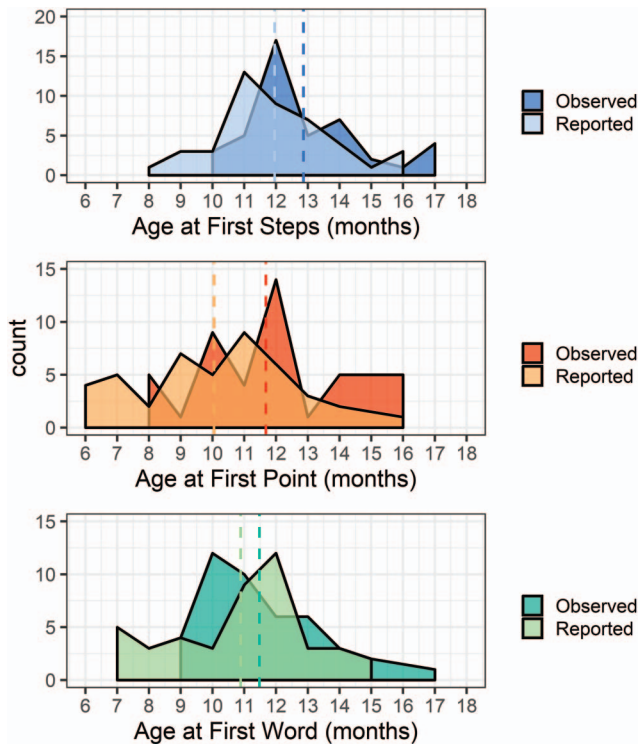


Figure 1. Density plot of observed versus reported age in months at first instance of walking, pointing, and talking (top to bottom). Dashed vertical lines represent mean for observed and reported data. Difference in distribution means is significant for pointing and walking, but not talking; see text for details. See the online article for the color version of this figure.

ing vs. walking, pointing vs. talking; talking vs. walking, in either reported or observed data; all p s > .05). This pattern suggests that a child's achievement of one of these skills does not predict achievement of either of the others; we return to this finding in the discussion.

Comparing Vocabulary Before and After Milestone Onset

We next used an age-held-constant approach to compare vocabularies across children at the same age, when some had achieved a given milestone and some had not (see Table 3 and Figures 4 and 5). We did this separately for each vocabulary measure. For reported measures, we looked at total receptive and productive vocabularies from the CDI. For observed measures, we looked at noun types produced by the child (as explained above, only nouns were annotated in the home recordings after first words were noted).

We first compared infants' vocabularies in each month where 10% to 90% of infants had begun pointing (6–12 months for reported, 8–15 months for observed). Descriptively, pointers had numerically larger vocabularies than nonpointers in 6 out of 7 months (86%) for reported receptive and productive vocabulary, and 6 out of 8 months (75%) in observed productive vocabulary.

We next compared infants' vocabulary via two-sample Wilcoxon test in the month with the most even n between pointers and nonpointers. This was Month 10 for reported data and Month 11 for observed data (see Table 3; Figure 4 also depicts adjacent months for transparency). In the reported data, we found that pointers had larger vocabularies than nonpointers for both productive and receptive vocabulary (productive: $M_{\text{difference}} = 1.75$ words, $p = .03$, Cohen's $d = -0.73$; receptive: $M_{\text{difference}} = 44.62$ words, $p = .02$, Cohen's $d = -0.77$). However, we did not find the same pattern in observed data ($M_{\text{difference}} = 0.57$ words, $p = .26$, Cohen's $d = -0.52$), likely because very few infants were observed to have said a word at this young age; see Figure 4.

Examining walkers versus crawlers with the same approach, we see a different pattern. Again limiting analysis to months where 10% to 90% of the sample was walking (9–14 months for reported, 11–16 months for observed), walkers had numerically larger vocabularies than crawlers in 3 out of 6 months (50%) using reported receptive data, 4 out of 6 months (67%) using reported productive vocabulary, and 2 out of 6 months (33%) using observed productive vocabulary.

Comparing vocabulary in the month where infants were most evenly split between crawlers and walkers (Month 11 for reported

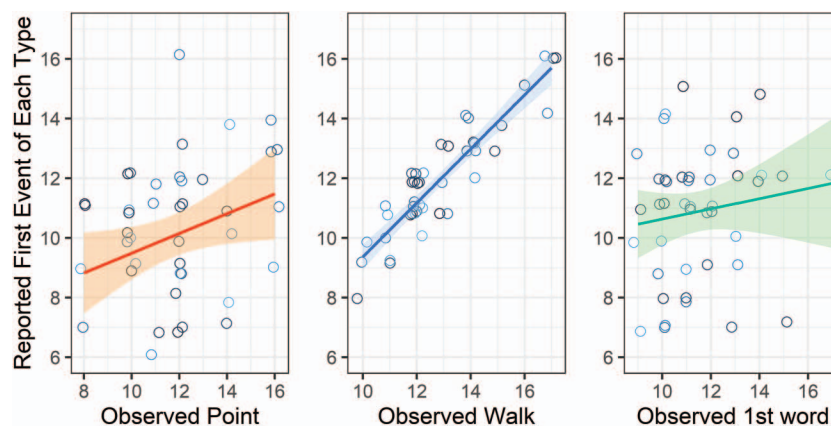


Figure 2. Correlations between observed (x-axis) and reported (y-axis) age in months at first instance of pointing, walking, and talking (left to right). Ribbon represents 95% confidence interval. Each dot represents one infant; dots are jittered slightly for visibility. See the online article for the color version of this figure.

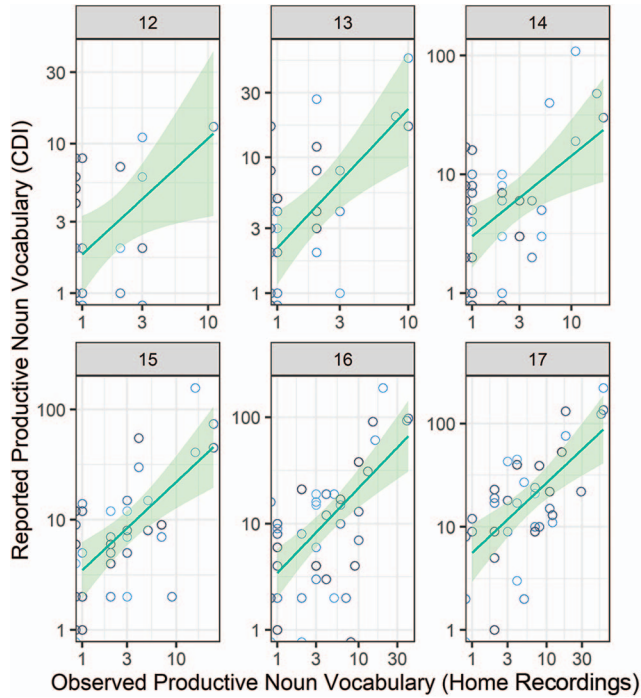


Figure 3. Observed versus reported noun vocabulary by month. Panels show separate months; each point in a panel represents one infant, the dot color corresponds to the same infant in each panel. Correlations in Months 13–17 are significant; see text for details. Axes are log–log scaled; half circles at the edge indicate vocabularies of 0. Ribbon represents 95% confidence interval. CDI = MacArthur–Bates Communicative Development Inventory: Words and Gestures. See the online article for the color version of this figure.

data and Month 12 in the observed data), our findings contrast with results from previous work. Namely, we found no vocabulary advantage for walkers over crawlers for reported or observed vocabulary (reported productive: $M_{\text{difference}} = -0.07$ words, $p = .97$, Cohen's $d = 0.02$; reported receptive: $M_{\text{difference}} = 18.37$ words, $p = .49$, Cohen's $d = -0.26$; observed productive: $M_{\text{difference}} = -0.68$ words, $p = .49$, Cohen's $d = 0.48$). See Figure

5 where we additionally depict adjacent months for transparency. Summarily, in these age-held-constant analyses of point and walk status, there was evidence supporting pointers having larger vocabularies than nonpointers across several measures, but no such evidence for walkers' versus crawlers' vocabularies.

Longitudinal Models of Point/Walk Status

Given our interest in the potentially interacting relationship between pointing and/or walking and vocabulary growth over time, we next sought to test such effects using multilevel models that included point and walk status, controlling for age and multiple measures per infant. Unfortunately, even with log-transformed vocabulary, the residuals of such models were highly structured, limiting interpretive confidence for model comparisons and betas in models including the full dataset. In an effort to strike a middle ground between statistical rigor and our theoretical questions, we opted to constrain the vocabulary range in our models (alongside using the log of vocabulary). For the observed data models, we included only timepoints where infants' productive vocabulary was greater than one word. For the reported data models, we applied this same criterion for the productive vocabulary models, and for the receptive models, only included timepoints where infants' receptive vocabulary was greater than 100 words. These models therefore include, at most, half of our total observations (reported receptive: 261 of 528 observations from 41 of 44 subjects; reported productive: 297 of 528 observations from 43 of 44 subjects; observed productive: 140 of 528 observations from 39 of 44 subjects). This approach improved (but did not fully resolve) concerns with model residuals; for transparency, we provide residual plots in Figure S1 in the online supplemental materials.

For each set of vocabulary data (reported receptive, reported productive, and observed productive), we compared four models—Model 1: vocabulary \sim age in months + (1|subject), Model 2: vocabulary \sim age in months + point status + (1|subject), Model 3: vocabulary \sim age in months + walk status + (1|subject), and Model 4: vocabulary \sim age in months + point status + walk status + (1|subject). Our baseline model (i.e., Model 1) predicted vocabulary with age as a fixed effect and infant as a random

Table 3
Number of Children Who Were Pointing, Walking, and Talking at Each Age (in Months)

Month	Walk 3 steps (observed)	Walk 3 steps (reported)	Point (observed)	Point (reported)
6	44c, 0w	44c, 0w	44np, 0p	40np, 4p
7	44c, 0w	44c, 0w	44np, 0p	35np, 9p
8	44c, 0w	43c, 1w	39np, 5p	33np, 11p
9	44c, 0w	40c, 4w	38np, 6p	26np, 18p
10	41c, 3w	37c, 7w	29np, 15p	21np, 23p ^a
11	36c, 8w	24c, 20w ^a	25np, 19p ^a	12np, 32p
12	19c, 25w ^a	15c, 29w	11np, 33p	6np, 38p
13	14c, 30w	8c, 36w	10np, 34p	3np, 41p
14	7c, 37w	4c, 40w	5np, 39p	1np, 43p
15	5c, 39w	3c, 41w	5np, 39p	1np, 43p
16	4c, 40w	0c, 44w	0np, 44p	0np, 44p
17	0c, 44w	0c, 44w	0np, 44p	0np, 44p

Note. c = crawlers; w = walkers; np = nonpointers; p = pointers.

^a The most even month between children who have and have not achieved each milestone.

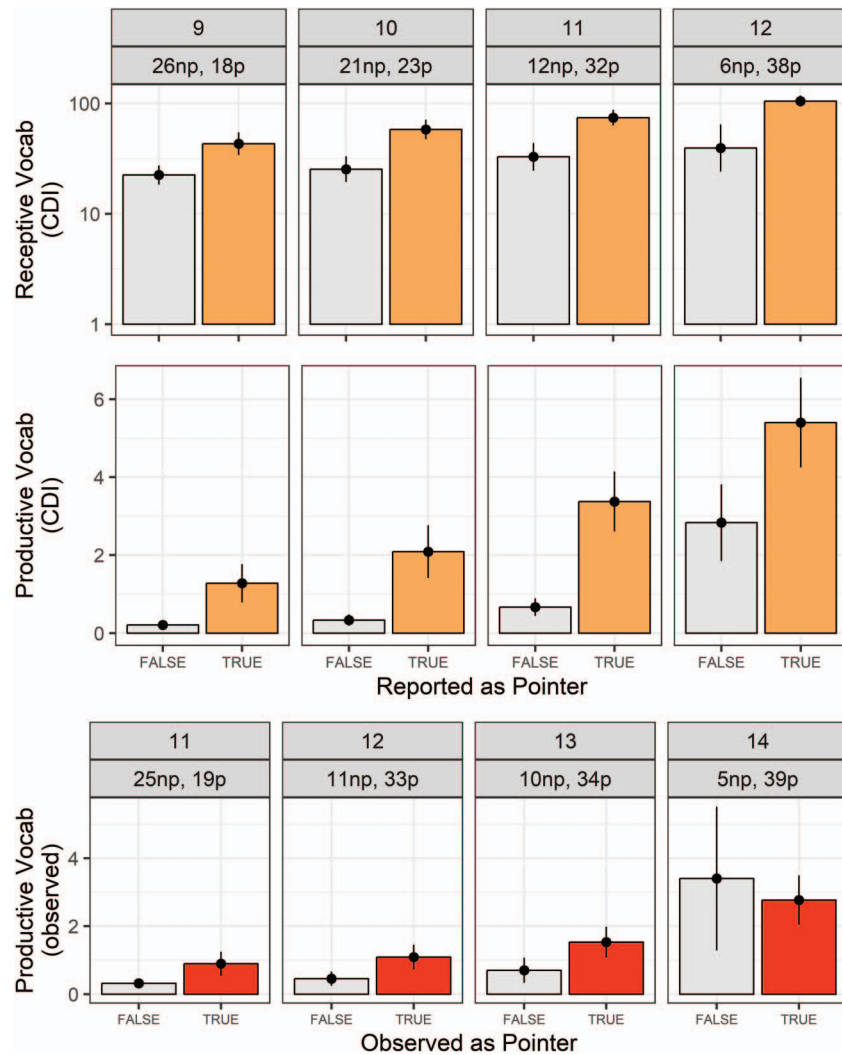


Figure 4. Monthly vocabulary as a function of pointing status. For reported data (top two rows, MacArthur–Bates Communicative Development Inventory: Words and Gestures [CDI] receptive and productive), we show 9–12 months, as these had the most even distribution of nonpointers and pointers (see Table 3). For observed data, we show 11–14 months, because mean, median, and mode productive vocabulary at 9 and 10 months were 0. Top label of panel indicates month, bottom label indicates number of nonpointers (np) and pointers (p); for example, top left: at 9 months, there are 26 np and 18 p reported). y-Axis is log scaled. The third row depicts observed data (i.e., number of child-produced noun-types). Gray (Leftmost) bars in each pair indicate np; colored (right) bars indicate p. Error bars show standard error. See the online article for the color version of this figure.

effect, with Models 2–4 adding point status, walk status, and both. We included age as a fixed effect to test whether pointing and walking status improve the models above and beyond the (expectedly strong) effect of age. We included a random effect of infant due to the longitudinal nature of our study, that is, our multiple measures within participant. See the [online supplemental materials](#) for regression table outputs of the best-fitting model for each set.

For reported receptive vocabulary, adding point status improved model fit over the baseline ($\chi^2 = 11.00, p = .001$). Point status also improved model fit after accounting for walk status ($\chi^2 = 11.05, p = .001$). Adding walk status did not improve model fit over baseline ($\chi^2 = 0.36, p = .550$) or after including

point status ($\chi^2 = 0.41, p = .52$). The best model by model comparison was Model 2, that is, the model that predicted reported receptive vocabulary with age and point status (marginal- $R^2 = 0.44$). See Table S2 in the online supplemental materials.

For reported productive vocabulary, the pattern was the same as for receptive, but weaker. That is, adding pointing status marginally improved fit over baseline ($\chi^2 = 3.23, p = .07$) and after accounting for walk status ($\chi^2 = 3.23, p = .07$). Adding walking status did not improve model fit over baseline ($\chi^2 = 0.04, p = .85$) or after accounting for point status ($\chi^2 = 0.05, p = .83$). Thus, for productive as for receptive vocabulary, adding walk status did not improve model fit; adding point status resulted in a marginally

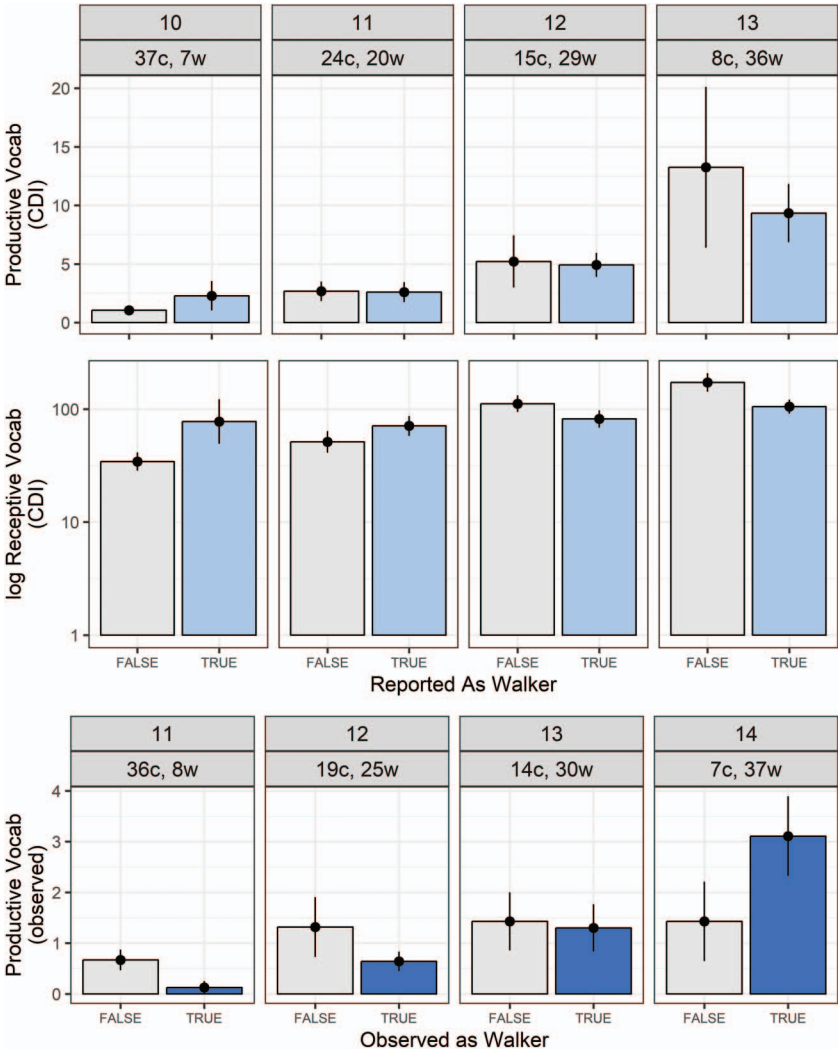


Figure 5. Monthly vocabulary as a function of walking status for the 4 months with the most even crawler/walker distribution (see Table 3). Top label of panel indicates month, bottom label indicates number of crawlers (c) and walkers (w); for example, top left: at 10 months, there are 37 c and 7 w reported. The first two rows depict reported data (MacArthur–Bates Communicative Development Inventory: Words and Gestures [CDI] receptive, CDI productive). y-Axis is log scaled. The third row depicts observed data (i.e., number of child-produced noun-types). Gray (leftmost) bars in each pair indicate c; colored (right) bars indicate w. Error bars show standard error. See the online article for the color version of this figure.

better model than baseline by model comparison (marginal- $R^2 = 0.44$). See Table S3 in the online supplemental materials.

For observed productive vocabulary, neither point status ($\chi^2 = 0.03$, $p = .86$) nor walk status ($\chi^2 = 0.09$, $p = .76$) improved model fit over baseline. Similarly, adding point status after walk status ($\chi^2 = 0.03$, $p = .87$) or vice versa ($\chi^2 = 0.09$, $p = .77$) did not improve model fit. Thus for the observed data, the baseline model, which included only age as a fixed effect and infant as a random effect, provided the best fit via model comparison (marginal- $R^2 = 0.25$). See Table S4 in the online supplemental materials. Summarily, models predicting vocabulary as a function of age, point status, and walk status found moderate evidence in support of an effect of point status alongside age, but no support for an analogous effect of walk status.

Models of Pointing and Walking Experience

Our analysis of the role of pointing and walking experience on language development (as opposed to the onset of the skills) used an age-held-constant approach. That is, we ran three models predicting the natural logarithm of vocabulary at 17 months (i.e., the last month of home recordings) as a function of the number of months each child has been pointing and/or walking. Similar to the preceding section, these models were Model 1: vocabulary \sim point status, Model 2: vocabulary \sim walk status, and Model 3: vocabulary \sim point status + walk status. Models were run separately for reported receptive, reported productive, and observed productive vocabularies. By 17 months, all children had begun pointing and walking. Visual inspection found all models' residuals to be nor-

mal and unstructured (see Figure S2 in the online supplemental materials). One subject was removed from the reported receptive models due to undue influence.

For reported receptive vocabulary, pointing experience alone explains 16.64% of the variance in vocabulary ($\text{adjusted-}R^2 = 0.14, p = .012$), while walking experience alone explains virtually none ($\text{adjusted-}R^2 = 0.02, p = .19$). When adding both point and walk experience, the model accounts for 18.72% of the variance in receptive vocabulary ($\text{adjusted-}R^2 = 0.14, p = .029$). In this model, point experience is a significant predictor ($\beta_{\text{point-exp}} = 0.07, p = .022$), but walk experience is not ($\beta_{\text{walk-exp}} = 0.03, p = .357$). The model with both point and walk experience is not a significant improvement over a model with only point experience ($\chi^2 = 0.87, p = .357$). See Table S5 in the online supplemental materials. Thus, the best model for predicting receptive vocabulary includes only point experience (and not walk experience) as a predictor. See Figure 6.

For reported productive vocabulary, as in the preceding set of longitudinal status models, the results showed the same pattern as for reported receptive vocabulary, but were weaker. The model

with point experience alone accounted for a marginally significant 8.05% of the variance in productive vocabulary ($\text{adjusted-}R^2 = 0.05, p = .084$). Again, walk experience did not explain significant variance in vocabulary ($\text{adjusted-}R^2 = -0.01, p = .39$). Finally, the model with *both* pointing and walking experience did not account for significant variance overall ($\text{adjusted-}R^2 = 0.04, p = .188$), and neither point experience ($\beta_{\text{point-exp}} = 0.14, p = .108$) nor walk experience ($\beta_{\text{walk-exp}} = 0.06, p = .530$) were significant predictors. See Table S6 in the online supplemental materials. Thus, as for receptive vocabulary, adding walk status did not improve model fit; point status provided a marginally significant improvement in fit. See Figure 6.

Finally, for observed productive vocabulary, neither point experience ($\text{adjusted-}R^2 = 0.00, p = .337$) nor walk experience ($\text{adjusted-}R^2 = -0.01, p = .483$), nor both together ($\text{adjusted-}R^2 = -0.02, p = .565$) explained significant variance in vocabulary. See Table S7 in the online supplemental materials. Thus, as in the preceding set of longitudinal skill onset models, neither point nor walk experience were a good fit to observed productive vocabulary. See Figure 6. Summarily, models predicting 17-month vocabulary as a function of point and walk experience found moderate evidence in support of an effect of point experience (particularly for reported vocabulary), but no support for an analogous effect of walk experience.

Discussion

The results above provide several key findings in answer to our two questions: the nature of the links between pointing, walking, and talking and the degree to which parental report and lab-observed data for these milestones are aligned. We found that alignment between parental report and observational data varied by skill, with strongest correlations for walking and for estimates of productive vocabulary at 11–17 months. We found that the age of acquisition of one milestone did not directly predict the age the others came online. In both age-held-constant and longitudinal analyses of skill status and experience, we found relatively robust evidence that pointing was tied to higher vocabulary. In contrast, we did not find evidence that vocabulary was modulated by walk status or experience in this sample.

Reframing these results in relation to our initial four specific predictions: (a) we did find that pointers generally had larger vocabularies than nonpointers, (b) we did not find that walkers had larger vocabularies than crawlers, (c) we did find stronger relationships between pointing and talking than between the other skills, and (d) we found reasonable alignment between parental report and observational data.

Our results point to the separable and interconnected nature of infants' growing social, language, and motor skills. For instance, the lack of zero-order correlations within either reported or observed data is consistent with an account that each ability first emerges independently and unsystematically in relation to the other two. Of course, it is possible we were underpowered to see such effects, though this would suggest that they are relatively small compared to the other effects we measured. Taken at face value, the lack of such correlations is inconsistent with either a simple "all or nothing" account, where an advance in one area percolates to either of the others monotonically or a "when skill *X* improves, skill *Y* is delayed" account. That is, based on the current

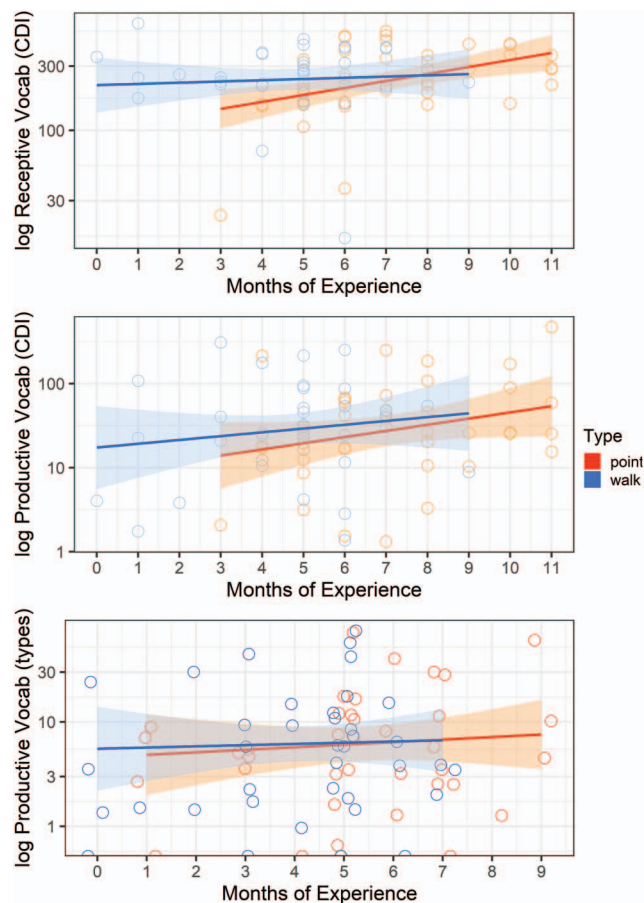


Figure 6. Infants' 17-month vocabulary as a function of months of experience pointing and walking. Ribbon represents 95% confidence interval. Point experience was a significant predictor of reported receptive vocabulary (red [rightmost] line, top panel). CDI = MacArthur–Bates Communicative Development Inventory: Words and Gestures. See the online article for the color version of this figure.

results, knowing that a child has begun to point does not improve predictions about when walking or talking will begin.

However, beyond age-of-onset, our results did suggest relationships between these milestones. In particular, we tested the potential roles of point and walk experience on vocabulary, above and beyond the obvious role of age (which itself is closely tied with language experience and input). We found that pointing predicted vocabulary across several analyses; however, we did not see a relationship between walking and talking.

Vocabulary and Pointing

We found that social and linguistic development were linked in our sample. In months where infants were split between pointers and nonpointers, pointers showed a vocabulary advantage 75–86% of the time. The “most evenly split month” comparison found a statistically robust difference in the reported receptive and productive vocabulary, although not in the observed vocabulary (see Table 3). This may be due to a floor effect. While observed word counts were greater for pointers than nonpointers in 6 out of 8 months, our observed word counts were relatively low at the most evenly split 11-month mark for observed vocabulary ($M = 0.57$ words, $SD = 1.13$, range = 0–5), even relative to the most evenly split month for reported data, occurring 1 month earlier at 10 months ($M = 1.25$ words, $SD = 2.53$, range = 0–10). It is difficult to detect a vocabulary difference in this limited range. Indeed, previous work suggests that gesture continues to benefit vocabulary growth even after the onset of pointing; pointing at an object usually precedes first production of its label by 3 months (Goldin-Meadow, Goodrich, Sauer, & Iverson, 2007).

We saw similar patterns in both our models of vocabulary as a function of age and skill status, and vocabulary as a function of skill experience. Pointing predicted reported receptive vocabulary above and beyond the effect of age alone, and above the effects of age and walking status. The analogous results were marginally significant for reported productive vocabulary, but not for observed productive vocabulary, perhaps again due to more limited observed data. By and large, our results for pointing and vocabulary are thus consistent with previous work finding a close link between pointing and vocabulary (Colonnese et al., 2010), particularly for the reported data.

Our results support a larger literature suggesting that these communicative skills hang together, perhaps as milestones along the same developmental path. Pointing is inherently a communicative and symbolic act, and it may be that children use these same underlying abilities when they begin pointing and talking to communicate with their caretakers. Supporting this view, pointing has been linked directly to new word learning not only over months in the home (Goldin-Meadow et al., 2007) but over a single lab-session as well: 18-month-olds who point at objects learn their labels more readily (Lucca & Wilbourn, 2018). Our work also supports the idea that pointing and language are tied due to the symbolic reference enabled by both pointing and talking, and the ability of each to garner sociocommunicative interaction (e.g., labeling and shared gaze). That is, on our take, pointing is a viable mechanism by which infants may effectively communicate with—and solicit desired linguistic input from—caregivers.

Vocabulary and Walking

In contrast to pointers, walkers only had larger vocabularies than crawlers in 33–66% of months where infants were split between walkers or crawlers, across measurements. Moreover, there were no significant differences in crawlers’ and walkers’ observed or reported vocabularies in the most evenly split months. While this may also be due a floor effect, as described for observed vocabulary and pointing above, the vocabulary range here is wider, especially for observed data (where the infants are a month older), making this possibility somewhat less likely (reported: $M = 2.64$ words, $SD = 3.90$, range = 0–16; observed: $M = 0.93$ words, $SD = 1.84$, range = 0–11).

Our modeling work showed very clearly that walkers did not have a vocabulary advantage in our sample. In our longitudinal analyses, we find no evidence that walking status contributed to vocabulary above and beyond the effect of age in any of our models. Furthermore, we did not find that walking experience predicted vocabulary.

Although the onset of walking changes the way infants interact with their world and the way caretakers interact with them (Karasik et al., 2011, 2014), our results do not suggest that walking and talking are strongly linked. This stands in contrast to previous studies, which have found evidence that language skills benefit from walking experience (He et al., 2015; Walle & Campos, 2014). It is not entirely clear why we failed to replicate these previous results, which find a relationship between walking and vocabulary. One possibility is that while the sample size and measures across our studies were quite similar, some of the microvariations across our methods led to these discordant patterns. For instance, parents in our sample filled out the CDI and motor survey monthly, but started from the previous month’s answers rather than from scratch (to lessen their paperwork burden); in longitudinal analyses in Walle and Campos (2014), parents completed the forms every two weeks, and in their age-held-constant study used the CDI-short form. Moreover, the differences between our findings and those of Walle and Campos (2014) may reflect the differences in the larger goals of each study. Parents may be responsive to differences in their infants’ development depending on what they think researchers are interested in assessing. Parents in our study knew they were part of a language learning study, while those in the work by Walle and colleagues may have known they were participating in a motor development study, potentially biasing parental report differentially in each case. While keeping in mind these methodological differences, our results suggest that the link between language and motor development may not be as robust as previously thought.⁹

Relationship Between Observed and Reported Data

Our second aim was to investigate the relationship between parent-reported and researcher-observed onsets of pointing, walking, and talking. We found both elements of alignment and divergences across the results garnered by these methods. In principle, divergences could stem from (at least) two sources: different

⁹ In an effort to increase comparability, we have endeavored to replicate models and figures from Walle and Campos (2014) directly in our online supplemental materials.

standards of evidence and different access to relevant data, that is, different sampling rates in both capturing a skill's onset and observing or reporting it. These sources of divergence were likely both at play for all three skills we investigated.

For pointing, we found a marginal correlation between observed and reported milestone data, with observed data placing point onset later than reported data by an average of 1.6 months. Here, sampling rate likely played an outsized role. The only way for our measures to match parental report would be for our videos to catch the same initial pointing instances that parents did, which is unlikely given our one hour of home video each month and ~10 minutes of in-lab video every other month. Furthermore, pointing is a quick and variable hand movement, with different underlying motives (Colonnese et al., 2010). It may not carry the same "Big Firsts" saliency with parents that walking and talking do, perhaps influencing parents' memories of this event. Finally, because pointing (like talking) was only reported at the beginning of each month when parents completed the CDI (rather than the "start date" we received for walking), we were unable to be more precise about onset timing.

Of our three milestones, reported and observed data correlated most strongly for the onset of walking. We found a strong positive correlation between our observational assessment of each child's walk onset and each parent's assessment of their own child's walk onset, suggesting convergence in these methods of estimation, and thus likely overlap in the markers used to evaluate this skill. Walking is a relatively unmistakable gross motor skill. This contrasts with talking and pointing, which have huge variability in their clarity and intentionality. While the correlation between observed and reported walking was strong, the observed estimated age lagged behind the parent-reported age of walk onset by one month. We think this divergence is most likely attributable to sampling rate differences; our hour-long monthly video was dwarfed by parents' hundreds of hours of awake time with their child each month.

Talking onset showed an unexpected pattern. While the age of talk onset in reported and observed data did not differ significantly, they were also not correlated on a per-child basis. This is likely due to the difficulty both parents and researchers have in determining whether a child produced a word (rather than babble; Vihman & McCune, 1994). Moreover, while our researcher-observed data adhered to concrete and specific criteria for determining first word onset, parents likely had more idiosyncratic standards. Furthermore, while the daylong recordings add another ~16 hours per month in which to sample word production (beyond the roughly hour-long videos used for all other milestones), they still constitute a much smaller slice of a child's life than caretakers' experience. When considering these differences in standards and sampling between researchers and parents, the lack of correlation for first words is not surprising. Encouragingly, reported and observed data showed an increasingly strong positive correlation as children began producing more words, suggesting that both parents and researchers may improve at word identification as their evidence base grows and as children become more able talkers (see Figure 3).

A comparison of two common data collection methods—parent report and researcher observation—within a single sample is an important methodological contribution for developmental researchers. Parent-reported measures and researcher-observed data

each have their own strengths and limitations which must be carefully considered. On one hand, researchers can apply stringent, standardized criteria to determine whether a child is pointing, walking, or talking, and are often able to assess dozens of children at the same age or stage of development. However, collecting and scoring observational data is a labor-intensive enterprise. In contrast, parents have vast troves of experience with their child and are able to provide this information to researchers relatively easily, but typically have somewhat subjective views of their own child. The present results demonstrate that these two data collection methods can offer complementary information. Our results suggest that parents may be particularly good reporters of gross motor skills and of vocabulary beyond the first word. Gestures or vocal productions that are harder to operationalize (like points and words) may be more of a challenge for parents (or researchers) to report veridically, though frequent querying would likely improve precision.

Given our focus on vocabulary as an outcome, a final note is warranted about infants' very first words. In our analyses of the growing vocabularies of these 44 infants from 6 through 17 months, we found that 20% of children (9 of 44) in this sample went more than 1 month in between their first observed word and their next observed word. In the reported data, 25% of children (11 of 44) maintained a vocabulary of one word across at least 2 months. While this may intersect with the sampling issues raised above, it also converges with previous research and underscores that vocabulary growth can be slow to start (Dale & Fenson, 1996). Speculatively, we predict that models using a higher talk-onset threshold (e.g., age when five or 10 words are produced) will predict subsequent language skills better than models using infants' age when they say their first word. Further exploration of the dynamics of early word production is likely to be a fruitful avenue for future research.

Conclusions

It is no surprise that questions exploring the provenance of these early milestones and the relationships among them make up such a robust literature. Within weeks of their first birthdays, infants quickly acquire new skills across gesture, motor, and linguistic development, which seems too serendipitous to be mere coincidence. Infants' first points, steps, and words are of interest to parents and researchers alike, and our results suggest that these data-sources provide both convergent and complementary information. For both the onset of walking and for the size of the early productive lexicon, parental report and researchers' observations align. The pragmatic developmentalist taking these results at face value may feel more confident eliciting such measures from parents directly, given the relative simplicity of parental report over the collection and scoring of observational data. For pointing and very first word onset, however, convergence across these sources is more limited, at least with the sample size and methods applied here. Notably, the relative reliability of parental report and observational data in populations beyond the limited sample studied here is an important future step for a fuller and more appropriately representative understanding of human development.

We further find that despite the temporally bundled emergence of points, words, and steps, these milestones do not appear to reflect unidimensional improvement across all domains. Our re-

sults highlight closer ties between social skills (pointing and talking) than between language and locomotor development. While there are certainly clinical conditions and disorders (e.g., cerebral palsy) in which all three are affected, here we find that within typical development, infants can make gains in one domain independently of precocity or delay in others. Summarizing across the interlocking measures and patterns we find within this single sample, pointing ability appears to contribute to vocabulary more clearly than walking ability does, above and beyond the effects of age. This in turn helps refine our accounts of language learning and development: If social skills predict language skills more robustly than locomotor skills do, early sociocommunicative gestures in particular may be a promising target for larger scale diagnosis and intervention efforts, in both the realms of language deficits and language delays.

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Correction to Willoughby, Wylie, and Little (2019)

In the article "Testing Longitudinal Associations Between Executive Function and Academic Achievement," by Michael T. Willoughby, Amanda C. Wylie, and Michael H. Little (*Developmental Psychology*, 2019, Vol. 55, No. 4, pp. 767–779. <http://dx.doi.org/10.1037/dev0000664>), the authors report a coding error. Specifically, in the ECLS-K dataset, there are two variables that refer to free/reduced-price lunch. An incorrect variable was used to construct the free/reduced price lunch subsample, which resulted in the over-selection of students into the free/reduced-price lunch subsample (i.e., 2,830 were included but only 1,910 should have been). The authors reanalyzed all data using the corrected free/reduced price lunch subsample. While all the numerical values related to the free/reduced-price lunch subsample that were reported in the original text and tables changed, the magnitude of changes were trivial and none of the substantive conclusions made in the article changed. Updated results are available from the first author.

<http://dx.doi.org/10.1037/dev0000785>