Handbook of Understanding and Measuring Intelligence

Edited by
OLIVER WILHELM • RANDALL W. ENGLE
Humboldt-University, Berlin, Germany *Georgia Institute of Technology*

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UNDERSTANDING INTELLIGENCE

A Summary and an Adjustable-Attention Hypothesis

NELSON COWAN

THE DEFINITION OF INTELLIGENCE AND ITS THEORETICAL AND METHODOLOGICAL IMPLICATIONS

To introduce two often-considered questions—whether there is intelligent life on Earth and, if so, what form it takes—consider these evolutionary points (from Bower, 2003):

> The Stone Age was rough on community life, at least among animals trying to make a living in Africa. A range of species would move into a local habitat—gazelles, zebras, pigs, people, you name it—and take a few generations to establish the web of interactions that characterizes an ecosystem. After a millennium or so, dramatic climate shifts would then radically remodel the habitat, motivating the residents to leave. Eventually, a new collection of species would inhabit the area. . . . As these communities formed, dispersed, and reformed, one line of creatures always found a place in the mix—members of the *Homo* lineage, the ancestors of people today.
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> . . . the evolution of extended individual development, as in H. Sapiens, may have stoked an aptitude for learning and innovation that permits human adaptation to one habitat after another. (pp. 10, 12)

Weighing in with a more negative assessment of humans' survival intelligence, the unmanned expeditions to Mars have been met by several political cartoons in which the roving vehicles find signs that Mars used to be populated by humanoid beings, who became extinct after badly abusing the Martian environment and/or each other. The thought is that it seems too early in the history of human beings to congratulate ourselves for longevity as a species due to our intelligence.

Certainly, we hold survival of oneself and of the human species generally (or at least one's progeny) to be aims shared by most people, so that a definition of intelligence would have to include behavior that is helpful in promoting these aims. As Wilhelm (Chapter 21, this volume) discussed, Binet, an early pioneer in

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intelligence testing, ended up defining intelligence as the ability to adapt to novel situations. Although that is consistent with the view that humans have adapted to new environments throughout our history, there are problems with such a broad approach.

Problems With Taking Adaptability as the Theoretical Foundation of Intelligence Testing

It is problematic that the abilities needed to adapt to some novel environments differ from the abilities needed in others. For example, if new habitats are plentiful, a group of humans may benefit immensely from a peaceful demeanor that allows members of the group to work together to further their survival. However, that same peaceful demeanor may leave the group open to attack from other groups trying to capture the habitat, which becomes more likely if ideal habitats become scarce. So perfect adaptability would have to include anticipating events that may seem remote or far-fetched. Could a peace-like group adequately imagine that a never-before-encountered, warlike horde might appear on the horizon, and therefore could they plan an adequate defense? Similarly, can the generally peaceful, complacent citizens of modern democratic nations fully anticipate that other groups might be strongly motivated to displace or destroy them? Perhaps not. Many people appear to have been taken by surprise, for example, by the actions of Hitler's Nazis and, later, bin Laden's violent sect.

Fundamentally, do we wish to include factors of emotion, trust, gullibility, and so on as part of intelligence? Many related questions can be raised. What about the personality factors that distinguish between an individual who is basically cooperative versus another who usually appears cooperative to the public eye but is motivated primarily by social competition? (For a discussion see Geary, in press.) Is it impossible for a suicidal person to be considered intelligent? How about someone who selects a goal that most people do not share? Is intelligence the ability to pursue one's goal, which has to be taken as a given whatever it is, or is goal selection itself a function of intelligence? How about a stockbroker who is dishonest and incorrectly believes he will get away with it? If he correctly believes it? It is quite possible that we ultimately do want to consider such factors of emotion and personality in the definition of intelligence, and this has been a topic of much recent interest, as Roberts, Markham, Matthews, and Zeidner (Chapter 19, this volume) explain.

A wide-open definition of intelligence based on adaptability, including personality factors, emotional intelligence, and selection of goals and ambitions, poses rather unwieldy problems. For one thing, we currently do not have an integrated understanding of behavior with motivation, emotion, cognition, and social interaction rolled together. Also, it seems as if motivational and emotional factors change over time much more quickly than certain basic intellectual factors. We would not want, say, to judge a person "intelligent" for having just married happily and later judge the same person "unintelligent" for having divorced unhappily, even though greater intelligence might have led to better mate selection or better conflict resolution. These are viewed as complex life events that can make the same person seem well adapted at one point and poorly adapted at another point. In contrast, the search for a definition of intelligence has taken as one of its goals the identification of an intelligence rating that will stay relatively constant over time (throughout young and middle adulthood, assuming good health), that is, as an individual trait rather than a transient state.

Thus, there is some justification for breaking down the broad problem of intelligence into parts and asking, as an important part, whether intelligence might be defined more narrowly for many scientific and practical purposes on the basis of an ability assumed to be relatively stable within an individual: the ability to process information. That has been the basic thrust of psychometric intelligence testing ever since the early work of Binet (see Chapter 19, this volume), but there are still problems with it.

Information-Processing Ability as the Theoretical Foundation of Intelligence Testing: Some Unresolved Issues

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> a very general mental capability that, among other things, includes the ability to reason, plan, solve problems, think abstractly, comprehend complex ideas, learn quickly and learn from experience. It is not merely book learning, a narrow academic skill, or test-taking smarts. Rather, it reflects a broader and deeper capability for comprehending our surroundings—"catching on," "making sense" of things, or "figuring out" what to do.

The statement was later expanded (Gottfredson, 1997).

An inclusive shorthand for these skills is *information processing,* the ability to perceive and comprehend stimuli, categorize them, recall them later, and reason from them to make decisions. Although most tests of intelligence focus on this, there are important issues involved in measuring it. I will focus on two of them: issues regarding (a) the role of the motivation for studying human intelligence and (b) practical time constraints in intelligence testing.

The Role of the Motivation for Studying Human Intelligence. At least two chapters in this volume (Chapters 8 and 19) noted that there are two lines of research on intelligence. One line is motivated by the practical need to find out how well a given individual will perform in a job or academic setting. The other line is motivated by the more philosophical interest in what makes humans uniquely capable. One could easily argue that this philosophical interest is closely tied to others, such as what makes us human, what makes us experience the world in the way that we do, and so on.

A priori, there is no clear reason why these practical and scientific questions cannot assist one another. There is a long tradition of the two types of question being addressed together. For example, theoretically oriented cognitive psychology emerged largely from information-processing research carried out by the Applied Psychology Unit in Cambridge, England, related to practical issues of performance in the armed forces as well as issues of mental health, development, and aging (e.g., Broadbent, 1958); by artificial intelligence work with computers that sparked an interest in flow diagrams of information processing (e.g., Newell, Shaw, & Simon, 1958); and by the field of linguistics, which was rather applied and language specific in emphasis before the work of Chomsky (1957).

In the case of the psychometric testing of intelligence, however, the two motivations have led to rather divergent approaches. Ackerman and Beier (Chapter 8, this volume) noted that the practical branch of intelligence testing has shown that an individual's domain-specific knowledge counts for a lot, as tested by what Cattell (1943) termed *crystallized intelligence,* or Gc. Practical tests of intelligence also have incorporated tests of the ability to deal with new and cognitively demanding situations, of course, which Cattell termed *fluid intelligence,* or Gf. The main reason for doing so has been that Gf is expected to be less influenced by the specific learning opportunities that an individual has had. In practice, though, Gc and Gf are rather highly correlated and are bundled together to produce an intelligence score.

For the theoretical interest in the capabilities of humans, though, this equal footing of Gc and Gf will not do. The theoretical assumption that most researchers in this camp hold, I imagine, is that the correlation between Gc and Gf occurs because individuals with high Gf have a greater ability to acquire knowledge. Therefore, although knowledge may be practically important, differences in Gf comprise the factor that is viewed as a root cause of individual differences in intelligence and knowledge generally (although without denying the importance of environmental factors such as socioeconomic status).

An alarming gap in our knowledge, as Ackerman and Beier (Chapter 8, this volume) pointed out, is that there are few studies of the relation between Gf and real-world adaptations. A cursory electronic search that I carried out seemed to verify that there is this gap, with only a few exceptions. There are very many studies on the ability of intelligence, as well as of the *g* factor within intelligence tests, to predict job performance or academic success. However, these tests were typically not broken down to reveal the predictive value of Gc and Gf separately. Practical tests of knowledge are known to predict real-world performance, but the predictive value of Gf is, for the most part, still unknown after all these years. The theoretical prediction presumably is that Gf would play a large role in job or academic performance, inasmuch as success in those situations largely requires the acquisition of new information (Gc) and adaptation to new situations. However, it should be tested.

Another point that must be emphasized, regarding those interested in the philosophical nature of human beings, is that individual differences are not everything. Clearly, they provide one helpful method to understanding mechanisms of intelligence. If Mechanism X contributes to intelligence, then individuals who have more X should have more intelligence. In principle, though, there should be an emphasis also on ways in which individuals are alike as opposed to different. If Mechanism Y contributes to intelligent thought but is approximately the same in all normal adults, one can still manipulate Y experimentally, look for developmental changes in Y, and examine cross-species comparisons of Y and of intelligent behavior. All of these methods are relevant to the understanding of intelligence. They are all used, but sometimes individuals get caught up with one method and forget about the relevance of the others in forming a theoretical description of the mechanisms of human intelligence.

The methods may provide seemingly conflicting information. I can think of two actual cases in which individual differences show discrepancies from developmental differences, both of which are in the domain of serial recall (demonstrably relevant to intelligence testing because it is used in intelligence tests, in the form of digit spans). First, in irrelevant-speech tasks, memory for printed lists is poorer when irrelevant speech is heard during the list presentation (or during a retention interval) than it is when the list is read in silence. The susceptibility to irrelevant speech does not appear to correlate with list-recall performance in silence (Ellermeier & Zimmer, 1997), but this susceptibility decreases with age in childhood (Elliott, 2002). Second, when one measures the timing of spoken recall, preparatory intervals between the list and beginning of the response decrease across ages but do not correlate with span within an individual. This is in contrast to interword pauses in the response, which both change with age and correlate with span (Cowan et al., 1998).

Mechanisms that change with age in childhood but do not correlate with performance in adulthood can be informative. For example, they could be related to covert verbal rehearsal. Young children generally do not carry out such rehearsal, unlike adults (Flavell, Beach, & Chinsky, 1966). Rehearsal speed differences between adults appear important for recall (e.g., Baddeley, Thomson, & Buchanan, 1975), but for some processes, perhaps the rehearsal speed differences are not large enough to make a difference. Yet, the fact that such rehearsal takes place at all may be important. Regarding the empirical examples just given, rehearsal in mature individuals may reduce the susceptibility to irrelevant speech and may be associated with faster preparatory intervals.

The Role of Time Constraints in Psychometric Testing. Another factor that has a bearing on intelligence testing and how its results are interpreted is the practical limitation of time constraints. Intelligence tests depend on relatively rapid cognitive processing. Even if the test items are not timed, social and motivational considerations are such that the individual is only going to search for an answer for so long before guessing or replying, "I don't know." As Roberts et al. (Chapter 19, this volume) suggested, on intelligence tests, slow-working individuals are penalized for guessing at the end of the test.

In contrast, for many cognitive tasks, it may be that one can compensate to some extent for intelligence that is not extremely high, provided that enough time is available to complete the task. An example is writing a term paper. One can either retain factors to be considered in working memory (WM) or write them down systematically and know the important facts or look them up. Someone with lower intelligence might produce a paper comparable to a higher intelligence individual or might even come up with the same score on a classroom test by exerting more effort than a person of higher intelligence during the prior days of academic work or study. Theoretically, this difference between what is needed for cognition "on the spot," within intelligence tests, and cognition

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In support of time-limited tests, time often *is* very important in real-world situations. When a group of people is meeting to make a decision, the people who tend to have the most influence are those who can come up with an influential idea (or counter someone else's idea) in a timely manner and convey the idea in a way that takes into account what people in the group seem to be thinking and feeling. It is far less effective if one thinks of the crucial idea only after the meeting has ended. In nonsocial domains as well, time is often of the essence. Clear examples include warfare and uncommon emergencies, when quick thinking and quick access to one's knowledge base can be a matter of life and death. A less dramatic example would be success in various team sports. It would be quite interesting in such cases to assess the impact of fluid intelligence, general crystallized intelligence, and domain-specific knowledge.

Finally, for the comprehension of complex ideas, it is certain that compensation can take one only so far. Specifically, there is a need to understand relational complexity (e.g., Chapter 11, this volume) that cannot be totally met by writing things down. Suppose that a person is trying to understand the concept of a two-way statistical interaction. One can write down an example in which the means for Conditions A1, A2, B1, and B2 = 10, 1, 5, and 7, respectively. There is an A × B interaction because $(A1 - A2) \neq (B1 - B2)$. Even if one can use the formula, it takes a certain additional subtlety of thought to understand the meaning of the interaction. One must understand that the effect of one variable (the Level 1 vs. Level 2 effect) depends on whether one is referring to the A conditions or the B conditions. Even if all of the information is neatly written down, individuals without a certain level of fluid intelligence will just not be able to keep in mind all of the important factors at the same time and therefore will not be able to comprehend or explain the concept. A large amount of research on relational complexity (e.g., Andrews, Halford, Bunch, Bowden, & Jones, 2003; Zelazo & Frye, 1998) verifies this point. The relational complexity that one can comprehend may depend on how much can be kept in the focus of attention at one time, given the theoretical notion that associations are formed between all of the items in the focus of attention simultaneously (Cowan, 2001).

ROBERTS ET AL. ON SUGGESTIONS FOR THE FUTURE OF INTELLIGENCE TESTING

Roberts et al. (Chapter 19, this volume), after providing a historical overview, suggested four fundamental ways to improve intelligence testing in the future: (a) by placing greater emphasis on assessing specific cognitive processes, rather than taking a purely psychometric approach; (b) by assessing the speed of processing on various tasks and these speed scores within intelligence tests; (c) by extending our tests to include further sensory abilities, such as tactile, kinesthetic, and olfactory capabilities, which do not correlate well with other abilities; and (d) by including previously underemphasized constructs such as emotional intelligence, creativity, practical intelligence (tacit knowledge), metacognition, and wisdom (cf. Sternberg & Grigorenko, 2000). At least the first three of these probably depend on using computer technology in the testing process. The testing of emotional intelligence might benefit from it also; that testing could, for example, include video clips of facial expressions in action, which are then to be interpreted. The suggestions do seem to be on target, though they are so inclusive that there are difficult decisions ahead regarding which directions have highest priority.

Another attractive possibility that comes to mind is to use more derived measures from various tests, along with better use of outcome data (e.g., those that should be readily available in academic settings and in the armed forces). Concerning derived measures, there may be important information in the *interactions* between tests or subtests. For example, a relatively small WM or slow retrieval time may not be so bad if it is combined with the patience to write things down, look up or ruminate on information, and think carefully about the problem at hand until it can be solved. (I certainly have needed to rely on such devices to write this chapter.) In contrast, a low WM combined with a tendency to be impulsive and make snap decisions may be especially bad, leading to a poor prognosis. It should be possible to include at least some carefully selected two-way interactions in regression equations. This might lead to scores on comprehensive derived factors such as, in the example I have offered, "likelihood of working until a complex problem is solved." Some will score relatively high on such a measure by being able to solve problems quickly, whereas others will score equally high by slower, more persistent means. Perhaps it would also be possible to give week-long intelligence tests to some people in a controlled, experimental environment, so as to include questions and problems for which considerable preparation time is available.

Summary: Introduction to Defining and Studying Intelligence

In sum, a natural way to define intelligence, the ability to adapt to new situations, may be too broad for current methods when it includes social and motivational factors as well as information processing. Defining intelligence in terms of information processing is more tractable, but there are still important issues. There are at least two motivations for measuring intelligence: as a practical sorting tool and as a theoretical tool to understand the human mind. These can go together but also can be at cross-purposes. There also are problems with the fact that an intelligence test in a limited time (e.g., an hour) is used to predict outcomes taking much longer (e.g., months or years). Long-term motivational factors may compensate for some, though not other, deficiencies in intelligence. It will be possible to go in various directions toward improving intelligence tests in the future. This information must also be integrated with experimental, developmental, and cross-species comparative evidence.

SOME SPECIFIC ISSUES RELATED TO INTELLIGENCE

With this orientation in mind, I turn now to a discussion of several issues within chapters in the present volume having to do primarily with understanding, as opposed to measuring, intelligence. The issues are attention and executive control, knowledge and thinking, and developmental change and abnormality. The chapters to be discussed will be simplified somewhat so that each is taken to fit a single topic, but in reality, many chapters address multiple issues.

Attention and Executive Control

A question that pervades several chapters is whether attention and executive control can be considered synonymous with fluid intelligence (e.g., Kane, Bleckley, Conway, & Engle, 2001). Four relevant chapters in this book provide related, but subtly different, answers to this fundamental question.

Conway (Chapter 4, this volume): Executive Control and a Reductionist Approach. Conway calls his approach reductionist in that it looks for specific processes that influence intelligence, in contrast to a psychometric approach. He defines good (as opposed to greedy) reductionism on the basis that "validated," as opposed to "invalidated," "constructs and mechanisms are used to explain complex behavior." He argues that WM capacity and frontal lobe involvement are validated because much work has gone into determining what these constructs are and how they fit into processing. He adds that another sign of good reductionism is that knowledge is unified; one field of knowledge is not replaced by another, but they are connected and unified.

As a theoretical framework, Conway uses the model of Cowan (1988), in which the focus of attention includes only a subset of the temporarily activated information and the central executive represents the vehicle of voluntary control of that focus. It is not complete control, given reflexive orienting toward abrupt stimulus changes and inappropriate, prepotent responses to previously encountered situations.

It is important to backtrack a bit to discuss the conceptual basis of reductionism. By my understanding, it is defined with respect to levels of analysis. In human behavior, a high level of analysis is social interaction, a lower level is individual behavior, and a still lower level is the activity of neurons within the brain. Reductionism

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is explaining phenomena at one level of analysis (e.g., behavior) using phenomena at another level (e.g., neurophysiology). Intelligence tests and executive control both operate in the realm of behavior; therefore, it is not immediately obvious that explaining the first with the second is a case of reductionism. (When intelligence is explained in terms of frontal lobe mechanisms, it clearly is.) The intended conception may have been that intelligence tests examine behavior holistically, whereas cognitive processes such as executive control make up only part of this holistic behavior.

In either case, I see no basic problem with the use of "good reductionism." It becomes almost synonymous with scientific understanding, given that there is no way to understand causation without it. The problem is in making sure that one does not slip into greedy reductionism, though—no small task.

Here are some central examples. If the control of attention predicts performance on fluid intelligence tasks, additional questions still must be asked. First, are those fluid intelligence tasks and the tests of executive control unbiased in picking up variance in the real-world criteria? If both tasks tend to underemphasize how far one can go in the real world by using persistence (e.g., making the undesired prepotent response but then correcting oneself and moving on), then they are missing an important aspect of practical intelligence. Second, what should one conclude from findings that executive control predicts WM (and, potentially, fluid intelligence), even in a situation in which there is no memory load (e.g., Kane et al., 2001)? One can conclude that executive control is likely to be highly relevant. However, one cannot conclude that it is the only thing that is relevant. It may be that a situation in which there is a high memory load but little strain on executive control also predicts WM and fluid intelligence. Later, I will argue that that is the case, provided that verbal rehearsal is prevented. On a physiological level, I will argue that parietal areas may be as critical as frontal areas to WM.

Heitz, Unsworth, and Engle (Chapter 5, this volume): Capacity, Control, and Intelligence. This chapter is very much in line with the one by Conway (Chapter 4, this volume), and as a result, many of the same comments apply. The chapter goes on to introduce additional evidence that the control of attention is a general factor that cuts across domains and accounts for differences on standard WM tests and that the portion of the WM test variance that is picked up by the control of attention is the same as the portion that correlates with fluid intelligence.

The Conway and Heitz et al. chapters, taken together, discuss aspects of a study of the role of attentional control that, impressively, includes evidence for at least seven interrelated conclusions:

1. the relation between WM span and other intellectual skills is not entirely specific to a processing domain (e.g., Turner & Engle, 1989);
2. the relation between WM span and other intellectual skills is not totally a matter of knowledge or expertise in the processing task (e.g., Hambrick & Engle, 2001);
3. the relation between WM span and other intellectual skills depends on the availability of concentrated attention in the high-span individuals (e.g., Rosen & Engle, 1997);
4. the type of task in which storage and processing must be carried out together goes well beyond ordinary, simple memory span in correlating with intellectual skills (e.g., Engle, Tuholski, Laughlin, & Conway, 1999);
5. tasks that require attention but not memory (except holding in mind the goal of the task) are carried out better by high-span individuals than by low-span individuals (e.g., Kane et al., 2001);
6. superior WM performance depends on the involvement of brain mechanisms that help to carry out central executive processes (e.g., Gray, Chabris, & Braver, 2003); and
7. the attention-related WM processes are not related to all other WM processes (e.g., Engle et al., 1999).

In addition, Heitz et al. discussed a recently published submitted paper by Kane et al. (2004). That discussion deserves comment because of important methodological issues in Note 4. How is one to distinguish between the attention-related and attention-unrelated aspects of WM tasks? Engle et al. (1999) posited that there is a difference between WM and short-term memory (STM) tasks, the former meaning tasks with separate storage and processing components and the latter meaning simpler immediate-recall recall tasks. They assumed that STM = verbal ability, whereas WM = verbal ability + executive attention. Therefore, in a latent variable model, it was not the common component but the residual component unique to WM that was correlated with fluid intelligence. In contrast, Kane et al. used verbal and nonverbal WM and STM tasks and assumed that the common component was the one that reflects executive attention. Unlike the solution of Engle et al., they assumed that STM tasks involve some executive attention.

Given the assumption that STM tasks involve much less executive attention than WM tasks, one might have expected that a more successful approach would be one in which only the WM tasks were linked to the executive attention component. I do not know if such an approach was tried, but one reason it might not work is that spatial storage tasks correlate with measures of executive control much better than verbal tasks do (Miyake, Friedman, Rettinger, Shah, & Hegarty, 2001). What this suggests to me is that the separation of passive storage from executive attention is not as clean as it looks in the model. It may be that storage relies heavily on attention, except in situations in which verbal rehearsal helps to relieve some of the burden on attention (Baddeley, 1986).

One also must question the labels given to latent variables. There are separate variables for verbal storage and for spatial storage. It remains possible, though, that those two variables also include verbal and spatial domain-specific *processing*. In general, though, the model does help to support the notion of a general contribution of WM tasks, presumably related to attention, that contributes to fluid intelligence. I will argue, though, that it may not be specific to the control of attention and may also include other aspects of the focus of attention.

Kane (Chapter 9, this volume): Fluid Intelligence and the Frontal Lobe. This chapter summarizes a great deal of evidence that particular frontal lobe structures underlie the executive attention that is said to be the link between WM and fluid intelligence.

A difficult point in this area is that one can interpret frontal activity in more than one way. Is greater frontal activity in some individuals a result of better control of attention? That appears to be the case for individual differences among young adults. However, it does not necessarily explain the evidence in the elderly, who appear to have deficits in multiple brain areas and sometimes exert additional effort to compensate, which results in more widespread frontal activity in the elderly than in the young adults (e.g., Reuter-Lorenz et al., 2000). To resolve such discrepancies between the individual-differences data and the aging data, we need to have more sharing of methods between studies of these topics.

To the extent that the aim of examining executive attention and its brain representation is not practical but theoretical and philosophical (i.e., to understand the essential core of human thought), I would advocate an approach that is broader, yet not so broad as to include the whole brain. Cowan (1995) reviewed evidence that frontal areas of the brain were critically important for the control of attention but that parietal areas were more important for the focus of attention per se. Thus, for example, whereas frontal damage often results in deficits in planning and executive function, parietal damage more often results in deficits in awareness, such as hemispheric neglect (failure to attend to one half of space or one half of each object in space) and anosognosia (failure to be aware that one has a physical disability). What is important for WM and fluid intelligence, I would argue, may be the functioning of an integrated attention system, including at least portions of these two lobes and thalamic connections.

Oberauer (Chapter 22, this volume): Does Executive Control Equal WM Capacity? This chapter takes a more critical role and points out discrepancies in the executive attention account of WM and fluid intelligence. Key points in the chapter include at least the following:

1. There is no a priori definition of WM capacity; it must be refined through cycles of empirical work and theoretical reasoning.

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2. Particular correlations between measures can be interpreted in different ways, depending on one's theoretical commitments.
3. Factor structures are important in determining what is real.
4. What counts in WM tasks seems to be storage in the presence of processing, with little effect of the difficulty of processing.
5. Some STM tasks (e.g., some with spatial or semantic storage) correlate well with complex intellectual tasks, even though they do not include a separate processing component.
6. Some examples of what should be executive attention tasks, most notably attention-switching tasks, do not correlate well with the other executive attention tasks or WM tasks.
7. Therefore, it is unclear what is essential to WM tasks at this point.

I will suggest some possible resolutions of the mysteries and anomalies discussed here, as articulated in Points 4 through 7. Regarding Point 4, some new research suggests that what is important in the processing task within a WM task may be how often retrievals are necessary within the processing task. Barrouillet, Bernardin, and Camos (2004, Experiment 7) used a WM task in which the to-be-remembered items (printed consonants) were interleaved with the presentation of 4, 8, or 12 numerals to be read aloud as the processing task. The numerals (which fell in the range of 1–12) appeared during a total period of 6, 8, or 10 seconds. The number of numerals and the duration of their presentation were fully crossed, producing nine different varieties of processing task. It was found that recall was a decreasing linear function of the *temporal density* of numerals (number of items per second) in the processing task. Perhaps this is the case because retrievals prevent rehearsal, both by tying up the phonological loop and by reducing the possibility of re-retrieving items to be recalled from long-term memory during the delay period.

Regarding Point 5, what may be distinct about the STM tasks that correlate well with complex intellectual tasks and with WM is that they do not permit the use of phonologically based covert, verbal rehearsal. Either the material is difficult to encode verbally or the processing demands make it difficult to carry out rehearsal at the same time. Cowan (2001) argued that, under such circumstances, individuals must hold the items in the focus of attention to avoid its decay. An individual's limitation in doing so could be either in using executive attention to keep the items in the focus of attention during the necessary period, in the capacity of the focus of attention itself, or both.

Regarding Point 6, the reason that attention-switching tasks do not correlate well with other executive function tasks may be that it is not clear in those tasks what the optimal strategy or optimal outcome is. Should one aim for the fastest average performance or for better performance on one task at the expense of another? Regardless of WM span, some individuals may attempt to maintain preparation for whatever task was just carried out, whereas others may attempt to "wipe the slate clean" between trials. It may be that, if the strategy to be used were made part of the instructions, the results would correlate better with WM and intelligence.

Finally, regarding Point 7, I will suggest that the data Oberauer summarized may be consistent with an adjustable focus of attention that can zoom in for the sake of goal maintenance or zoom out to apprehend a set of up to about four separate chunks of information. Individual differences in either the zoomed-in or zoomed-out state (or in between?) can cause individual differences in WM.

Knowledge and Thinking

The importance of knowledge (reflecting Gc) is a strong counterpoint to the importance of executive attention (reflecting Gf). The more strongly two variables are related, the more heated the debate can become as to which came first or which is more important. Thus, if one looks at a detailed map of the Earth, the presence or absence of chickens is highly correlated with the presence or absence of newly laid chicken eggs. Which came first? The question does not arise between chicken and reptile eggs because the two are not so highly correlated.

Thinking abilities, including reasoning and metacognition, are interesting in this context because they must rely on both knowledge and basic processing capabilities, such as executive attention and STM storage. They can serve as a sort of a proving ground for how the two relate to one another, that is, how Gf interacts with Gc. Two chapters dealing with knowledge will be discussed, followed by two chapters dealing with thinking abilities.

Ackerman and Beier (Chapter 8, this volume): The Role of Knowledge. This chapter notes that tests of Gc and general intelligence have a proven track record in understanding practical differences in achievement, whereas tests of Gf have an unproven record. A theory of intelligence that was suggested was an "investment theory" in which personality and interests clearly do count. It was further pointed out that the structure of intelligence itself depends on knowledge. Thus, the same test may load on Gf in one sample but on Gc in another sample (e.g., children vs. adults). It was concluded that

> one might reasonably ask why so little effort has been devoted to the assessment of domain knowledge in the study of adult intelligence over the past several decades. The major reason for this, in our opinion, is the expense of time and effort on the part of the researchers and on the part of the examinees.

In my opinion, although the pivotal role of knowledge cannot be denied, there need not be as large a schism between Gf- and Gc-based approaches as investigators may assume. One reason little effort has been invested in domain knowledge may be that researchers interested in general principles of human cognition are looking for the origin of thinking and knowledge. They may well assume that Gf is the foundation of Gc and, therefore, the foundation of their own study.

Cognitive task analyses may resolve some of the difficulties of interpreting the currently used tests of intelligence. Take, for example, the point that the same test may load on Gf in one sample and Gc in another sample. Two studies have reported correlations between complex cognitive tasks and simple STM in young children of a magnitude comparable to what was found between these cognitive tasks and WM (Cowan, Towse, et al., 2003; Hutton & Towse, 2001). Typically, STM does not correlate with cognitive tasks that well in adults (e.g., Engle et al., 1999). The critical difference may be that adults use verbal rehearsal for the STM tasks, unlike children, and therefore have less need of Gf to carry out the tasks.

In our unpublished experiments, we have found less correlation than would be expected between verbal and nonverbal intelligence subtests. It may be that the modern exposure to video games and computer games, as well as school material and games influenced by intelligence tests, has invalidated the current nonverbal subtests to some degree. They were designed to determine how individuals deal with new situations, but the situations may not be so new anymore. This may explain the rise of intelligence scores worldwide—the Flynn effect discussed, for example, by Roberts et al. (Chapter 19, this volume). So the tests may need revision, even though the basic principles of their design are sound. The problem is in the shift in the population's knowledge over time.

Hambrick (Chapter 20, this volume): The Role of Domain-Specific Knowledge. This chapter, like that of Ackerman and Beier, stresses the importance of knowledge for intelligence. One key difference is that the chapter ends with a consideration of how knowledge and WM capacity interact. Hambrick examines the possibility that "having specific knowledge can replace having to exercise WM" (Ackerman & Kyllonen, 1991). He finds that participants with high WM sometimes are able to put higher domain knowledge to better use and therefore rules against the suggestion of Ackerman and Kyllonen (1991).

I have no disagreement with the substance of the chapter, but I believe that the framing of the question may require scrutiny. It is clearly evident in the data that were shown that someone with, say, high domain knowledge and only medium WM capacity can perform as well as another individual with lower domain knowledge and higher WM capacity. In that sense, knowledge clearly *can* replace WM capacity, which seems closer to what Ackerman and Kyllonen (1991) probably meant. That description is not inconsistent with the additional finding that higher span individuals make better, not poorer, use of a particular level of domain knowledge.

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Wilhelm (Chapter 20, this volume): The Role of Reasoning Ability. Wilhelm provides a taxonomy of reasoning abilities and wonders why humans do not follow logic consistently. He notes that a major determiner of reasoning success is the number of mental models that are compatible with the premises. He tests alternative models of reasoning abilities, and the major new finding is an absence of a pure difference between deductive and inductive reasoning methods. What does seem to exist is a Gf factor surrounded by verbal, figural, and quantitative varieties of reasoning.

My own opinion as to why humans do not reason consistently would include three main factors. First, humans may not interpret the question in the same way as it was intended. Second, they may not be able to carry out the computations necessary to reason correctly in a particular case and may resort to a heuristic that is not consistently accurate. Third, their own experiences may override the standard logic. I will comment further on the last two factors.

Regarding carrying out computations, the number of mental models that is needed seems closely tied to both the limit in how much can be held in attention at once (Cowan, 2001) and the limit in relational complexity (Andrews et al., 2003). To be completely comprehended, a 2 × 2 statistical interaction might be expected to require four mental models, one for each combination of variables. If the concepts of capacity in chunks, relational complexity, and mental models could be combined, they might provide a "unified theory" of processing and storage demands on WM (at least in the absence of separate sources of interference).

Regarding experiences that conflict with reasoning, consider the following example. If reasoning leads to the inference that a particular store must be closed on Saturdays, but one has shopped there on Saturday several times recently, it is tempting to assume that something went wrong with the logical process and that one's prior experience is more likely to be correct. Thus it is that reasoning (Gf) can come into conflict with experience (Gc).

Thus it also is that the more time has passed since knowledge has been accrued, the more likely it is that the knowledge is out of date. In a quickly changing society especially, the old Gc of elderly individuals may tend to be distrusted by youth, leading to some discounting of their wisdom.

Hertzog and Robinson (Chapter 7, this volume): The Role of Metacognition. This chapter convincingly emphasizes the importance of metacognition, or knowledge about one's cognition, as a potential contributor to intelligent performance. It is a "potential" contributor because, short of training studies, it is difficult to know whether metacognition is a cause of intellectual advances, a result of intellectual advances, or a little of each.

Here, it seems to me that an interactional approach might be very interesting. It only helps to know about a particular processing strategy if one has enough resources, WM capability, and knowledge to execute it successfully. Perhaps if we were able to measure basic, strategy-free aspects of processing successfully, we could then define metacognitive skill as the ratio between some measure of the strategies an individual understands and some measure of the strategies that he or she presumably can employ successfully, given his or her level of basic processing. That ratio would reflect whether the individual knows enough strategies to maximize his or her performance. It is clearly a difficult issue to resolve empirically however.

Developmental Change and Abnormality

Developmental change includes both childhood development and aging. It is a challenge to determine whether the increases in intelligence that occur throughout childhood, the decreases that occur with aging, and individual differences within an age group are all caused by the same factors or different ones, and figuring that out would provide quite a boost to our understanding of what it takes for intelligent behavior. Abnormal development also can yield important information, especially regarding the link between deficits in one process or domain and deficits in another process or domain. Three chapters examined these topics.

Pascual-Leone and Johnson (Chapter 11, this volume): The Construction of Intelligence in Childhood. This chapter explains the detailed theory of cognitive development put forward in previous publications by Pascual-Leone. The theory is comprehensive and includes various operators that may change with development. Four critical ones are the M operator (a scheme activation resource, which is a limit in how many independent schemes can be active at once), the I operator (an attentional interrupt, used for schema inhibition), the F operator (a field mechanism that produces stimulus-response compatibility), and the E operator (the currently dominant set of executive schemes). M, I, and E are said to be prefrontal functions dependent on attention, whereas F is said to come from lateral inhibition locally in the brain representations. The growing cognitive ability of children is said to coincide with an increase not only in learning but also in the M space up to a mature level of seven schemes. Larger M values allow more complex interrelations between elements to form more complex ideas (similar to Halford and collaborators, with whom these authors disagree on other grounds).

The authors state that the stable developmental change that coincides with increases in M space can be observed only in situations that are termed *misleading*. This proviso seems similar to what Conway (Chapter 4, this volume) and Heitz et al. (Chapter 5, this volume) suggest. Proactive interference from similar stimuli in previous trials produces a conflict, and WM is needed to overcome that interference. In the absence of interference, though, the processing limits do not apply as prepotent responses tend to be correctly used.

This theoretical framework is promising and exciting, although it is bound to remain controversial as it does not seem possible to test all of its explicit assumptions at once. Like many mathematical models, the application of the model to a particular situation seems to require some judgment that is open to question.

One question in need of more resolution is how a "misleading" situation is defined. For example, consider a situation in which an array of small, colored squares is followed by a second array, identical to the first or differing only in the color of one square (Luck & Vogel, 1997), with the task being to indicate whether the arrays are the same or different. The number of squares per array varies from trial to trial. (In one version, a single square in the second array is cued for a decision and is the square that changed, if any did.) Colors are sampled with replacement, and a changed square is allowed to change to any color, including a color already present elsewhere in the array. This task can be carried out easily with up to four items per array, with quickly diminishing accuracy after that. We have found smaller capacity limits in this task in children (Cowan, Elliott, et al., 2003). Therefore, it seems that the situation is capacity limited and, most likely, susceptible to M space limitations. It also correlates with measures of Gf (Raven's Progressive Matrices; Stanford-Binet Pattern Analysis subtest).

What is the misleading information in the array comparison task? One could suggest that it is misleading that a changed square may be incorrectly judged unchanged because it matches a color elsewhere in the array. Yet, a pattern similar to the overall data (but at a higher level) occurs when examining only those trials in which the changed item was changed to a color that did not appear elsewhere in the array. It is still possible to define *misleading* to include proactive interference from previous trials with similar materials, but then almost any memory load will count as misleading in a multitrial experiment.

An alternative suggestion that I would make is that the situation does not have to be misleading; if it is not misleading but verbal rehearsal is unfeasible, the task will be constrained by M space relative to the number of schemes that must be activated. This can explain the aforementioned correlations between Gf and simple STM tasks in children too young to rehearse (Cowan, Towse, et al., 2003; Hutton & Towse, 2001).

In order to count M = 7 in adults, I wonder how one explains performance in a situation such as ordinary memory span, in which only about seven items can be recalled. Using seven slots to account for seven items in the list leaves no room to use slots to carry out the span procedure. One solution is that the span procedure is quickly automated and therefore does not occupy any M space. However, then there is a question of how one accounts for the finding that adults' recall is limited to about four items in a running-span procedure, in which many items are presented and the end of the list is to

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One account of performance in the two types of span tasks is as follows. There may be a simple capacity limit of about four chunks of information in adults (Cowan, 2001) and not seven, as Pascual-Leone has proposed. In a memory span (or other verbal STM) task, performance is boosted above that limit because of the contribution of rehearsal and/or grouping items together to form multiword chunks. In running memory span with a fast, auditory presentation (we used four digits per second, with compressed speech), that grouping and rehearsal is not possible because there is not enough time and the end point is unpredictable. Hence, it appears to provide a better measure of the capacity limit. In support of that view, running span correlates with intelligence much better than simple memory span (e.g., Mukunda & Hall, 1992), with the possible exception of young children who do not rehearse, for whom both types of measures must rely on capacity.

Lövdén and Lindenberger (Chapter 12, this volume): Development and Aging. These authors argue for the need for an individual approach to aging. They describe a "dedifferatiation hypothesis." Essentially, a high general intelligence allows more investment in one domain at the expense of others, whereas individual profiles of abilities are not as prevalent when general intelligence is lower. With adulthood, differentiation is increased, whereas with aging, differentiation declines again along with general intelligence.

This fascinating trend requires some thought given another trend: that heritability estimates of intelligence increase with development in childhood (Bishop et al., 2003). For this to be the case, should we assume that Gc and the profile of abilities contribute a great deal to the increase in heritability and not just Gf? That is, as individuals come into their own as adults, do they grow into an inherited profile of tendencies? If so, it truly provides an argument favoring the importance of individual differences in multiple aspects of intelligence, despite the fact that they may all correlate with one another highly in the population.

Swanson (Chapter 23, this volume): Aspects of WM, Intelligence, and Learning Disabilities. Swanson discusses some fascinating and unsettling dissociations between factors underlying learning disabilities. The factors do not hang together as well as they do in normal populations. Children with learning disabilities do poorly not only in tasks requiring phonological abilities but also in tasks requiring WM, even with psychometric intelligence controlled. It was remarked that "isolated components of the phonological and executive systems" seem to be affected.

There also are important dissociations between executive functions that are affected in different disorders. Children with learning disabilities suffer from poor WM and, often, difficulty holding information in the face of interference, whereas children with attention deficit and hyperactivity disorder (ADHD) tend to have normal WM but poor planning abilities, impulsivity, and distractibility. It is as if they are affected by disabilities in different types of behaviors that all have been classified as central executive functions.

The most straightforward interpretation of these group differences might be a reductionist one in which damage is postulated in different parts of the brain. This sort of approach seems reasonable given that brain damage can be limited to certain brain areas or neurotransmitter systems, cutting across multiple behavioral functions along the way. It does suggest that WM cannot simply be equated to executive attention if, in fact, the functions of executive attention are so dissociable (see also Chapter 22, this volume).

It may be worthwhile to step back a bit, as well, to place the disabilities within the context of psychometric testing and how it is carried out. We might distinguish between first-order and second-order disabilities. A first-order disability would be that in which an individual is near the low end of the population distribution of scores. With respect to intelligence, the relevant disability is retardation. In contrast, a second-order disability is based on scientific knowledge about the usual structure of intelligence test results. The usual structure includes a positive manifold between test results; different subtests tend to have substantial positive correlations with one another. Therefore, when we find an individual who has a large discrepancy between different subtests, it is not usual, and we classify the lower subtests as the result of a disability. The inference is that the cause of the disability is a medical disease, but that does not necessarily follow logically. Someone has to be at the low end of each distribution, and someone has to fit the positive manifold less well than others.

What may be most informative is the high frequency of certain profiles of ability and the paucity of other, theoretically possible profiles. It may point to true syndromes. At least, this would be informative if we could be sure that the findings were not influenced by the filtering influences of the particular theoretical lenses worn by clinicians who diagnose the children.

A THEORETICAL PROPOSAL: THE ROLE OF AN ADJUSTABLE ATTENTIONAL FOCUS

In the interpretation of various findings reviewed throughout this chapter, I have suggested a view that is intended to account for WM performance, including its normal mechanisms, developmental trends, and individual differences. Like Engle et al. (1999), I believe that attention-related processes of WM are critical in accounting for fluid intelligence. However, instead of postulating that the control of attention for goal maintenance in the face of interference is uniquely important (e.g., Kane et al., 2001), the proposal is that a general, flexible attentional system is important. Attention can *zoom in* to hold onto a goal despite the presence of interference, but it can also *zoom out* to apprehend up to about four separate chunks of information at once in the absence of interference. Presumably, there is a trade-off, with a smaller capacity in the presence of interference than in its absence. It is theoretically possible that individual and developmental differences in both manners of attentional capability depend on aspects of the control of attention and on aspects of frontal lobe function, as Kane (Chapter 9, this volume) suggests. However, I will describe evidence that there may also be differences in the scope or capacity of attention, which depend more on parietal lobe function. As described in the discussion of Kane's chapter, Cowan (1995) reviewed evidence that the frontal lobes are more critical for the control of attention but that the parietal lobes are more important as the seat of attention. Most important, it is only half true to state that WM capacity is not about how many items can be stored in WM. I will review evidence for a capacity limit followed by evidence for its relevance to intelligence.

Evidence for a Basic Attentional Capacity Limit

The scientific community has had an ambivalent relationship with the notion of a capacity limit. Miller (1956) famously proposed a limit of seven but also proposed the concept of chunking, or grouping items together to allow more items to be retained. The limit of seven items in verbal STM is not easy to interpret because these items may be rapidly encoded as a smaller number of multiword chunks. (Telephone numbers may be presented in groups precisely to allow that chunking to be carried out.) It also may be possible to exceed the basic limit by using mental tricks such as verbal rehearsal (Baddeley, 1986) to recirculate items in and out of the capacity-limited mechanism, keeping accessible a number of items exceeding the basic capacity.

In fact, Miller (1989) explained that he did not really have any investment in the alleged capacity limit of seven and used it only as a rhetorical device to link together two areas of his research, for an invited address that otherwise could have seemed incoherent. Many sophisticated readers have been skeptical that there is any fixed limit, even as others have assumed a limit of seven.

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gravitational constant can be observed clearly only if wind resistance is controlled (e.g., in the observation of planetary motion as opposed to the motion of objects on Earth). Similarly, there is evidence for a basic capacity limit, but only if one can take into account the effects of chunking and rehearsal. Broadbent (1975) pointed out this basic limit, citing evidence in several areas. For example, in the retrieval of items from a category of long-term memory (e.g., name all of the states of the United States.), the names are retrieved in small bursts of typically four or fewer items. It is as if the "bucket" of limited-capacity WM has to be dipped into the enormous "pool" of long-term memory and then poured out repeatedly, until no more relevant information can be retrieved.

Method of Limiting Chunking and Rehearsal. Cowan (2001) extended this approach by reviewing a wide range of phenomena for which it seemed that chunking and rehearsal were not possible. For example, there are many studies in which a list of words or digits is presented during "articulatory suppression" to prevent verbal rehearsal (which presumably also prevents grouping items together to form larger chunks). If the words are presented in spoken form, there is no danger that they cannot be recognized during suppression. Each item that is recalled presumably remains a separate one-item chunk in WM. Therefore, the number of items reflects the number of chunks recalled. These studies consistently indicate that adults can recall only about four items in the correct serial positions under such circumstances. Other examples include the running memory span and visual-array comparison procedures discussed above. In running span, the rapid presentation with an unpredictable end prevents effective rehearsal or chunking; in visual array comparisons, it is the relatively quick presentation of many items at once that does so. This is not a perceptual problem; presentation time does not matter, provided that it is under about a half second (Luck & Vogel, 1997; Sperling, 1960). In each case, an average of about four items is recalled, presumably equaling about four chunks, given the impediments to multi-item chunking during the presentation. It is tentatively assumed to reflect a limit in the capacity of the focus of attention.

What strengthens the conclusion that these cases reflect a constant capacity limit is (a) the convergence of capacity estimates across many procedures and (b) the finding of a constant capacity limit across many set sizes. Exemplifying the latter, Cowan, Nugent, Elliott, Ponomarev, and Saults (1999) presented, in a developmental study, spoken digit lists during a distracting, silent video game that also tied up phonological processes. Most spoken lists were ignored, but occasionally there was a signal to recall a list that had just ended. The experimental logic is that the use of attention to rehearse and group stimuli is prevented, allowing a clearer view of how attention can be used specifically to apprehend items from an auditory memory stream that outlasts the list. It was found that adults were able to recall about 3.5 items on average, no matter whether the list length equaled the participant's span or was one, two, or three digits shorter than that. This pattern was in striking contrast to a control condition in which digit lists were attended. Then, the number of digits recalled increased markedly across list lengths, with no observation of a constant capacity. Children showed the same pattern of results but with a smaller capacity in each condition.

Note that one can distinguish between the capacity-limited portion of WM and the unlimited portion of activated memory. In the unattended-speech task, presumably all nine digits are in an activated state throughout most of the experiment. However, binding between a digit and its serial position in the list is necessary for correct performance, and it is that binding to which the capacity limit actually applies.

In the visual-array comparison task of Luck and Vogel (1997), described above, the important binding is between a color and its location in the array. Cowan, Elliott, et al. (2003) found developmental increases for that task also. A simple formula to estimate how many items are apprehended, correcting for guessing (Cowan, 2001, p. 166), indicated that capacity was stable across array sizes at three to four items.

There is evidence that maintenance in this task is attention related. It is affected by having to recite a verbal memory load of seven random digits during the interarray interval. Yet, it is unaffected by having to recite one's own seven-digit telephone number, so the effect cannot be attributed to blocking rehearsal (Morey & Cowan, 2004). Performance is also affected to a surprising extent by the simple task of having to identify a tone as quickly as possible (Stevanovski & Jolicoeur, 2003).

A recent functional magnetic resonance imaging (fMRI) study of the visual-array comparison task (Todd, Marois, & Gauthier, 2003) showed that two small brain areas, the intraoccipital and intra-parietal sulci, displayed activity during an interarray maintenance period that increased as a function of the array size but only up to the behaviorally observed capacity limit of three to four items. No other brain area was capacity sensitive in that manner, though frontal areas responded to the need to maintain activity. Ruchkin, Grafman, Cameron, and Berndt (in press) summarized a great deal of evidence toward the notion that the frontal areas hold "pointers" to representations in posterior regions of the brain and that the pointers keep those representations active. In my terms, this may reflect the frontal control of attention and the parietal seat of attention.

It is not yet clear whether the capacity limit occurs because of limitations in how many pointers can be held in the frontal regions or how much information can be included within the attended representation in the parietal region. Current physiological theories of WM limits are based on the concept that the features of an attended item are represented by synchronous neural activity and that capacity limits occur to avoid cross-talk between the features of different objects (e.g., Luck & Vogel, 1998), which would tend to favor a limit in the representation itself as opposed to a limit in pointers within the frontal lobe. However, this is very speculative.

Method of Known Chunks. The methods presented so far rely on an assumption that rehearsal and chunking has been prevented, an assumption that not everyone will buy. As a more direct approach, Cowan, Chen, and Rouder (in press) decided to control the formation of chunks (see also Tulving & Patkau, 1962). In a training phase of the experiment, words were presented four times each. However, words differed in how often they were paired with a mate. In the four-paired condition, each time a word was presented, it was followed immediately by its designated paired-associate word. In the two-paired condition, the familiarization to the words was the same, but the familiarization to the pairs was less: Each word was presented twice as a singleton and twice with its designated paired-associate word. In the one-paired condition, only one presentation was in pairs, and the other three were as singletons. In the zero-paired condition, only singletons were presented, four times each. (There were designated pairs of words, but the participant had no way to know this yet.) There also were words that were not presented at all in this phase of the experiment, but they are unimportant in explaining the main conclusions.

The remaining phases were cued recall and serial recall, phases that were presented in either order for different participants. The cued-recall test (typing the second word in a pair upon presentation of the first) showed steadily increasing performance across training conditions (no-study and zero-, one-, two-, and four-paired conditions).

In serial recall, lists of eight words were composed of four pairs in a row from a single training condition. The task was to write the words in the order that they appeared. As in cued recall, there was a marked improvement in performance across training conditions. However, the main empirical question was whether this improvement was a result of an increase in the *number* of chunks recalled or only in the *mean size* of chunks recalled. We had several types of information about the identity of chunks. We used information about associated pairs of words recalled in adjacent positions versus recall of only one item or recall of the two items separated or in reversed order; information from a mathematical model to take into account that two items from an associated pair might be recalled separately, yet still in order; and information from cued recall as to which pairs had been learned. All of these sources of evidence led to the conclusion that the number of chunks recalled stayed fixed across the zero-through four-paired conditions, at about 3.5 chunks per list. What differed between these conditions was just the mean size of chunks. (Additional supporting analyses are provided by Cowan, 2004.)

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people. For example, Ericsson, Chase, and Faloon (1980) showed that an individual could be trained to recall about 80 digits. Still, the present assumption is that the basic capacity applies and that only about four chunks of information can be held in the attention-related portion of WM at once. To recall the list, one would have to be shifting attention from the super-chunk level to the chunk level, to the level of digits within a chunk in order to recall them, and then back up to higher levels, in repeating cycles, until the list has been recalled.

Relevance of the Capacity Limit for Intelligence

Various procedures that I have taken as an indication of the basic capacity limit, presumably the limit in the attention-related portion of WM when zoomed out to apprehend multiple items, have been shown to correlate with measures of intelligence and with Gf. Mukunda and Hall (1992) found this in a meta-analysis with a mixed-age sample. Running memory span correlated with aptitude at $r = .40$, almost as high as reading span ($r = .43$) and higher than counting span ($r = .28$). Cohen and Sandberg (1977) had related findings.

In our own, still-unpublished research with elementary school children and adults (Cowan, Elliott, et al., 2003), we found correlations between two measures of Gf (Raven's Progressive Matrices and the Pattern Analysis subtest of the Stanford-Binet test) and three measures of capacity (running span, visual-array comparisons, and an auditory analogue of that task based on tone sequences). Respectable correlations were obtained, in the range of .4 to .5 for the Raven's test after age group was partialled out (and almost .6 with age group included). We also had standard measures of WM (listening and counting spans) and STM (digit span), as well as measures of Gc (the Peabody Picture Vocabulary Test and the Vocabulary subtest of the Stanford-Binet test).

We investigated the relation between different types of span and intelligence in several ways. It was possible to carry out all orders of stepwise regression and, on that basis, to determine the shared and unique contributions of three sets of predictors at a time to a dependent variable (see Chuah & Maybery, 1999). For the *g* factor in intelligence (with age contributing to the variance, not age normed), our predictors were standard WM tasks, digit span, and capacity estimation tasks. WM tests accounted for .59 of the variance in *g*, capacity tests accounted for .41, and digit span accounted for .34. Of that variance, .27 was shared by all variables, and the total variance accounted for was .62, not much higher than what WM tests alone accounted for.

We were concerned that part of the additional variance accounted for by the WM tasks might be domain-specific skill. In our most exacting analysis, we factored out age group and Gc and used the residual variance in Gf as the dependent variable. The residual amount of variance was small, of course, given the powerful variables that were partialled out initially, but the pattern was clear. Digit spans accounted for 0. The two standard measures of WM together accounted for .11 of the residual Gf variance, whereas the measures of capacity accounted for .07. Of this variance, .05 was shared between them. But what was the basis of the slight superiority of the standard measures? It turned out that only counting span contributed anything to the Gf variance that was unique to the standard WM tasks (.06). That may reflect a domain-specific nonverbal skill, whereas the rest of the ability contributing to Gf was shared between the standard WM tasks and the capacity tasks. (The exact numbers are nonfinal as we are in the process of exploring different derived measures for each task.)

More evidence is needed, but at this point, it seems at least plausible that attention operates as a system in WM and that the integrity and functioning of the system in both the zoomed-in and zoomed-out modes are germane to intelligence.

In another experiment, Cowan, Elliott, et al. (2003) found that running span and the unattended-speech task of Cowan et al. (1999) were slightly better predictors of high school grades (with significant correlations of $r = .33$ and .30, respectively) than were the standard WM tasks (with insignificant correlations of .24 for counting span and .23 for listening span). It is not yet clear how to interpret these results; perhaps high school fundamentally is an unattended-speech task! There are not yet pure measures of capacity, and the role of attention in these measures is still in need of further examination. In any case, it behooves the WM research and intelligence research communities to obtain the simplest measures that do what they are supposed to do. I hope to have demonstrated that a flexible attentional system may serve as an important topic on which measures might be constructed for future exploration.

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