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Philosophy and the Mind Sciences

Theory of mind deficits in language delayed deaf subjects?

A reconsideration of evidence from false belief tasks

Endre Begby[a] (endre.begby@gmail.com)

Abstract
Recent studies claim to show that language delayed deaf subjects typically display long-lingering deficits in Theory of Mind (ToM) development, despite suffering no known deficits in other cognitive domains. These claims are supported by experimental evidence indicating that such subjects fare poorly on False Belief (FB) tasks. This paper turns a critical eye on these claims. In particular, I argue that the reported results raise important questions about the status of FB tasks as evidence, and about how such evidence should be weighted against naturalistic observations of subjects engaged in everyday activities requiring complex social coordination. I conclude that these studies give us no decisive reason to believe that language delayed deaf subjects suffer distinctively and symptomatically in the domain of social cognition. To the contrary, the attribution of significant socio-cognitive impairment is potentially stigmatizing and may not help us understand the unique challenges these subjects face or suggest remedial strategies to aid them in overcoming these challenges.

Keywords
Deafness · False belief · Gesture · Sign language · Social cognition · Theory of mind

1 Introductory

Deafness or severe loss of hearing is something that many of us should expect to have to cope with at some stage in our lives.[1] But congenital or early-onset deafness can create a different set of challenges altogether.[2] Among the most significant of these challenges is that deafness disrupts the typical pathway for language acquisition and thereby jeopardizes the child's access to the wealth of social learning that linguistic communication provides.

[a] Simon Fraser University.

[1] According to the WHO, about 1/3 of people over 65 are affected by disabling hearing loss. https://www.who.int/news-room/fact-sheets/detail/deafness-and-hearing-loss

[2] Congenital hearing loss affects 1.33/1000 live births. https://www.ncbi.nlm.nih.gov/pmc/articles/PMC5675031/

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https://doi.org/10.33735/phimisci.2023.10269

Even so, it is important to note that there is nothing intrinsic to human language which dictates that it must pass through the oral-auditory channel. Sign language has long been recognized as a full-blown manifestation of the human language capacity,[3] and adequate early exposure to sign leaves subjects with no discernible disadvantages in meeting cognitive developmental milestones.[4] Perhaps, then, the challenges associated with pre-lingual deafness are not so consequential after all.

In reality, however, things are more complicated. Very few deaf children are born into a pre-existing signing community. Instead, the vast majority of them (90% or even more)[5] are born into hearing families, and it is exceedingly rare for these families to already know sign. For a best-case scenario, then, assume that the child's deafness is diagnosed at an early stage, and that the family undertakes the commitment to learn sign and is able to harness relevant social and institutional resources to facilitate that learning. Even under such optimal circumstances, it must be borne in mind that for adult caregivers, acquiring sign, like acquiring any new and unfamiliar language, is *hard*: developing any level of fluency will take significant time and personal investment. In such cases, the child's early exposure to language is likely to be less voluminous, less fluent, and less spontaneous by orders of magnitude than what hearing children can count on as a matter of course. Placing the child in signing early education centers, etc., may serve to mitigate the problem but cannot act as a full substitute for the volume of fluent linguistic communication that occurs within a typical hearing family.[6]

Just how consequential is this? On the one hand, we know that, with adequate resources, most of these children will eventually acquire full fluency in sign language,[7] even if they do so with an appreciable delay and will only be able to meet the usual developmental signposts at a more advanced age than hearing children do.

However, there is an active strand of research that appears to throw this rather hopeful observation into stark relief. According to this research, even subjects who eventually acquire sign language and who become fully integrated into a language community are likely to suffer significant and long-lasting cognitive deficits,

[3] Though not for as long as one might think: the turning point in the recognition of sign as *language* (as opposed to, say, a largely imagistic communication system) is generally attributed to Stokoe (1960). For an overview, see Goldin-Meadow & Brentari (2017).

[4] See, e.g., Emmorey (2002), Meier (2016).

[5] Mitchell & Karchmer (2004).

[6] Cf. Napier et al. (2007); Lederberg et al. (2013). For a contrasting perspective, see Caselli et al. (2021), which reports typical vocabulary growth in deaf children born to hearing parents in cases where the child's first exposure to Sign occurs before the 6-month mark. Notice, however, that the study concerns vocabulary only (as opposed to, say, grammar), does nothing to specify what it means by "exposure," and, most concerningly perhaps, relies entirely on parent-reported data.

[7] On the other hand, we also know of exceptional situations where this doesn't happen, and the child will come of age without ever developing anything like a full linguistic repertoire (on this, see, e.g., Goldin-Meadow, 2002, 2003). We will address the situation of so-called "homesigners" later in this paper.

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specifically in the domain of social cognition. This is shown by their relatively poor scores on False Belief tasks, the predominant tool for measuring socio-cognitive abilities in the contemporary psychological literature. If this is correct, it is obviously a very serious matter: socio-cognitive deficits of this sort should be expected to significantly impair subjects' ability to navigate our social world, forming rewarding interpersonal relationships, and so on.

In this paper, I turn a critical eye on this claim. I argue that (i) the underlying research lacks internal cogency, and (ii) is broadly inconsistent with what we must presume to know, on independent grounds, about the subjects in question. In particular, we have extensive documentation of these subjects engaged in everyday activities which plainly require the relevant socio-cognitive abilities. So without underplaying the real challenges these subjects face, and without in any way casting doubt on the critical need for early interventions in these situations, I argue that the research gives us no decisive reason to believe that these subjects suffer distinctively and symptomatically in the domain of social cognition. To the contrary, attributing significant socio-cognitive impairment to these subjects is in important ways stigmatizing and does not help us understand the challenges they face or suggest remedial strategies to help them overcome these challenges. Along the way, I also use these reflections as a stepping-stone for thinking more broadly about favorable and unfavorable settings for deploying the False Belief paradigm. Part of our problem, I argue, is our tendency to overvalue a certain kind of experimental evidence without properly considering the broader behavioral indicators that would make the test evidentially relevant.

2 Social cognition and the False Belief paradigm

Human beings think an extraordinary amount about what's going on in other people's minds. We do so not just because we might be worried about our own social standing or because we want to be in a position to predict others' behavior. Rather, we also do so with a view to orienting our own thinking about the world. Seeing how other people act in certain situations can provide genuine information about our shared environment. This, some researchers claim, is a uniquely human ability[8] – indeed, some go so far as to say it is part of what defines us as human.[9]

Until quite recently, it would have been tempting (and very natural) to think of these socio-cognitive abilities simply as a high-grade expression of our "general intelligence" or problem-solving ability. Human beings are smart: it's simply in virtue of being smart that we are able to think explicitly about the hidden mental states of others in this way.

Meanwhile, the history of modern cognitive psychology is to a very large extent the history of the splintering of this notion of general intelligence into a large

[8] See, e.g., Frith (2012, p. 2213); Tomasello (2018, p. 8497).

[9] See, e.g., Baron-Cohen (2000, p. 3).

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variety of specialized *sub-domains* of intelligence, each with its own guiding principles and (perhaps) each subserved by its own specialized neural architecture.[10] Even as a typically developing human being will quickly gain a foothold in all of these domains, and will, as a result, come to develop significant and flexible abilities to reason across them, it is clear that these domains of intelligence operate to a significant degree independently of each other. The strongest evidence for this often comes from studies of selective impairment: people who have lost, or fail to develop, abilities in one particular domain of intelligence, but nonetheless appear to retain (or develop) full functioning in many others.

Social cognition – via the concept of a "Theory of Mind" (i.e., a cognitive capacity specifically dedicated to thinking about other organisms as engaged in perception, reasoning, and deliberation, which in turn causes their outward behavior) – is a relatively recent addition to this trend.[11] The False Belief test was instrumental in bringing about this change.

Here is a brief history of the development of the False Belief paradigm. In 1978, Premack and Woodruff offered experimental evidence in support of the claim that chimpanzees have a Theory of Mind in something like this sense. For instance, chimpanzees appear to be capable of predicting how (human) agents will act to solve certain problems. Premack & Woodruff (1978) thereby concluded that chimpanzees are capable of attributing various complex mental states to other agents, including beliefs and intentions.[12]

In response, several philosophers and cognitive scientists (among them, Daniel Dennett, 1978) pointed out that a more decisive indicator of a Theory of Mind would involve testing the chimpanzee not on its ability predict actions based on full and shared information, but rather on its ability to predict actions prescinding from an agent's *false* beliefs, i.e., whether they would be able to predict how an agent will act in a situation where that agent doesn't possess all the information that the experimental subject possesses. Such a test would be more decisive, because it requires keeping apart two distinct perspectives on the world, my own and that of the agent whose actions I am trying to predict. By contrast, the simpler task that Premack and Woodruff originally investigated could be successfully answered simply by asking, "how would I solve this problem?"[13]

[10] See Spelke (2022) for a detailed overview.

[11] Note that this doesn't yet settle whether, for instance, we should think about ToM in terms of some more expansive notion of "the modularity of mind" (either in the strict sense of Fodor (1983) or according to some more relaxed criteria of modularity). Nothing in the arguments offered or surveyed in this paper requires taking a stance on these issues. Nor is it clear that they require taking a stance on the question of whether ToM-capacities are "innate" or acquired by way of general learning capacities Garfield et al. (2001). For further perspectives on these issues, see Gopnik & Wellman (1992); Leslie et al. (2004); Westra (2017).

[12] See, e.g., Heyes (1998) for a critical review.

[13] It is worth noting, though, that "False Belief test" is something of a misnomer here, since what is being tested is more generally a subject's ability to reason from another's *diverging* beliefs; for obvious reasons, these beliefs can very well be true. In a recent and insightful paper, Phillips

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This crucial amendment was soon absorbed in research. In 1983, Wimmer and Perner published the first results from applying an explicit False Belief test to human children. In their experiments, Wimmer and Perner presented subjects with a narration in which Maxi puts some chocolate into a cupboard. In his absence, Maxi's mother removes the chocolate and places it in a different cupboard. Subjects are then asked to indicate where Maxi will look for the chocolate when he returns. Wimmer & Perner (1983) report that none of the children in the 3-4 age bracket reliably passed this test, while 57% of the children in the 4-6 bracket and 86% of the children in the 6-9 bracket did.

Whereas Wimmer and Perner's results concerns typically developing children, another important source of insight comes from the study of atypical development. In 1985, Baron-Cohen, Leslie, and Frith published results comparing performance on a slightly modified False Belief task of typically developing children with children with Down's syndrome and subjects with autism. The modified task – now known as the "Sally-Anne" task – involves enacting with dolls a plotline very similar to the one that Wimmer and Perner originally presented in narrative form, thereby significantly reducing the demands on verbal comprehension involved in the experiment. They found that 85% of typically developing children (mean age 4 years 5 months) passed this test, whereas only 20% of a significantly older group of autistic children did (mean age 11 years 11 months) (Baron-Cohen et al., 1985). But importantly, they also found that in a similarly aged group of children with Down's syndrome, the success rate was essentially indistinguishable from that of the typically developing children (86% v. 85%: though we should again bear in mind that the mean age of the children with Down's syndrome was twice that of the typically developing children).

These results served to firmly place the False Belief test at the center of research on the development of the Theory of Mind. Jointly, they appear to provide strong support for the following claims: (i) that the onset of ToM abilities occurs on a relatively predictable timeline (i.e., it doesn't appear gradually, but seems to emerge relatively abruptly at a certain point in development),[14] and (ii) that its emergence is relatively independent of development along other dimensions of human intel-

& Norby (2021) argue that the exclusive focus on false belief attribution has led experimenters astray. I fully agree, though their point, as I understand it, is strictly orthogonal to the issues at stake in this paper.

[14] It is worth noting that with yet further modifications to the task – for instance, modifications aimed at reducing the verbal or attentional demands placed on the subject –, significantly younger children might also pass the test (see, for instance, Rubio-Fernández & Geurts, 2013). Taken at face value, however, what these results accomplish is primarily a matter of moving and providing more detail to the timeline; they do not, as such, undermine the idea that there *is* a timeline on which the onset of the relevant abilities marks a relatively abrupt change (cf. the early meta-study by Wellman et al., 2001; or the discussion in Leslie et al., 2004). Another challenge comes from studies deploying very different methodologies – for instance, eye-tracking studies – with the aim of showing that children or infants as young as 12-13 months form information-specific expectations about others' behaviors (Baillargeon et al., 2010; cf. Onishi & Baillargeon, 2005; Surian et al., 2007). It is now common to take these results to suggest that human social cog-

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ligence (since subjects with Down's syndrome score so much better on the test than autistic subjects, despite the fact that they typically have significantly lower non-verbal IQ).

Hopefully, this thumbnail history suffices to provide a sense of how the False Belief test works, and why results obtained by it are important. On the other hand, I hope it also serves to make clear that we are not particularly interested in FB tests just as such. That is, we are not presuming to test subjects simply for their ability to pass the test. Rather, we are interested in these tests because they are supposed to provide a certain kind of *evidence*. Evidence of what? Presumably, evidence of the presence or absence of distinctive socio-cognitive capacities ("Theory of Mind"). ToM deficits (whole or partial "mindblindness" in Baron-Cohen's vocabulary) in turn predict significant problems in understanding normal social interactions, including verbal and non-verbal communication, and in forming normal human relationships. In other words, there are important evidential and explanatory links between FB tasks (as experimental evidence), ToM development (as a theoretical construct), and normal socio-cognitive abilities of the sort that we can observe in everyday interactions.[15] If these links were found *not* to obtain, we would have to be prepared to tell a very different story about the scientific relevance of the False Belief test.

3 The role of language in Theory of Mind development

One important subsequent strand of this research concerns possible causal connections between language acquisition and ToM-development. These causal connections could take several distinct, but perhaps complementary pathways.[16] One possibility is simply that language facilitates Theory of Mind development by way of providing us with a ready-made vocabulary for referring to the hidden mental states of others ("belief," "intention," etc.). Another is that language is crucial because it typically permits (indeed encourages) embedding of complement clauses under specifically cognitive verbs ("Tom believes that / hopes that …," etc.) Finally, it could be that language facilitates Theory of Mind development simply by way of facilitating high-grade interpersonal communication: one's ability to reason routinely about other people's mental lives is just the natural by-product of extensive engagement in such communication.[17]

How might one test hypotheses that connect language development to ToM? The problem is that in usual cases, language deficits tend to go hand in hand with

nition in fact deploys two distinct systems for tracking others' beliefs, one "implicit," the other "explicit." We will return to the question of "implicit" Theory of Mind in section 6 of this paper.

[15] For less accommodating perspectives, see Bloom & German (2000); Reddy & Morris (2004).

[16] For perspectives, see, e.g., Harris et al. (2005); De Villiers & De Villiers (2014).

[17] For an overview, see Milligan et al. (2007).

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larger obstacles in cognitive development. Therefore, it's difficult to implement these tests in a way that would specifically favor the hypothesis that it is lack of language rather than these more encompassing cognitive obstacles that makes it the case that a particular subject is struggling with FB tasks.

But there is one group of subjects that can provide us with something like a natural experiment here, namely deaf children born into hearing families. These are children who, so far as we can tell, suffer no profound developmental challenges other than those which flow directly from the deafness itself.[18] And the most obvious such obstacle is, of course, the inability to access human speech. Unlike deaf children born into Deaf families, who will meet all the same milestones of linguistic development as hearing children do, and on a comparable time scale, deaf children born into hearing families are, as we have seen, likely to experience some degree or other of language delay.

Accordingly, we can ask, how does language delay of this form affect ToM development? In an influential early study, Peterson and Siegal claimed to find that 65% of normally intelligent pre-lingually deaf children born into hearing families (8-13 years) "failed a simple test of false belief which normal preschoolers, mentally retarded children, and other handicapped groups – apart from children with autism – routinely pass at a mental age of 4-5 years" (Peterson & Siegal, 1995, p. 459).

The test they administered was a slightly modified version of Baron-Cohen et al.'s Sally-Anne test. And interestingly, Peterson and Siegal claimed to find no significant difference between the results of these deaf children and the results of autistic subjects, as previously reported by Baron-Cohen et al. This similarity in performance on False Belief tasks leads Peterson and Siegal to speculate that ToM deficits in autistic and pre-lingually deaf subjects also share a common cause.[19] On this view, autism would not be, or at least not entirely be, a neurological disorder; rather, it would be a disorder whose aetiology would involve the affordances – specifically, linguistic, conversational affordances – provided in one's social environment. (I mention this background hypothesis here because (i) it's important to the way they frame their study, and (ii) because the comparison with autism will become important to my own argument at a later stage.)

[18] With an emphasis on "profound," since there is an emerging literature claiming to find parallel deficits also in late-signing deaf subjects' spatial cognition (Gentner et al., 2013; e.g., Pyers et al., 2010) and mathematical cognition (see Santos & Cordes, 2022, for a recent review article). It must fall to a different paper to examine in detail the empirical basis for these claims. But I am not aware of anyone arguing that there is pattern connecting these various deficits, other than their putative overrepresentation in this one group of subjects. That is, once we control for the variable of being born deaf into a hearing family, there is, to my knowledge, no suggestion that displaying any one of these deficits increases the probability that you will also display any of the others.

[19] If this comes as a surprise, we might bear in mind that there is a long history – going back to Keeler (1958) and Chase (1972) – of psychologists engaging in similar speculation about the "autism-like" character of many congenitally blind children. See, for instance, Brown et al. (1997, p. 701), who argue that "there is indeed something special about congenital blindness that predisposes to a full or partial syndrome of autism." For a more recent perspective, see Bishop et al. (2005).

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This reasoning caught on, and in 2000 Peterson and Siegal were able to report on 11 new studies, all corroborating the previous findings. They summarize this research as follows: "these studies provide consistent support for the proposition that signing deaf children from hearing families are seriously delayed in acquiring a theory of mind" (Peterson & Siegal, 2000, p. 131). And specifically, they endorse an observation coming out of a study by Russell et al. (1998), according to which it is not until the age of 13-16 that "deaf children's success rates were seen to approximate those of normally developing 4-year-olds" (Peterson & Siegal, 2000, p. 127).

(One further point of clarification: in the wider ToM literature, some authors give serious consideration to the possibility that what specifically eludes younger children is a "representational" ToM, centered on the explicit concept of belief, but that these children may nonetheless have significant facility with other "mentalistic concepts" such as desire or intention.[20] This possibility of a more graduated entry into the full suite of mindreading capabilities is rarely at the front of research on language-delayed Deaf subjects.[21] Instead, these authors seem to naturally gravitate in the direction of treating the ToM as something closer to an all-or-nothing matter, as can be seen from how they typically define the concept; e.g., "[the] term 'theory-of-mind' describes an individual's awareness of human behavior as governed by an interconnected network of mental states, including beliefs, desires, and feelings" (Peterson & Siegal, 1995, p. 459); "[a] theory of mind is thought to confer the ability to impute mental states like beliefs, intentions, memories, and desires to self and others" (Peterson & Siegal, 2000, p. 123).)

A sizable literature has since grown up around these findings. Here is a selection of headlines: "understanding theory of mind (ToM) is seriously and similarly delayed in late-signing deaf children and children with autism" (Peterson et al., 2005, p. 502). "Late signers – like children with autism – are markedly delayed in their understanding of mental states" (Harris et al., 2005, p. 70). "Deaf children of hearing parents show a protracted delay in performance on 'theory of mind' measures that suggests they encounter difficulties in acquiring knowledge of false beliefs and other mental states" (Morgan et al., 2014, p. 41).

Many of these studies specifically affirm a causal role for language in the development of a Theory of Mind. Here are some examples: "These deaf children are well above the usual age at which false belief reasoning is mastered, and yet they are often failing the tasks of all varieties"; accordingly, "it is clear that language is a critical factor in children's ability to reason about the mind" (Schick et al., 2007, p. 391). Along similar lines, Moeller and Schick (2006, p. 752): "there is little evidence that children acquire these skills [ToM abilities] in the absence of language, and language proficiency at some level of sophistication seems to be necessary."

These are not, it should be emphasized, fringe opinions. To the contrary, these conclusions are now treated as established scientific fact: for instance, a 2022 review article in *Nature* argues that

[20] Cf. Gopnik & Wellman (1992); Wellman et al. (2001).

[21] We will look at two exceptions in section 6.

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> [c]omparisons of deaf children who grow up in hearing families without native signers and deaf children who grow up in families with native signers reveal fundamental differences: the latter show typical language development, quickly become fluent native signers, and show typical ToM development. By contrast, children in the former group show delayed language development and equally delayed ToM development. These different trajectories in ToM development in two populations that differ primarily in their linguistic experience thus suggest a crucial role for language in the emergence of ToM. (Rakoczy, 2022, p. 230)

Striking as they are, these results nonetheless leave open an important question. Off the cuff, it would be tempting to read these results as suggesting that once these children have attained a relevant degree of language mastery, we also should expect their problems with ToM to go away. In their original 1995 study, however, Peterson and Siegal (1995, p. 472) also touted the more radical possibility that the ToM deficits might endure even after the children have been fully integrated into a language community.[22] Subsequent studies affirm this possibility, arguing that these children will continue to be stymied by False Belief tests even long after they have caught up on language. This provides strong evidence, according to this line of research, that not only is social cognition dependent on language acquisition; delayed language acquisition can also cause protracted delays in socio-cognitive development which aren't easily resolved even by addressing the deficiencies in language.

To this effect, Meristo et al. (2007) find that late-signing deaf children score significantly worse on False Belief tests than younger, "native signers" (i.e., deaf children raised in signing families), despite the fact that *at the time of testing*, the late-signing children were more proficient in the language.[23] By contrast, the native signers were indistinguishable from similarly aged hearing children in terms of their ToM performance (Meristo et al., 2007, p. 1156). They conclude that although "a certain level of linguistic skill may be necessary for children to succeed on particular ToM tasks, many children do achieve proficiency in formal aspects of language understanding, but still do not answer correctly" (Meristo et al., 2007, p. 1167).

Morgan & Kegl (2006) address this question of a possible "critical period" for ToM development (akin to what we suppose exists for language learning) in the context of the Nicaraguan Deaf community. This might seem like an odd and perhaps arbitrary choice. But in fact, there are some unique features of this community and its recent history which makes it extremely well suited for research into these questions.[24] Until the late 1970s, Nicaragua had no schools for the deaf, as a result

[22] See also Siegal and Varley (2002, p. 469).

[23] The tests were conducted in British Sign Language. For similar results involving American Sign Language, see Schick et al. (2007).

[24] For philosophical accounts, see Westra (2017); Begby (2017).

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of which most deaf children would grow up in complete linguistic isolation. But then, starting in 1977, authorities began busing deaf children to a school in Managua. However, the school implemented a curriculum based on lip reading and finger spelling Spanish. As we now know, that doesn't work; it's effectively like trying to teach someone to read before teaching them how to speak. But instead of learning Spanish, something rather more surprising happened, namely that the children, during the course of schoolyard play, would spontaneously develop their own language, using manual gestures. From early, primitive beginnings, this language developed very rapidly, since each new cohort of children effectively acted like a new generation of learners. Linguists describe this as the birth of a whole new language occurring over the course of 10-15 years.[25] So, in addition to being an extremely inspiring and hopeful story, we now have, as a result of the fact that scholars have been on the scene from the early days, very close documentation of the stage-by-stage development of the Nicaraguan Sign Language (NSL), from its early beginnings in the 1980s. But also, since the community is so small, experimenters have unique personal knowledge of these subjects, having followed them over decades.[26]

Deploying FB tests on various groups of NSL signers, Morgan and Kegl quickly converge on the age of 10 as marking the critical point: subjects who are first exposed to language only after that age are likely to suffer long-lingering deficits in ToM development. Specifically, it seems that their deficits will persist well past the point where the subjects have effectively caught up with their peers in terms of language production. To spell this out, imagine, for instance, a 10 year old who has been part of the school system since the age of 8, and a 14 year old who has been part of the school system since 11: even though they might at this point be essentially indistinguishable in terms of their linguistic abilities, the 14 year old is likely to have problems with FB tasks that the 11 year old does not. And that difference can only be explained by the fact that the 14 year old's first exposure to language came after a certain critical age. It is age of acquisition, and not years of exposure to language, which predicts success on False Belief tasks (Morgan & Kegl, 2006, p. 815).

Ann Senghas and Jennie Pyers have also worked extensively with the Nicaraguan Deaf community. In a 2009 co-authored paper, they look at productive mental state vocabulary, such as "knows," "thinks," "believes" and so forth. One thing we know from having followed the development of the NSL is that the language did not accrue specific mental state terms until fairly late in its development. Another general observation is that older signers who learned early versions of the language typically struggle – even in adulthood – to pick up the innovations, be they grammatical or lexical, of subsequent cohorts. (Even so, it is important to note that these subjects are still fluent, by any relevant measure, in that earlier version of the language.)

[25] For a detailed overview, see Kegl et al. (1999).

[26] See, for instance, Morgan and Kegl (2006, p. 131).

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Against this background, Pyers and Senghas find that "adults who acquired a nascent form of the language during childhood produce few mental-state signs and fail to exhibit false-belief understanding." This correlation is strong enough to support a causal claim: the acquisition of mental state terms is a "necessary prerequisite" of FB understanding, "one that cannot be replaced by even 25 years of social experience." They conclude, with a nod to the work reviewed above, that "language and false-belief understanding develop hand-in-hand" (Pyers & Senghas, 2009, p. 810). Nonetheless, even if language acquisition is *necessary* for FB understanding, it is evidently not *sufficient* for its full development: if, for instance, the language one acquires is underdeveloped in certain kinds of syntactic and lexical resources, one is likely to show enduring impairment in socio-cognitive abilities even in adulthood. Similarly, as we saw with Morgan and Kegl, if one's first exposure to language comes only after a certain age, one might well continue to struggle with social cognition even after one has attained native-like fluency in the language.

4 Discussion: False Belief tasks as evidence

Put together, this certainly looks like it amounts to an impressive accumulation of evidence that language-delayed deaf subjects will suffer significantly in socio-cognitive development. Moreover, it offers the outline of an intuitively plausible causal explanation: normal ToM development is crucially facilitated by language. If language development is impaired or significantly delayed, we should expect to see impaired or delayed ToM development as well. However, even after subjects have developed full facility with language, it is likely that they will continue to struggle with False Belief tasks. Even under favorable circumstances, it can take years for them to catch up on Theory of Mind, if they catch up at all.

But on second thought, these results seem anomalous. I submit that we really *should* be more surprised at these findings than researchers mostly seem to be. What I have in mind here is that there appears to be a striking discordance between the experimental evidence provided by FB tasks and the evidence that we obtain by observing the same subjects engaged in everyday interactions.[27]

This kind of observational evidence could come in many forms. For a simple example, consider spontaneous play. Spontaneous play often involves significant role differentiation, with each role charged with distinct epistemic tasks. So if you see a child who is actively monitoring the situation of other players, making sure everyone is alert and "looking out," then, one would think, you also have evidence of relatively highly developed ToM abilities.[28] Quite simply, this kind of on-the-fly

[27] Remember, I presume that we have no real interest in different peoples' scores on false belief tasks per se. Rather, these scores are of interest only insofar as they provide us with evidence, specifically evidence of the presence or absence of certain kinds of socio-cognitive abilities.

[28] Notice, for instance, how Baron-Cohen et al. (1985) point to lack of pretend play as an indicator of the "social abnormalities" typical of autism.

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coordination requires significant ability to reason about the informational situation of others.[29]

Quite often, this coordination manifests in the form of explicit communication, verbal and non-verbal. So in order to get a firmer sense of what I have in mind, we can focus on a more complex source of evidence, namely unscripted, intentional communication, following broadly Gricean tracks (Grice, 1967/1991). In particular, I'm interested in the adoption of simple "natural communicative stances" such as asking questions, or intentionally providing information that might be useful to one's interlocutor. Let's say I see you walking around looking for something. I infer that you are looking for your glasses, which you need for reading. I inform you that you were wearing your glasses when you left the room five minutes earlier, but not when you came back. In that way, I'm volunteering information which helps you narrow your search space, information which I have reason to believe you don't have, since otherwise you wouldn't be spending your time looking in this room. Intentional communication of this sort clearly seems to turn on a sensitivity to the "informational imbalance" between two subjects, an awareness of the fact that others inhabit different cognitive perspectives on our shared world.

At a fundamental level, and as Grice observed, communication of this sort is cooperative: it is driven by the insight that I might know something you don't (or vice versa), coupled with an understanding that different sorts of communicative stances – asking, telling, and so on, – provide different mechanisms for redressing that informational imbalance. More generally, communication of propositions that one must already assume to be part of "common ground" (or "shared knowledge") between speaker and hearer will often fail the test of *relevance*. By contrast, whenever a speaker systematically abides by a criterion of relevance, we should presume they have a reasonably developed sensitivity to other people's distinctive perspectives on the world – in the simplest case, what one can assume that they do or don't know.[30]

In typical cases, it is obvious that the syntactic and semantic apparatus of language plays a hugely important role in facilitating such communication. But as the above reasoning brings home, pragmatics is another crucial part of the picture. As Baron-Cohen puts it (1995, p. 29):

> [One] way in which mindreading is held to play an essential part in successful communication is in the speaker's monitoring the informational needs of the listener - that is, in the speaker's judging what the listener may already know or be ignorant about, and what information he or she should supply so that the listener will be able to understand the message.

[29] One might object, citing more recent theoretical developments, that such observations could only be evidence of a qualitatively different, less demanding, form of "implicit" ToM. I will respond to this strategy in section 6 of this paper.

[30] For an important account of a more encompassing notion of relevance and its role in communication, see Sperber & Wilson (2002). For further reflections on the links between intentional communication and the Theory of Mind, see Rubio-Fernandez (2019).

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To see how this matters, consider the phenomenon of "homesign." Homesigners are deaf subjects who have never been fully functionally integrated into any signing community (or are integrated only at advanced age), and so have never had the opportunity to develop their language capacities in full. If the broader thesis under consideration is that impaired or delayed language acquisition will seriously affect one's socio-cognitive abilities, we can accordingly take the situation of homesigners to be the most extreme manifestation of the problem. Homesigners do, of course, engage in communication with their non-signing peers. But even in adulthood, their communicative productions show only very minimal hallmarks of syntactic structure; moreover, they have virtually no relevant access to a public lexicon.[31]

Nonetheless, it is quite clear from the literature that homesign communication is richly suffused with pragmatics.[32] For instance, we find extensive evidence that homesign communication involves intentionally establishing joint visual attention for securing reference.[33] That is, homesigners seem to understand full well that their communicative efforts are highly unlikely to succeed unless they occur in a jointly attended visual space. They go out of their way to "ground" their communication in this way (cf. Goldin-Meadow, 2003, p. 226). Moreover, there is clear evidence of audience design: homesigners design their gestures to facilitate uptake by interlocutor, for instance by settling on a relatively small lexicon of stable "generic" gestures rather than explore the full potential for analog representation inherent in the manual modality (ibid., pp. 84-85). They are carefully monitoring for audience comprehension; in particular, they have been observed to repeat (and modify) their gestures in the absence of evidence of comprehension (ibid., pp. 83-84). Throughout all of this, it seems that they display a reasonably robust and workable understanding of the principles of Gricean communication. They abide by implicit rules of conversational turn taking;[34] more generally, their contributions build on previously established common ground in a manner that – given the lack of a public language – mirrors the structure of ordinary conversation to a surprising degree.

[31] See Goldin-Meadow (2002); Goldin-Meadow (2003).

[32] See Begby (2017) for an overview.

[33] In fact, it is standard practice in this literature to refrain from coding any motor-behavior as a gesture unless it involves establishing eye-contact with the interlocutor (cf. Goldin-Meadow et al., 1984; Hunsicker & Goldin-Meadow, 2013). I should add that while the concept of "joint (visual) attention" is central also to the communicative pragmatics literature, what I am invoking here is not *simply* the idea of an early developing "implicit" ToM to be contrasted with the "explicit" ToM measured by the FB test. I will provide a fuller discussion of "implicit" v. "explicit" ToM in section 6 of this paper; meanwhile, note that the abilities involved in communicative pragmatics reach much farther and typically develop on a much more protracted timeline than either of implicit or explicit ToM. My point is that it would be difficult to account, theoretically or empirically, for how a subject could have relatively advanced facility with communicative pragmatics without assuming that they are also, for instance, able to "impute mental states like beliefs, intentions, memories, and desires to self or others" (Peterson & Siegal, 2000, p. 123).

[34] See, for instance, the accounts of particular communicative interactions in Goldin-Meadow (2003, pp. 149–150).

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To see how this is relevant, it will be particularly illuminating to contrast with autistic subjects (remember how Peterson and Siegal were sufficiently impressed with the similarity in failure rates on FB tests between autistic and late signing deaf subjects to suggest there might even be a common cause). In the case of autistic subjects, Baron-Cohen (1995, p. 66) reports widespread failure to establish eye contact for joint attention and overall, little deployment of "declarative gestures", which is, of course, the hallmark of homesign communication. Further, there is systematic failure to monitor for comprehension, and in general, little understanding of audience needs, little observance of the maxim of relevance, etc.

One way of summarizing these results is to point out that autistic subjects typically display significant impairments in pragmatic processing, while sometimes – at least in high-functioning cases – displaying (near) normal facility with literal meaning. By contrast, homesigners' entire communicative repertoires are in a sense grounded in pragmatics, precisely because they cannot rely on the shared linguistic conventions that are typically thought to specify literal meaning (Begby, 2017). In short, it seems that all of those features that are *distinctively and diagnostically absent* from the communication of autistic subjects are clearly present in the communication of homesigners.

Well-established connections between pragmatic abilities and the ToM help drive home the importance of these observations.[35] To quote Baron-Cohen again:

> for communication to succeed, the speaker must monitor whether the meaning of an utterance has been received and understood as he or she intended it to be, or whether rephrasing is required to resolve ambiguity. Dialogue understood in this way becomes much more than the production of speech: it is revealed as intrinsically linked to the use of mindreading skills. (Baron-Cohen, 1995, p. 29)

Against this background, it seems plausible to say that autistic subjects' widely observed pragmatic impairments – in particular, their failure to abide by the principle of relevance and to monitor for comprehension – just *is* the manifestation (in the domain of communication) of their ToM deficits. I submit, we find nothing comparable in the case of homesigners. If anything, we should expect them to be, in some sense, pragmatically *hypersensitized*, precisely because they need to craft their communicative opportunities from scratch.

Even in the extreme case of the homesigner, then, we find extensive evidence of relatively highly developed pragmatic capacities. We have strong grounds to presume that these capacities are correlated with Theory of Mind abilities. Naturally, this evidence is even stronger if we look at the communicative behaviors of deaf children born into hearing families, once they are firmly on the track of language acquisition. Yet these are the subjects who, in the line of research currently

[35] See, for instance, Tager-Flusberg (1997); Sperber & Wilson (2002); Wilson (2005); Perkins (2007); Cummings (2013).

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under review, are said to be suffering significant and protracted delays in Theory of Mind development.

5 A closer look

So, even if we allow that results of FB tasks are evidence, we have no reason to presume that they are the *only* source of evidence we have when it comes to socio-cognitive abilities. And how *strong* FB tasks are as a source of evidence will depend in part on what other sorts of evidence we have at our disposal. In autism research, FB tasks provide *good* evidence in large part because results cohere with long-established natural-observational evidence of socio-cognitive impairment.[36] That is, we presume to know, on independent grounds, that autistic subjects often struggle with social interactions. But in Deafness research, I argue, implications of FB failure precisely *contradicts* what we should presume to know on the basis of natural-observational evidence.

In fact, this contradiction is so close to the surface in some of these studies, that I find it surprising none of the researchers are brought to remark on it. Speaking generally of ToM, they say things like the following: ToM abilities involves "understand[ing] that people, including themselves, possess minds" (Russell et al., 1998, p. 903). ToM involves the "the realization that every person holds a subjective view of the world based on his or her experience" (Lohmann & Tomasello, 2003, p. 1130). As such, it is no surprise to learn also that ToM is "vital to everyday life" (Woolfe et al., 2002, p. 768), and indeed that it is "one of the quintessential abilities that makes us human" (Baron-Cohen, 2000, p. 3). Further, "[a] theory of mind is essential for effective communication" (Peterson & Siegal, 2000, p. 124). Finally, "Difficulties in reasoning about ToM would [...] preclude one from forming mature interpersonal relationships" (Morgan & Kegl, 2006, p. 812).

We can add, just to be clear: these aren't incidental observations regarding subjects who also happen to suffer from ToM deficits; instead, we can take these claims to be more or less definitional of the Theory of Mind as a theoretical construct. ToM deficits is what we have when otherwise normally intelligent subjects display a lacking understanding of the fact that others have beliefs and desires different from one's own and thereby are precluded from participating effectively in communication and from forming mature interpersonal relationships. Without this sort of stipulated connection to naturalistic, everyday observations, the ToM-construct would be of dubious empirical and theoretical value.

But on the other hand, we read in the same studies that homesigners, for instance, "are *not socially impaired*; they enjoy relatively *typical social interactions* with their hearing families, friends, and neighbors" (Gagne & Coppola, 2017, p. 3,

[36] Though we should note that even here, the picture has become significantly more complicated over time, not least as the diagnostic criteria for what is now called "autism spectrum disorder" have evolved. See Altschuler & Faja (2022) for a critical review of the test-retest reliability of various ToM tasks, including FB tasks, as deployed on school-aged children with ASD.

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my emphasis). Similarly, and with regard to the larger class of language delayed deaf children, we learn that in schools where sign is used, these children will "typically *enjoy cohesive relationships* and *easy communication* with their signing classmates" (Peterson et al., 2005, pp. 514–515, my emphases). Again, there's more: these deaf children "are *actively sociable*, even with language delays," they "*do not have any special problems with social interactions* other than that imposed by delayed language skills" (Schick et al., 2007, pp. 379–380, my emphases). Further,

> Signing deaf children of hearing parents are found to lag several years behind hearing children in their performance on false-belief tests, even when care has been taken to include only *children of normal intelligence and social responsiveness* in the deaf samples. (Peterson & Siegal, 1999, p. 126, my emphasis).

This is troubling, and as I said, I find it mildly surprising that the scientists claiming to find significant socio-cognitive impairments in language-delayed deaf subjects don't even appear to notice the discrepancy between the conclusions they draw on the basis of their experimental results, on the one hand, and their own observations of the same subjects as they engage in their everyday lives, on the other. It is surprising, not least because, as Peterson and Siegal point out, the requirement of "normal intelligence and social responsiveness" is effectively encoded in the selection criteria.[37] That is, if their subjects did *not* evince a largely normal range of social and non-social intelligence, they would have been precluded from participation in the experiments.

Moreover, we really cannot have it both ways here: we cannot claim that these subjects suffer significant ToM deficits while also conceding that these deficits fail to manifest in ordinary social interactions. Having it both ways would effectively entail a retraction of claims that ToM is "vital to everyday life," that it is "is essential for effective communication," that ToM deficits would "preclude one from forming mature interpersonal relationships" and so on. But if we were prepared to make those retractions, then it is no longer clear what theoretical and empirical role the ToM construct is meant to play in the larger picture of human cognitive functioning. Naturally, we would also be losing our sense of the False Belief test is supposed to be evidence *of*.

Again, none of this is to suggest that the FB test is entirely lacking in "ecological validity."[38] Nor is it to say that the FB test is restricted to merely providing confirmation of facts that we already presume to know on independent grounds. It is perfectly consistent with the argument of this paper that FB tests can contribute

[37] In other words, there is little reason to think that these subjects display the particular clustering of mental health issues that have recently been brought together under the diagnostic heading of "Language Deprivation Syndrome" (Glickman & Hall, 2018; Gulati, 2018; Hall, 2017; see, e.g., Hall et al., 2017).

[38] Cf. Bloom & German (2000); Reddy & Morris (2004).

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significantly to our overall understanding of the structure and emergence of ToM-abilities, for instance by providing developmental timelines or by shedding light on their correlation with or dissociation from other cognitive abilities. It is specifically in its diagnostic applications that we must take care to consider our total evidence. But this is in no way unique to the FB test: after all, any screening tool will have a certain false-positive rate which we will need to bear in mind when we presume to draw inferences from the results of individuals or demographic groups.

Nonetheless, one can argue that even if the experimental results are anomalous, they are nonetheless deeply puzzling in their own right and robust enough to merit *some* kind of explanation. And if we are not prepared to accept the story about ToM deficits, what other explanation is available to us?

But perhaps we should also consider the possibility that there is actually nothing here to explain. Initial appearances to the contrary, maybe what we have here is not evidence, but just noise. Without access to the raw data underlying all of these articles, we cannot explore this possibility in full detail. But for a rough outline, here's the direction I have in mind: all of these studies present themselves as building on previous work, adding further evidence corroborating the central claim, namely that language delayed deaf subjects have impaired ToM abilities. The force of the conclusion fully depends not on the results of any one study, but on the appearance that the different studies all build upon each other, adding cumulative weight to the conclusion. But here's the rub: on closer examination, it may turn out that these studies actually contradict each other on several crucial points of detail, thereby leaving us with nothing to explain.

Here are some examples indicating the direction that an argument of this form might take. In early studies, Peterson & Siegal (2000) report that only 35% of 26 language-delayed deaf subjects in a group aged 8-13 (median age 10.7) passed the False Belief test. Moreover, they argue that it is only by age 13-16 that such subjects begin to approximate results of typically developing 4-5 year olds.

Initially, Morgan and Kegl's results appear consistent with this. But once we start looking into the details, things are no longer so clear. Morgan and Kegl's study distinguishes two groups for comparison: early learners and late learners. If we look specifically at the results they report with early learners, things look different: 91% success rate in a group aged 8-24 (median age 13.9), with only one subject failing (Morgan & Kegl, 2006).

Let us spell this out in more detail. Peterson and Siegal are reporting on subjects in the range of 8-13 years. Since we don't have access to their raw data, we don't know precisely how many of Morgan and Kegl's subjects fall in that same range. But it's reasonable to think that a fair few of them must, if the group as a whole has a median age of 13.9. Now in that 8-13 subgroup, we know that at most one subject failed the test, and possibly that no subject failed the test. So however this pans out, it is very likely that their results are at strong variance from those reported by Peterson and Siegal (35% v. verging on 100%). So although the headline conclusion they offer appears consistent with previous studies, it looks like there's significant inconsistencies at the level of detail.

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This pattern persists. As we have seen, Pyers & Senghas (2009) report strong correlations between size of mental state vocabulary and success on FB tasks. This correlation, they believe, is strong enough to support a claim of causation. Testing over a two-year period, they claim that a subject's acquiring as little as one new mental state word would lead to improved FB performance.

But in Morgan and Kegl, the predictor of success on FB tasks was the age of acquisition, not the complexity of language at time of testing. And in fact, Morgan and Kegl make a specific observation in connection with one of the experiments they administered, namely that they found no significant difference in productive language between those who passed and those who failed the test in terms of "reference to belief, doubt, desire, thought, decision, purpose and goal" (Morgan & Kegl, 2006, p. 817). Again, the sense of building momentum from study to study appears to crumble when we look into the specifics of each study.[39]

A final example illustrates the point from a somewhat different angle. In a recent Nicaragua study, Gagne & Coppola (2017) set out to compare the performance of three groups on FB Tasks and executive function tasks: homesigners, NSL signers, and unschooled hearing adults.

We will focus on the FB tasks. The results are striking. All of the four homesigning subjects they tested failed on both of the tasks given to them. Based on these experiments, Gagne and Coppola conclude that access to a language community is necessary for ToM development. (Of course, the homesigners do have access to a community, only not, in the strict sense, a language community.) They write: "Given previous studies showing delays in False Belief abilities in deaf children and adults with compromised access to language …, it is not surprising that the homesigners performed poorly" (Gagne & Coppola, 2017, p. 15).

What should we make of this? It is certainly true that scientific inquiry often throws up some surprising results. But sometimes the results are *so* surprising that they should force us to reconsider the validity of the experiment. This might be one such case: that homesigners would score poorly might be predictable in light of previous results; that they would score infinitely worse than chance is a different matter altogether. One possibility might be that this stark result is simply an artifact of the small number of participants. But in general, citing the small *n* is hardly a convincing way to vindicate the evidential value of one's experimental results, and certainly not if those results are highly surprising if seen against the background of well-established natural observational evidence of the same subjects engaged in everyday interactions.

So it is fortunate that the same paper also gives us independent reason to think that something has gone wrong here, namely the results of the same test administered to unschooled adult hearing subjects. Bear in mind, these are perfectly ordinary adult subjects, who might be married with children and hold down eminently

[39] For more, see Gagne et al. (2019), which, though containing many supportive references to work reviewed in this article, also concedes to finding "no correlation between FB scores and age, years of language exposure, or age at which [subjects] had first been exposed to sign" in its own dataset (Gagne et al., 2019, p. 267).

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respectable jobs in their everyday life. The only thing that marks them out is they never received formal schooling.[40]

Testing such unschooled adult hearing subjects on the same task, Gagne and Coppola obtain merely at-chance results. To let the significance of this sink in, remember that this means these adult subjects performed significantly worse than what we should expect from typically developing 5 year old subjects. But presumably, their lack of schooling notwithstanding, this is a group that you would expect would do *at least as well as any* 5 year old, including, it should go without saying, 5 year old counterparts of themselves. For obvious reasons, we can't explain their poor showing on the test by pointing out that they are unschooled, because, well, most 5 year olds are unschooled too![41]

To my mind, this sounds very much like a control-group failure, which would invalidate the whole experiment. In effect, maintaining the validity of the experimental results would entail holding that these people have meanwhile lost crucial socio-cognitive abilities that we must assume they possessed as pre-schoolers. Moreover, we would have to suppose that they have lost these abilities at some point during decades-long careers of maintaining rich and rewarding social relationships, holding down normal jobs and engaging in day-to-day banter about politics, culture, and sports. Again, it seems that maintaining the evidential significance of these results could only come at the expense of effectively retracting the claim that ToM is "vital to everyday life," "essential for effective communication," and that ToM deficits would "preclude one from forming mature interpersonal relationships."[42]

[40] For reference, bear in mind that the adult literacy rate in Nicaragua, while improving, is still comparatively low (82.61%, as of 2015: https://ourworldindata.org/literacy).

[41] Gagne and Coppola cite, in their favor, results suggesting that children in pre-literate cultures also show poor FB scores. To my mind, such findings could only serve to further problematize FB tests as a relevant source of evidence, not to vindicate their research. Moreover, their problem remains: even if *children* growing up in pre-literate cultures are delayed or impaired in their performance on FB tasks, we are now talking about high-functioning adults. We cannot fail to miss the point here that up until very recently in human history, most people would grow up and indeed live the fullness of their lives in non-literate cultures. To this day, many do. If we are now considering the hypothesis that the ToM is a peculiarly *modern* innovation, one that is effectively dependent on widespread literacy, the explanatory landscape becomes very different. In particular, it is hard to square with any plausible picture of the evolution of these cognitive capacities, if, for the vast majority of human history, we've not been able to benefit from them.

[42] For an analogy, consider the fact that some people can go through their lives with no real awareness that they are red/green color blind. We can ask, could ToM be like *that*? Could subjects with significantly impaired ToM abilities nonetheless so successfully navigate our social spaces that not even their loved ones would notice? Could it be that such deficits are only discernible under specialized experimental conditions? I doubt that very much. At any rate, if this were the case, we would have to retract the idea that ToM is vital to everyday life, essential for communication, part of what makes us human and so on, since it would clearly not have any evidential or explanatory connections to patterns of behaviors on display in everyday life.

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6 Explicit v. implicit ToM?

Minimally, these reflections should serve to remind us that we often have multiple bodies of evidence bearing on a particular hypothesis, and that there can sometimes be a clear conflict between these bodies of evidence. In a case such as this, there is no obvious reason – at least not one that I've seen articulated by anyone – why we should systematically prefer the experimental evidence over the natural observational evidence.

I have argued that mastery of Gricean communication provides strong natural-observational evidence of significantly developed ToM abilities. The same can be said for the experimenters' own observations of their subjects engaged in everyday interactions. We can assume that these observations have been made and have not been meaningfully drawn into doubt, since they are, effectively, encoded into the selection criteria determining eligibility for participation in the experiments.

However, drawing on evidence from communicative pragmatics and broader social interactions in this way might seem problematic in light of another development in ToM-research. In the face of mounting evidence of mindreading capacities in children significantly younger than 4, some theorists have begun to draw a distinction between "implicit" and "explicit" Theories of Mind, exploring the possibility that these might be underwritten by different cognitive mechanisms which emerge at different stages of development.[43] The sort of evidence that I have called attention to in this paper – e.g., the grounding of referential communication in joint attention, and monitoring the informational needs of others – would, on this story, be part of an early developing "implicit" ToM. By contrast, the FB test would measure the emergence of a later developing "explicit" form of ToM. Apperly & Butterfill (2009) compare this to the distinction between two systems of numerical cognition in human reasoning, as explored, for instance, by Carey (2009).

This distinction is by no means widely adopted in the literature we have surveyed so far, though some authors have recently nodded in its direction. In this vein, they might acknowledge, for instance, that behavioral evidence – including evidence from communication in natural settings – serves to indicate that these subjects may indeed have some "precursor" abilities to the full ToM. Nonetheless, they maintain that *some* important dimension of the ToM must continue to elude them, since they score so poorly on FB tasks. For instance, Gagne and Coppola (2017, p. 17) concede that their results fall short of conclusively demonstrating that homesigners are "incapable of understanding others' beliefs." In particular, they allow that "[p]ositive findings with homesigners using an implicit measure could lend credence to two-system accounts of social cognition" (ibid., p. 18).[44] Similarly, Pyers and Senghas (2009, p. 805) suggest that first-cohort signers of NSL "may have

[43] See, e.g., Apperly & Butterfill (2009); Rakoczy (2017); Low et al. (2016).

[44] I would argue that we don't particularly need sophisticated experimental paradigms to probe the question of whether homesigners have "implicit" ToM abilities, since we have plenty such evidence available already, in the form of detailed accounts of how their communication is grounded in joint attention, involves monitoring for comprehension, obeys the principle of relevance, etc.

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previously relied on an understanding of emotions and desires to function in daily interactions." But it was only with their exposure to the language of the (significantly younger) second-cohort NSL signers that they were given access to "the linguistic tools to support a more mature theory of mind." Accordingly, they argue that "the richness of one's language must play a key part in driving a mature theory of mind", and that "in cases where the necessary language is not available during childhood, this transition may occur decades later" (ibid., p. 806) – if, indeed it occurs at all.

These invocations of a distinction between implicit and explicit Theories of Mind raise questions beyond the scope of this paper. But even in lieu of a full discussion, there are a number of important points worth noting: (1) two-systems accounts of ToM abilities are *controversial*, on both empirical and theoretical grounds: it is unclear that a two-systems account is strictly warranted by the empirical evidence (see, e.g., Carruthers, 2016; Rakoczy, 2022). More importantly, to my mind, it remains theoretically unclear, once the full complement of "implicit" ToM abilities are in place, what a subject would gain by adding the supposedly "explicit" ToM ability. In particular, we must be on guard against the possibility that a subject's possession of "explicit ToM abilities" would be measured by nothing other than their passing the FB test itself.

(2) If it were conceded that there *is* ample evidence for implicit ToM capacities in language-delayed deaf subjects, then arguably none of the dramatic and sweeping claims considered in the previous section would follow. That is, it is not *explicit* ToM capacities which "makes us human," etc., but rather any significant development of ToM capacities of any form.[45]

Finally (3), the distinction between implicit and explicit ToM is standardly invoked to explain the differential socio-cognitive abilities of typically developing children in the age groups 1-3 and 4-5 (roughly speaking). It is extremely doubtful that it can be similarly invoked to explain the differential socio-cognitive abilities of typically developing children (who are presumed to reach the "mature" stage of their ToM by 4-5 years) and socially high-functioning adolescents and adults, as most of these deaf subjects will be.[46] As we have seen, the only evidence we would have to support this claim would be the latter group's relatively poor scores on FB tests. By contrast, all our natural-observational evidence supports the claim that they suffer no particular problems in terms of social cognition. If we continue to insist that the FB tests show otherwise, we run the very real risk that one's transitioning to the level of "explicit" ToM is empirically indicated by nothing other than

Nonetheless, I am glad to note that they follow up this suggestion, with positive results, in Gagne et al. (2019).

[45] Cf. Rubio-Fernandez (2021).

[46] For instance, in section 4, I referred to socio-cognitive competencies involved in communicative pragmatics as a relevant source of evidence: while communication importantly involves a capacity for joint visual attention (typically associated with early-developing "implicit" ToM), it's important to understand that the development of the fuller range of pragmatic competencies reaches well beyond the timeline of either implicit or explicit ToM.

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one's passing the FB test itself. At this point, the idea that the FB test connects in explanatorily relevant ways with other behavioral observations we might make is quite lost.

7 Concluding remark

Sustained research aimed at understanding the unique challenges posed by deafness in a world made for hearing people is highly welcome and should be of intrinsic interest. By contrast, claims imputing to subjects a significantly impaired (or delayed) understanding of the fact that other people even "possess minds" or "hold a subjective view of the world," while they may be good for drumming up broader interest in the subject, should always be treated with a significant degree of caution. This is especially so when we are talking about an already vulnerable (but otherwise very diverse) population. The reason is that such claims are potentially highly stigmatizing and can contribute to normalizing negative stereotypes about these subjects.

The argument of this paper provides further and more specific grounds for caution. In light of the failure of these results to "stack up" across these several studies, it's just not clear that this research meets the evidential bar for asserting that language-delayed deaf subjects suffer distinctively and symptomatically in the domain of social cognition. Such claims are not, as I have argued, empirically well-grounded; as a result, they do not help us understand the challenges faced by these subjects or suggest remedial strategies to help them overcome these challenges.

To be very clear, none of this is to downplay the crucial importance of early interventions and the development of institutionalized support networks for deaf children born into hearing families. Quite the contrary: the relative social isolation and loss of opportunity associated with being born deaf into a hearing community is sufficiently devastating on its own terms to warrant such measures.[47] There is no need to add further justification for such measures by speculatively claiming developmental deficits dubiously supported by the evidence.

Acknowledgments
I owe a particular thanks to Paula Rubio-Fernandez for early encouragement in pursuing this argument and for invaluable advice along the way, as well as to Rob Stainton and Robyn Carston for detailed written comments on a previous draft, and to Kathleen Akins for early discussion and Holly Andersen for continuing feedback. Thanks also to audiences at the Centre for the Study of Mind in Nature's workshop *The Role of Intentions in Communication* in 2016, Simon Fraser University's *Defining Cognitive Science* speaker series in 2017, and the *Mind & Language* workshop on gesture and communication in 2019. Finally, I would like to thank two anonymous reviewers of this journal for

[47] See, for instance, the literature emerging in support of "Language Deprivation Syndrome" as a clinical diagnosis: on this, see, e.g., Hall et al. (2017); Hall (2017); Glickman & Hall (2018); Gulati (2018).

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their detailed and helpful comments, and Safa Ahmadian for editorial assistance in preparing the final manuscript for publication.

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