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Using Habituation of Looking Time to Assess Mental Processes in Infancy

Lisa M. Oakes
University of California, Davis

Abstract

Habituation of looking time has become the standard method for studying cognitive processes in infancy. This method has a long history and derives from the study of memory and habituation itself. Often, however, it is not clear how researchers make decisions about how to implement habituation as a tool to study processes such as categorization, object representation, and memory. This article describes the challenges for implementing this tool, and describes a set of best practices for its use to study perception and cognition in infancy.

Introduction

Habituation of looking time is a primary tool for assessing mental processes in infancy. It emerged from Fantz's observation (1964) that infants prefer to look at novel compared to familiar stimuli and findings that non-human animals' responses to stimuli decrease, or *habituate*, with repeated stimulation (e.g., Groves & Thompson, 1970). In habituation paradigms infants are repeatedly presented with one (or more) stimulus and their looking time is recorded (other behaviors, such as sucking, Haith, 1966, or heart-rate responses, Horowitz, 1972, can be recorded; the focus here is on looking time, which is the primary measure when habituation is used to study infant cognition). Typically, infants' looking time decreases, or habituates, with repeated exposure to the stimulus, and it increases to novel items. On the basis of Sokolov's classic comparator model, researchers have assumed that this behavior reflected the infant comparing the currently available stimulus with a remembered stimulus: when similarity is detected infants' looking decreases, when differences are detected their looking time increases (see Colombo & Mitchell, in press, for a review). This recovery of interest to novel stimuli typically is referred to as *dishabituation*, although in the animal literature dishabituation refers specifically to renewed interest in the habituation stimulus (see Groves & Thompson, 1970).

Habituation of looking time has become a standard procedure for assessing a broad range of infants' abilities, including memory, sensitivity to feature combinations, and recognition of abstract properties (i.e., categories, facial expression). It is relatively easy to use with infants ranging from newborns to toddlers. Although it does not require a computer, the availability of inexpensive computers and software developed by different labs has made it easy to test infants' responses to a variety of stimuli.

Correspondences should be sent to Lisa M. Oakes, Center for Mind and Brain, University of California, Davis, 267 Cousteau Place, Davis, CA 95618, USA. lmoakes@ucdavis.edu.

Additional resources

Colombo, J., & Mitchell, D. W. (in press). Infant visual habituation. *Neurobiology of learning and memory*.

Schöner, G., & Thelen, E. (2006). Using dynamic field theory to rethink infant habituation. *Psychological Review, 113*, 273-299.

Thomas, H., & Gilmore, R. O. (2004). Habituation assessment in infancy. *Psychological Methods, 9*, 70-92.

Moreover, we know a lot about infants' habituation. Pioneering work revealed that infants look longer at stimuli that are novel (Fagan, 1970; Fantz, 1964; Saayman, Ames, & Moffett, 1964) or more complex (Brennan, Ames, & Moore, 1966; Cohen, 1972; McCall & Kagan, 1967; Thomas, 1965), and that their habituation is influenced by factors such as stimulus complexity, and infant age and gender (Caron & Caron, 1969; Cohen, DeLoache, & Rissman, 1975; Friedman, Nagy, & Carpenter, 1970; Miller, 1972; Wetherford & Cohen, 1973). Models of the *process* of habituation (e.g., Schöner & Thelen, 2006; Sirois & Mareschal, 2004; Thomas & Gilmore, 2004) have shown that apparently irrelevant factors (such as inter-trial intervals, number of trials, how habituation is calculated) can have a profound effect on responding during the post-habituation test.

This “tools of the trade” paper describes key challenges for using habituation and provides a set of recommended best practices for adopting this tool to study infant cognition. The goal is not to provide a comprehensive review of what is known about habituation, but to use this knowledge to help researchers design experiments. Note although some of the issues are common across procedures, the focus here is on *habituation*, or presenting stimuli until infants' looking reaches some criterion, not on *familiarization*, or presenting all infants with a fixed number of familiarization trials regardless of changes in attention (e.g., Kovack-Lesh, Horst, & Oakes, 2008; Quinn, Yahr, Kuhn, Slater, & Pascalis, 2002).

Challenges for using habituation

Before describing the recommended best practices, three challenges facing researchers using this tool are discussed.

Challenge 1: Infants have preferences for some stimuli

Infants not only look longer at novel than at familiar stimuli (Cohen, Gelber, & Lazar, 1971), they also have preferences for features of stimuli, such as complexity (Brennan, et al., 1966) and whether the stimuli are static or dynamic (Shaddy & Colombo, 2004). Such preferences can make infants' responding following habituation ambiguous. Consider as an example a study by Quinn et al. (2002; note although this study used a fixed familiarization phase rather than habituation, the preference issues are also relevant to habituation studies). Following familiarization with a series of male faces, 3- to 4-month-old infants with female primary caregivers preferred female faces to male faces. These infants appeared to have learned about male faces during familiarization, and as a result preferred the novel female face. However, another experiment revealed that infants with female primary caregivers preferred female faces *even without familiarization with male faces*. Thus, infants' looking in the first experiment likely was influenced both by infants' a priori preferences for female faces and their learning during familiarization. Accurate conclusions therefore require that researchers take steps to identify, and minimize the influence of, such preferences.

Challenge 2: Infants' dishabituation is not a direct measure of all mental processes

In classic models of habituation, infants' dishabituation is thought to reflect their recognition that the new stimulus differed from the remembered stimulus or stimuli. For example, if infants dishabituate to a blue item following habituation with a red item, we know that they remembered the color, compared the colors, and perceived the differences between the colors, *as long as the two items are identical in every respect except color*.

Conclusions about the mental processes involved in dishabituation are less clear when novel and familiar stimuli differ not only in perceivable, physical features such as color, direction of movement, location, or amount of occlusion, but also in inferred features such as an actor's goal, which object was the causal agent, or the level of physical support. When infants dishabituate to such changes it is clear that they have remembered, perceived, and

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detected physical features of the stimuli associated with these non-perceived features (e.g., the direction an actor's head is pointed, which object moved first, the amount of contact between two objects). But, all you know from habituation is that infants have detected those physical differences; their dishabituation does not directly reveal whether their interpretation of those physical differences is the same as an adult's. Only through careful experimental designs and converging evidence can we draw conclusions that patterns of dishabituation reflect infants' attention to or inferences about such non-visible features.

Challenge 3: Infants' looking reflects competing preferences for novelty and familiarity

Habituation procedures are based on the assumption that infants' looking is influenced by novelty, and that infants look longer during test at novel items (Welch, 1974; Wetherford & Cohen, 1973). In reality, infants' looking reflects competing preferences for novelty and familiarity; when fully familiarized infants prefer novel stimuli, but when not fully familiarized infants prefer *familiar* stimuli (Hunter & Ames, 1988; Hunter, Ross, & Ames, 1982; Roder, Bushnell, & Sasseville, 2000; Rose, Gottfried, Mello-Carmina, & Bridger, 1982). In some experimental designs *any* systematic preference is interpretable (e.g., Bahrick & Pickens, 1995). Because infants can prefer either familiar or novel stimuli, preferences should be evaluated using 2-tailed tests of significance.

Competing novelty and familiarity preferences, however, can create ambiguities for interpreting infants' responses in many experimental designs. Consider, for example, the commonly used design in which infants' responses are tested to multiple novel stimuli. This design is powerful because we can examine how the same infants respond to different kinds of novelty (e.g., changes in appearance, relations among features, some abstract property). But responding to these tests is ambiguous if it is not clear that infants are fully habituated (and thus are likely preferring novelty). Consider as an example a study by Casasola (2005). Eighteen-month-old infants were habituated to several events in which different objects depicted a particular spatial relation (e.g., an animal, a car, a candle, and a peg all being placed *on* a container) while “Look, it goes on” was spoken. They then were tested with (1) a familiar object in a new spatial relation (e.g., the peg was placed *in* the container), (2) a novel object depicted in the familiar spatial relation (e.g., a cup was placed *on* the container), and (3) a novel object in a new spatial relation. Infants dishabituated to (1) but not (2). If infants preferred *novelty*, this pattern indicates that they detected the new spatial relation but not the new object. But, if infants instead preferred *familiarity*, then the observed pattern actually indicates that infants attended to the familiar object and a failed to attend to the spatial relation. Fortunately, Casasola (2005) included in the analysis only infants who met the habituation criterion, and she reported that as a group infants exhibited both low levels of looking to a completely familiar event presented after the criterion was met *and* exhibited high levels of looking to the completely novel event. Thus, in this case, we have confidence that infants were responding to novelty, and the first interpretation is correct.

Such analyses are not typically included in published reports (indeed, often infants are not given a post-criterion test with a familiar item or a test with a completely novel stimulus). An examination of 37 papers published in 2008 and 2009 using habituation to assess some aspect of infant mental processing revealed that typically there is not sufficient information provided to allow us to be certain that recovery during test reflects a *novelty* preference (these 37 papers were used to draw many of the conclusions about common practices described here; a list of the papers is available from the author). A challenge for researchers is to provide sufficient detail for readers to determine whether the pattern of responding during test reflects a preference for novelty.

Best practices

What follows are a recommended set of best practices for using habituation. Because habituation is a dynamic process determined by a large number of variables, it is impossible to identify a set of rules that is appropriate in every experiment. In addition, although these are the best practices for using *habituation*, some of the issues are relevant for fixed familiarization procedures. Indeed, in considering these practices, some investigators may determine that a fixed familiarization procedure is more appropriate for their research question.

Best practice 1. Minimize the effect of a priori preferences

Adopting one or more of the following strategies will minimize the effect of a priori preferences on infants' responding during test.

1. Do not use as tests stimuli for which infant have a priori preferences— Although robust a priori preferences for one stimulus category over another can suggest sensitivity to some stimulus differences, they also mean that infants' responding to those stimuli following habituation is influenced both by habituation *and* those a priori preferences. The best practice, therefore, is to use stimuli that are equally compelling to infants. However, this practice requires first identifying whether preferences exist, and it is not immediately obvious *how* to assess such preferences. To be efficient, often a separate group of infants are presented with a large number of stimuli over multiple trials. In the main habituation procedure, in contrast, infants typically are presented with one or two novel and familiar stimuli on a smaller number of test trials. Thus, the preferences of one group of infants, obtained in the context of multiple stimuli on many trials, are used to interpret the responding of different group of infants to a smaller number of stimuli on a smaller number of trials. Because infants' preferences are almost certainly context-dependent, preferences assessed with a large number of different stimuli over several trials may not generalize to preferences assessed with a smaller number of stimuli on a smaller number of trials.

To be clear, *any* observed preferences are useful for identifying potential problems for interpreting the data from habituation procedures. Researchers simply must be cautious about the kinds of comparisons they make between preferences obtained in very different contexts. Clearly, direct statistical comparisons should not be made of infants' responding in the two contexts just described. The most unambiguous way to evaluate the effect of preferences is to present the identical test phase (i.e., same number and duration of trials, same trial orders) to one group of infants who receives the habituation phase and a second group of infants who does not (see, e.g., Hayden, Bhatt, & Quinn, 2006). Then researchers can directly compare how infants respond to the tests with and without habituation.

2. Use the same stimuli during both habituation and test (see also Bogartz, Shinskey, & Speaker, 1997, "event by set" design)—Sometimes it is impractical to completely eliminate stimuli based on a priori preferences. Moreover, preferences themselves might reveal insight into the processes under study. For example, Quinn et al. (2002) observed that infants familiarized infants with male faces showed a novelty preference for female faces, but infants familiarized with female face looked equally at the novel male and novel female faces. Quinn et al. uncovered the asymmetry in infants responding to male and female faces only by using both sets of faces during familiarization. In this case, the preferences were not eliminated, but testing both conditions allowed questions to be answered about the preferences themselves, rather than the preferences causing the researchers to draw inaccurate conclusions. The point is that the same stimuli should be used as habituation for some infants and tests for other infants.

3. Use a variety of habituation and test stimuli—Using many stimuli during habituation and test across infants can reduce the influence of a priori preferences. A larger stimulus set also increases the generalizability of the findings. In addition, a priori preferences might be revealed by differences between infants who were habituated to different stimuli (see, e.g., Bahrick & Newell, 2008).

Best practice 2. Maximize the number of infants who actually habituate

Typically, in habituation studies infants are presented with a stimulus or set of stimuli until the average looking on some block of trials decreases to some pre-specified criterion (e.g., 50% of what it was on first block). Habituation is calculated by averaging infants' looking on blocks of trials and comparing those averages as the session progresses. Although the assumptions are that all infants who reach a criterion have actually habituated (Bornstein, 1985) and are at the same level of processing (Colombo & Mitchell, 1990), infants' looking at any moment is actually influenced by a number of variables, including their processing of the stimuli, detection of novelty, internal events (e.g., a burp), a sound in the hallway, and so on. Because looking time does not solely reflect habituation and dishabituation (see, e.g., Richards & Casey, 1992, for a discussions of how looking relates to different aspects of attention), many factors contribute to why infants' looking is relatively low or high on any given trial. Therefore some infants will meet (or fail to meet) the habituation criterion by chance (Dannemiller, 1984; Thomas & Gilmore, 2004).

This is a particular problem because habituation criteria vary across studies, and thus likely differ in how many infants included in the final analyses did not actually habituate. Including different numbers of infants who actually habituated in the final analyses will change the contribution of the competing influences of novelty and familiarity preferences on infants' responding during test. Although using a habituation criterion allows the experimental session to be tailored to each infant's individual style of looking, thus yielding low attrition, it is critical to use a habituation criteria that is neither too lenient (resulting in including many infants who habituated by chance) nor too stringent (resulting in excluding many infants who did habituate). If the research question does not require that infants fully habituate—i.e., responding during test does not require an unambiguous novelty preference but only a systematic preference between two types of stimuli—the best practice may be to use a fixed familiarization period. This is certainly the best practice if all infants will be included in the analysis regardless of whether or not they meet the habituation criterion. However, when habituation is used, it is important to ensure that most infants actually habituate. Adopting the following four recommendations will increase the number of infants who habituate.

1. Use a stringent criterion—Cohen (2004) advocated using a decrease of at least 50% from the initial level of responding. This is the most common practice adopted. Requiring a larger decrement may minimize the effect of regression to the mean on infants' responding (Ashmead & Davis, 1996), but may also mean that infants who did habituation are not included in the final analysis. Even a stringent criterion may not accurately classify all infants as habituators, thus violating the basic assumption that this procedure equates the infants along some dimension (Thomas & Gilmore, 2004). Thus, adopting a stringent criterion is only one way to increase the number of habituators in the final analysis.

2. In general, use a sliding window of 3 trials—The window used to evaluate habituation can be *sliding* or *fixed*, and it can vary in size (i.e., the number of trials). When using a *running average* (or *sliding window average*), a new average is calculated after each trial (i.e., trials 1, 2, and 3 are compared to trials 2, 3, and 4, and so on). Such averages result in short sessions and lower attrition. When using a fixed window average, a new average is calculated after each *N* trials (i.e., trials 1, 2, and 3, are compared to trials 4, 5, and 6, and so on). Such averages ensure that infants have the same exposure to several individual items; for example, each of three stimuli might be presented once in each block of 3 trials and habituation is assessed after trials 6, 9, 12, and so on. Meeting the habituation criterion is more difficult with a fixed window, almost certainly resulting both in fewer infants habituating by chance *and* in more infants who actually *do* habituate not meeting this stringent criterion. Researchers should adopt a fixed window average when it is important that infants have equivalent exposure to different stimuli used during habituation, but it is important to be aware that it is unknown how this procedure affects attrition or the number of infants who actually habituate.

The *size* of the window also is important. Ashmead and Davis (1996) caution that the size of the window when using a running window average can affect accurately identifying habituators. They provide support that a running window of 3 trials is better than running windows of 2 or 4 trials (the optimal window size may be different when using a fixed window average), although they ultimately recommend using their polynomial regression approach that utilizes all the available looking time (rather than just the data in the baseline and criterion windows) to determine whether individual infants have habituated. In general, most published studies use a window of 3 trials, although some use 2 or 4.

3. Use the first window as the *baseline* for habituation—Although most researchers use this baseline, because infants' longest looks often occur after the initial trials (e.g., Peterzell, 1993; see also discussion by Groves & Thompson, 1970) some researchers evaluate infants' habituation using their *peak* level of looking, or the window that represent the longest look (see discussion of “floating point” criterion by Colombo & Mitchell, 1990). Because of other factors that contribute to variations in looking, however, using the peak looking may result in including in the final analysis a larger number of infants who did not actually habituate. It is therefore most conservative to use infants' initial looks as the baseline.

4. Use a sufficient maximum number of trials—Determining the appropriate maximum number of trials is not straightforward. On the basis of a Monte Carlo simulation, Dannemiller concluded that the appropriate maximum is 13 to 15 trials (although this will likely depend on a number of factors including the kind of stimuli, the task, the infants' age, and so on). Increasing the maximum number of trials will increase the number of infants who actually meet the habituation criterion. However, allowing too many trials to meet the habituation criterion may increase the number of infants who habituate by chance (Dannemiller, 1984), and it may increase attrition due to fussiness. Providing too few trials may result in lower attrition, but it will result in more infants failing to meet the criterion.

It is not clear, however, that the maximum number of trials is related to attrition. In the 37 recently published studies described earlier, there was no correlation between the proportion of infants who failed to complete the study due to fussiness and the maximum number of trials (note that it is possible a relation would be observed if both published and unpublished studies could be evaluated). Careful piloting should be conducted to determine the maximum number of trials to ensure that most infants meet the criterion and are included in the final analyses.

Best practice 3. Do not use infants' responding on the criterion trial as the baseline for dishabituation

When using a habituation criterion, looking on the last habituation trial *by design* will be low. As described earlier, factors other than habituation contribute to variations in infants' looking on each trial. If infants' looking on the criterion trial is artificially low, their looking during the criterion trial underestimates their interest to familiar stimulus or stimuli, and their looking on the next trial will be higher simply due to regression to the mean (Bertenthal, Haith, & Campos, 1983; Cohen & Menten, 1981; Dannemiller, 1984). Therefore, comparing infants' looking on the last habituation (i.e., criterion) trial and a novel stimulus presented on the next trial may over-estimate infants' dishabituation.

One solution is to present post-habituation half of the infants with a familiar stimulus and half with a novel stimulus (e.g., Young-Browne, Rosenfeld, & Horowitz, 1977). Thus, the critical test is a between-subjects comparison of post-habituation looking to the novel and familiar stimulus (see also the "partial-lag" design, Bertenthal, et al., 1983). This solution is optimal because infants' responding to the key items will be equally influenced by regression to the mean. However, this design is best suited when the comparison is between one novel and one familiar stimulus; it cannot be easily implemented when testing infants' responses to several novel items.

Another solution is to present all infants with a familiar and novel items post-habituation (e.g., Cohen & Strauss, 1979). In this case, the baseline for dishabituation to the novel stimuli is the post-habituation trial with the familiar item. This design is optimal because it depends on powerful within-subjects comparisons, and infants provide their own baseline for dishabituation while eliminating the dependence on a single trial that may by chance be artificially low. However, it is not clear when the post-criterion familiar item should be presented. If it is presented before the novel items regression to the mean effects will be different for the novel and familiar items. If novel and familiar tests are completely counterbalanced, responding to and processing of the novel stimuli may interfere with responding to the familiar item. Researchers must consider these issues and determine the design that makes the most sense given the question of interest.

Best practice 4. Report analyses only from infants who actually habituated

Non-habituating infants potentially contaminate responding during test because they prefer familiarity rather than novelty (Cohen, 2004). However, in most published studies the results reported include both infants who did and did not meet the habituation criterion. The best practice is to analyze data only from infants who habituated. Although this is the best practice, it is important to remember that excluding non-habituators may reduce the generalizability of the results—the results can be extended only to infants who would have habituated in the maximum number of trials provided. If the number of infants who habituate is small, excluding non-habituators biases the result toward the pattern exhibited by *fast* habituators (Schöner & Thelen, 2006), and fast and slow habituators have been shown to have different responses to tests (Baillargeon, 1987). Designing experiments that ensuring that the largest number of infants possible have habituated is critical. Adopting the following strategies will help researchers be certain the analyses reflect infants' preference for novelty.

1. Analyze only data from infants who met the habituation criterion—Infants who did not meet the habituation likely will have a familiarity preference, thus reducing any overall novelty preference.

2. Report infants' habituation responding—Readers must have confidence that infants are responding to novelty. This requires information about habituation. For example, the number of trials infants required to meet the habituation criterion—and any differences between infants tested in different conditions, of different ages, and so on—helps readers evaluate differences in habituation. Mean levels of looking during the initial and criterion habituation trials (although it is not legitimate or necessary to statistically compare their looking during these trials), as well as comparisons of looking on the criterion trial to a post-criterion baseline and a completely novel test can reassure researchers (and readers) that it is infants' responding to critical tests is appropriately interpreted as a novelty preference.

3. Exclude infants who apparently habituated by chance—A small number of infants habituate and then exhibit extremely long looking (e.g., several standard deviations above the mean) at a post-criterion familiar test, suggesting they met the habituation criterion by chance. Some researchers use infants' looking to this item to identify outliers. Excluding outliers must be done carefully, however. Eliminating only infants who look relatively long to the familiar may introduce a bias toward dishabituation. Of course, because infants' looking to the familiar stimulus is generally low, infants rarely have looking times to the familiar several SD *less* than the mean.

Summary

In summary, habituation of looking time is a powerful tool that can be used to study a wide range of content areas across the entire period of infancy. Researchers who adopt this tool must understand that infants' looking is complexly determined by stimulus novelty and other factors. Unambiguous conclusions from habituation require consideration of the challenges described here and careful experimental design and controls. A set of 4 best practices for implementing the tool is provided to aid researchers in designing experiments. Habituation will continue to be an important tool for aiding researchers make significant contributions to our understanding of perceptual and cognitive development in infancy.

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