Habilitation à Diriger des Recherches

James R. Schmidt

Université Bourgogne Franche-Comté

Experimental and Neural Network Investigations in Learning, Attention, and Cognitive Control

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Acknowledgements

An academic career is, of course, not built on one's own. In that vein, I am indebted to the numerous colleagues and collaborators that have assisted me in my early career development, those who inspired me toward new research directions, or those who have simply sat down with me to hash out new ideas. It would be impossible to list everyone, but I acknowledge below those to whom I am particularly indebted.

I would like to begin by thanking Jim Cheesman for helping to cultivate my early interest in cognitive psychology in general and in the Stroop effect in particular. I would also like to thank everyone else at the psychology department of the University of Saskatchewan (e.g., Ron Borowsky, Valerie Thompson, Jamie Campbell) for our many useful discussions.

I would similarly like to thank everyone in the cognition department at the University of Waterloo, especially my fellow graduate students and Derek Besner, my graduate supervisor, for my time as a Master's and doctoral student.

Additional thanks are of course due to Jan De Houwer, who saved my academic career from a funding crunch more than once, and the other members of the Learning and Implicit Processes lab (LIPlab) at Ghent University, in addition to everyone in the Department of Experimental Clinical and Health Psychology and the Department of Experimental Psychology.

I would be remiss not to thank all those in my current laboratory in Dijon, especially Bénédicte Poulin-Charronnat, Bob French, and Annie Vinter who played key roles in finding me a job in a time where tenure-track positions are like unicorns (only a myth) and helping me navigate the unfamiliar world of French academia.

I would also like to thank my family, who have been unconditionally supportive of my academic pursuits... even though it has meant me voyaging further and further away from home to follow my scientific dreams. Finally, I must thank my girlfriend, Christelle Pêcher, for supporting me no matter what and who, in a certain indirect way, is responsible for this Canadian's move to "Team France."

Introduction

I am currently a UBFC-ISITE International Junior Fellow (associate professor) working in the Laboratoire d’Etude de l’Apprentissage et du Développement (LEAD) at l’Université de Bourgogne. I completed my B.A. in psychology with high honours and a minor in philosophy at the University of Saskatchewan in 2005. In my last years, I worked under the supervision of Prof. Jim Cheesman. I then completed my Master’s in 2007 and my Ph.D. in 2009 at the University of Waterloo under the supervision of Prof. Derek Besner. I then worked as a postdoctoral researcher at Ghent University under the supervision of Prof. Jan De Houwer. I currently reside in France, but I am a Canadian citizen.

RULE I

We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.

– Isaac Newton, 1726 –

I am a cognitive psychologist with a background in learning, attention, and computational modelling. I believe strongly in the value of integrative research that seeks parsimony (ala the Newton, 1726/1846, quotation above) while exploring research across the sometimes arbitrary boundaries of paradigm-focused subfields. In that vein, my research interests have been rather scattered across different domains, but with common threads linking them together.

I have done considerable research on human contingency learning and evaluative learning. This work includes the development of the colour-word contingency learning paradigm, which is now used by numerous labs worldwide (e.g., Colin MacLeod, Céline Lemercier, Maria Augustinova, Nart Atalay, Joseph Tzelgov, Yoav Bar-Anan, Eliot Hazeltine, Toby Mordkoff, Chris Blais, etc.). My work in this domain has covered a variety of issues, including basic questions about how we learn to associate events in our environment to how we acquire likes and dislikes. This learning psychology research often intersects with my work in other domains where I have, for instance, considered the implications of learning principles on purported measures of attentional or executive control.

I also have a background in research on cognitive and attentional control, most notably in the conflict monitoring and task switching domains. In this research, I have investigated phenomena in the attentional and executive control domains, but through the lens of a learning perspective. In particular, certain phenomena (e.g., proportion congruent effects, switch costs, etc.) are typically presented as evidence for cognitive control. As I argue in my research, these effects can often alternatively be explained by much more basic learning processes, such as contingency learning, rhythmic responding biases, and feature binding. My general goal is to study human cognition through the perspective of a more broadly-focused memory framework (e.g., rather than appealing to very vague and task-specific “cognitive control” homunculi).

My work in the attention domain also links well with applied and developmental work at LEAD. For instance, Jean-Pierre Thibaut, Aurélia Bugaiska, and Robert French have done work on attentional processes in children and ageing populations (e.g., Bugaiska & Thibaut, 2015; Clarys, Bugaiska, Tapia, & Baudouin, 2009; Thibaut, French, & Vezneva, 2010). Their expertise in developmental psychology combined with my experience in the cognitive control domain could thus result in interesting collaborations. For instance, many claims have been made about attentional control over the lifespan using the paradigms that I frequently work with (e.g., Bugg, 2014b). My work would suggest that this past work may have actually been studying lower-level learning processes, and not cognitive control per se. Reinvestigation of these issues with better-controlled experiments (e.g., Schmidt, 2013a) might therefore result in new insights into development (Lemercier, Simoës-Perlant, Schmidt, & Boujon, 2017) and the origin and progression of psychological disorders (e.g., E. Abrahamse et al., 2016).

I also have expertise in computational modelling (e.g., neural networks). For instance, I programmed and work frequently with a Java-based neural network model that learns via episodic storage and retrieval (Schmidt, 2013a, 2013c, 2016a, 2016b, 2018; Schmidt, De Houwer, & Rothermund, 2016; Schmidt, Liefooghe, & De Houwer, 2017; Schmidt & Weissman, 2016). A major goal of this modelling project is to demonstrate how a few basic assumptions about how we store and retrieve episodic memories can have wide applicability across a range of research domains: skill acquisition, contingency learning, binding, timing, attentional control, and more. A key goal of mine is to further increase the breadth of scope of this neural network. For instance, sequential learning, action sequencing, and temporal perception are important aspects of human cognition that have yet to be explored in the PEP framework. Borrowing aspects of some of the models from my current lab, such as TRACX (French, Addyman, & Mareschal, 2011), PARSER (Perruchet & Vinter, 1998, 2002), and GAMIT (French, Addyman, Mareschal, & Thomas, 2014) could allow for one coherent framework that can learn regularities that occur in series across time.

I am a natural team player, having collaborated with scientists around the globe (France, Canada, Belgium, US, UK, Germany). I am also an excellent leader, as indicated by my numerous first-author publications. I further feel that I have worked hard to produce quality contributions to my field, rather than taking the easy road of publishing high-quantity incremental research. I feel that this has paid off: I have not only published frequently in high impact journals, but my novel contributions have had impact in the learning, attention, and cognitive control domains (e.g., over 1200 citations). I also have a strong record of securing external research funding. This includes Canada Graduate Scholarships (both Master's and Doctoral) from the Natural Sciences and Engineering Research Council of Canada, a Postdoctoral Researcher Mandate (and renewal) from the Research Foundation – Flanders, and an International Junior Fellowship from the UBFC. I am highly motivated, passionate about science, and enthusiastic about future supervision of Ph.D. and Master's students.

This thesis is divided into five chapters. The first chapter covers my research on the Stroop effect. The second chapter covers my research on human contingency learning, most notably my work with the colour-word contingency learning task (and variants thereof). The third chapter covers my research on attentional control, primarily my work critiquing the highly popular conflict monitoring account. The forth chapter covers my research on neural network modelling, which is closely entwined with my experimental work discussed in the second and third chapters. The fifth and final chapter covers some miscellaneous research that does not fit as clearly in one of the above-mentioned categories. This work includes some work on pragmatics in formal reasoning, temporal learning, and task switching.

Curriculum Vitae

James R. Schmidt

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Education

2009 Ph.D. in Cognitive Psychology
University of Waterloo
Advisor: Derek Besner

2007 M.A. in Cognitive Psychology
University of Waterloo
Advisor: Derek Besner

2005 B.A. High Honours in Psychology (minor in Philosophy)
University of Saskatchewan
Advisor: Jim Cheesman

Research Experience

2018 – Associate Professor
Université Bourgogne Franche-Comté (UBFC) / Université de Bourgogne

2009 – 2018 Postdoctoral Researcher
Ghent University / Research Foundation–Flanders (FWO)
Advisor: Jan De Houwer

2005 Research Assistant
NSERC / University of Saskatchewan
Advisor: Valerie A. Thompson

2005 Research Assistant
University of Saskatchewan
Advisor: Jim Cheesman

2004 – 2005 Research Programmer
University of Saskatchewan
Clients: Jim Cheesman, Lorin Elias, Peter Hall, Michael McGregor

Doctoral Supervision

2018-2021 Šaban, Iva

Stage Supervision

2019 Jondot, Anna

Teaching Experience

2019 Workshop Instructor
Universität Trier
Course: Introduction to the PEP model

2009 – 2018 Teaching Assistant
Ghent University
Courses: Introductory, Learning, Health, Clinical, Family

2005 – 2009 Teaching Assistant
University of Waterloo
Courses: Introductory, Statistics, Research Methods, Thesis

2008 Instructor
University of Waterloo
Course: Statistics (tutorial component)

2007 Guest Lecturer
University of Waterloo
Course: Psychology of Reading

2005 Tutor
University of Saskatchewan
Course: Advanced Cognitive Science

2004 – 2005 Teaching Assistant
University of Saskatchewan
Course: Introductory Psychology

Departmental Service

2011 – 2018 Academic bibliography manager

Editorial Contributions

2017 – Associate Editor, Experimental Psychology
2016 – Associate Editor, Frontiers in Psychology
2013 – 2015 Guest Associate Editor, Frontiers in Psychology

Ad Hoc Reviewer Contributions

● Acta Psychologica ● Advances in Cognitive Psychology ● Attention, Perception, & Psychophysics ● Brain and Behavior ● Brain Imagining and Behavior ● Cognition ● Cognition & Emotion ● Cognitive, Affective, & Behavioral Neuroscience ● European Journal of Cognitive Psychology ● Experimental Psychology ● Frontiers in Human Neuroscience ● Frontiers in Psychology ● Human Brain Mapping ● Human Movement Science ● Journal of Cognitive Psychology ● Journal of Experimental Psychology: Human Perception and Performance ● Journal of Experimental Psychology: Learning, Memory, and Cognition ● Learning and Motivation ● Memory ● Memory & Cognition ● Motivation and Emotion ● Neuropsychologia ● Perceptual and Motor Skills ● PLOS ONE ● Psicológica ● Psychiatry Research ● Psychological Research ● Psychology and Aging ● Psychonomic Bulletin & Review ● Quarterly Journal of Experimental Psychology ● Visual Cognition

Grants, Contracts, Scholarships, and Awards

Grants

2018 Investissements d'Avenir, Project ISITE-BFC, ANR15-IDEX-0003
Title: Cognitive psychology, learning, and modelling
Amount: €472,500/3 years

2013 FWO Postdoctoral Researcher Mandate – Renewal (PI)
Title: Reassessing conflict: Basic learning processes or conflict adaptation?
Amount: €175,000/3 years (incl. €15,000 operating funds)

2010 FWO Postdoctoral Researcher Mandate (PI)
Title: Colour-word contingency learning: Automatic or controlled?
Amount: €165,000/3 years (incl. €15,000 operating funds)

2009 FWO Visiting Postdoctoral Fellowship (PI)
Title: Automatic processes in psychopathology and health-related behaviour
Amount: €29,000/1 year (incl. €2000 operating funds)

Contracts

2016 Methusalem Postdoctoral Fellowship (Resigned Early for UBFC)
Title: Learning and implicit processes
Amount: €180,000/3 years

Scholarships

2006 NSERC Canada Graduate Scholarship–Doctoral
Amount: $105,000/3 years

2006 Ontario Graduate Scholarship–Doctoral (Offer Declined for NSERC)
Amount: $60,000/4 years

2005 President's Graduate Scholarship
Amount: $40,000/4 years

2005 NSERC Canada Graduate Scholarship–Master's
Amount: $17,500/1 year

2004 University of Saskatchewan Undergraduate Scholarship
Amount: $1,500/1 year

Awards

2005 NSERC Undergraduate Student Research Award
Amount: $5,600

2004 Hantelman Humanities Scholarship
Amount: $500

Approximate Total: €1,175,000 / $1,770,000

Publications

Peer-Reviewed Journals

in press Braem, S., Bugg, J. M., Schmidt, J. R., Crump, M. J. C., Weissman, D. H., Notebaert, W., & Egner, T. (in press). Measuring adaptive control in conflict tasks. Trends in Cognitive Sciences.

Liefooghe, B., Hughes, S., Schmidt, J. R., & De Houwer, J. (in press). Stroop-like effects for derived stimulus-stimulus relations. *Journal of Experimental Psychology: Learning, Memory, and Cognition*.

2019 Schmidt, J. R. (2019). Evidence against conflict monitoring and adaptation: An updated review. *Psychonomic Bulletin & Review, 26,* 753-771.

Schmidt, J. R., & Lemercier, C. (2019). Context-specific proportion congruent effects: Compound-cue contingency learning in disguise. *Quarterly Journal of Experimental Psychology, 72,* 1119–1130.

Schmidt, J. R., & De Houwer, J. (2019). Cue competition and incidental learning: No blocking or overshadowing in the colour-word contingency learning procedure without instructions to learn. *Collabra: Psychology, 5,* Article 15.

2018 Schmidt, J. R., Augustinova, M., & De Houwer, J. (2018). Category learning in the colour-word contingency learning paradigm. *Psychonomic Bulletin & Review, 25,*658–666.

Schmidt, J. R. (2018). Best not to bet on the horserace: A comment on Forrin and MacLeod (2017) and a relevant stimulus-response compatibility view of colour-word contingency learning asymmetries. *Memory & Cognition, 46,*326–335.

Schmidt, J. R., Hartsuiker, R. J., & De Houwer, J. (2018). Interference in Dutch-French bilinguals: Stimulus and response conflict in intra- and interlingual Stroop. *Experimental Psychology, 65,* 13–22.

2017 Schmidt, J. R. (2017). Time-out for conflict monitoring theory: Preventing rhythmic biases eliminates the list-level proportion congruent effect. *Canadian Journal of Experimental Psychology, 71,* 52–62.

Lemercier, C., Simoës-Perlant, A., Schmidt, J. R., & Boujon, C. (2017). Stroop interference and development: Influence of expectation on color-naming response times. *European Review of Applied Psychology, 67,* 43–50.

2016 Schmidt, J. R. (2016). Context-specific proportion congruency effects: An episodic learning account and computational model. *Frontiers in Psychology, 7,* Article 1806.

Schmidt, J. R., De Houwer, J., & Rothermund, K. (2016). The Parallel Episodic Processing (PEP) model 2.0: A single computational model of stimulus-response binding, contingency learning, power curves, and mixing costs. *Cognitive Psychology, 91*, 82–108.

Schmidt, J. R., & De Houwer, J. (2016). Time course of colour-word contingency learning: Practice curves, pre-exposure benefits, unlearning, and relearning. *Learning and Motivation, 56,* 15–30.

Schmidt, J. R. (2016). Proportion congruency and practice: A contingency learning account of asymmetric list shifting effects. *Journal of Experimental Psychology: Learning, Memory, and Cognition, 42,* 1496–1505.

Schmidt, J. R., & Weissman, D. H. (2016). Congruency sequence effects and previous response times: Conflict adaptation or temporal learning? *Psychological Research, 80*, 590–607.

Schmidt, J. R., & De Houwer, J. (2016). Contingency learning tracks with stimulus-response proportion: No evidence of misprediction costs. *Experimental Psychology, 63,* 79–88.

Schmidt, J. R. (2016). Temporal learning and rhythmic responding: No reduction in the proportion easy effect with variable response-stimulus intervals. *Frontiers in Psychology, 7,* Article 634.

Schmidt, J. R., & Liefooghe, B. (2016). Feature integration and task switching: Diminished switch costs after controlling for stimulus, response, and cue repetitions. *PLOS ONE, 11,* e0151188.

2015 Schmidt, J. R., & Weissman, D. H. (2015). Contingent attentional capture triggers the congruency sequence effect. *Acta Psychologica, 159,* 61–68.

Schmidt, J. R., Notebaert, W., & Van Den Bussche, E. (2015). Is conflict adaptation an illusion? *Frontiers in Psychology, 6,* Article 172.

2014 Schmidt, J. R. (2014). Contingency and congruency switch in the congruency sequence effect: A reply to Blais, Stefanidi, and Brewer (2014). *Frontiers in Psychology, 5,* Article 1405.

Schmidt, J. R., De Schryver, M., & Weissman, D. H. (2014). Removing the influence of feature repetitions on the congruency sequence effect: Why regressing out confounds from a nested design will often fall short. *Journal of Experimental Psychology: Human Perception and Performance, 40,* 2392–2402.

Schmidt, J. R., Lemercier, C., & De Houwer, J. (2014). Context-specific temporal learning with non-conflict stimuli: Proof-of-principle for a learning account of context-specific proportion congruent effects. *Frontiers in Psychology, 5,* Article 1241.

Schmidt, J. R., & Weissman, D. H. (2014). Congruency sequence effects without feature integration or contingency learning confounds. *PLOS ONE*, *9,* e0102337.

Schmidt, J. R. (2014). Contingencies and attentional capture: The importance of matching stimulus informativeness in the item-specific proportion congruent task. *Frontiers in Psychology, 5,*Article 540.

Schmidt, J. R. (2014). List-level transfer effects in temporal learning: Further complications for the list-level proportion congruent effect. *Journal of Cognitive Psychology, 26,* 373–385.

2013 Schmidt, J. R. (2013). Temporal learning and list-level proportion congruency: Conflict adaptation or learning when to respond? *PLOS ONE*, *8,* e0082320.

Schmidt, J. R. (2013).Questioning conflict adaptation: Proportion congruent and Gratton effects reconsidered. *Psychonomic Bulletin & Review, 20,* 615–630.

Schmidt, J. R., Cheesman, J., & Besner, D. (2013). You can't Stroop a lexical decision: Is semantic processing fundamentally facilitative? *Canadian Journal of Experimental Psychology, 67,* 130–139.

Schmidt, J. R. (2013). The Parallel Episodic Processing (PEP) model: Dissociating contingency and conflict adaptation in the item-specific proportion congruent paradigm. *Acta Psychologica, 142,* 119–126.

2012 Schmidt, J. R., & De Houwer, J. (2012). Learning, awareness, and instruction: Subjective contingency awareness does matter in the colour-word contingency learning paradigm. *Consciousness and Cognition, 21*, 1754–1768.

Schmidt, J. R., & De Houwer, J. (2012). Contingency learning with evaluative stimuli: Testing the generality of contingency learning in a performance paradigm. *Experimental Psychology, 59*, 175–182.

Schmidt, J. R., & De Houwer, J. (2012). Adding the goal to learn strengthens learning in an unintentional learning task. *Psychonomic Bulletin & Review*, *19*, 723–728.

Schmidt, J. R., & De Houwer, J. (2012). Does temporal contiguity moderate contingency learning in a speeded performance task? *Quarterly Journal of Experimental Psychology*, *65*, 408–425.

2011 Schmidt, J. R., & De Houwer, J. (2011). Now you see it, now you don't: Controlling for contingencies and stimulus repetitions eliminates the Gratton effect. *Acta Psychologica*, *138*, 176–186.

2010 Schmidt, J. R., De Houwer, J., & Besner, D. (2010). Contingency learning in the blink of an eye: A resource dependent process. *Consciousness and Cognition*, *19*, 235–250.

2008 Schmidt, J. R., & Besner, D. (2008). The Stroop effect: Why proportion congruent has nothing to do with congruency and everything to do with contingency. *Journal of Experimental Psychology: Learning, Memory, and Cognition*, *34*, 514–523.

Schmidt, J. R., & Thompson, V. A. (2008). "At least one" problem with "some" formal reasoning paradigms. *Memory & Cognition*, *36*, 217–229.

2007 Schmidt, J. R., Crump, M. J. C., Cheesman, J., & Besner, D. (2007). Contingency learning without awareness: Evidence for implicit control. *Consciousness and Cognition*, *16*, 421–435.

2006 Risko, E. F., Schmidt, J. R., & Besner, D. (2006). Filling a gap in the semantic gradient: Color associates and response set effects in the Stroop task. *Psychonomic Bulletin & Review*, *13*, 310–315.

2005 Schmidt, J. R., & Cheesman, J. (2005). Dissociating stimulus-stimulus and response-response effects in the Stroop task. *Canadian Journal of Experimental Psychology, 59*, 132–138.

Other

2019 Schmidt, J. R., & De Houwer, J. (2019). Correction to Schmidt and De Houwer (2012). *Experimental Psychology, 66*, 255–256.

2015 Schmidt, J. R., Notebaert, W., & Van den Bussche, E. (Eds.). (2015). Is conflict adaptation an illusion? [Special issue]. *Frontiers in Psychology*.

2012 Schmidt, J. R. (2012). Human contingency learning. In N. M. Seal (Ed.), *Encyclopedia of the sciences of learning* (pp. 1455–1456). New York: Springer.

Invited Talks and Major Conference Presentations

Symposium organisation

2017 Schmidt, J. R., Giesen, C., & Rothermund, K. (2017). Contingency learning, binding, and their interrelations. Symposium at the *European Society for Cognitive Psychology* in Potsdam, Germany.

Invited talks

2019 Schmidt, J. R. (2019). The Parallel Episodic Processing (PEP) model. Invited talk presented at *Université Franche-Comté* in Besançon, France.

Schmidt, J. R. (2019). Applications of an exemplar-based neural network. Invited talk presented at *Universität Trier* in Trier, Germany.

Schmidt, J. R. (2019). Integrative exemplar-based neural network. Invited talk presented at *Centre National de la Recherche Scientifique (CNRS)* in Paris, France.

2018 Schmidt, J. R. (2018). Learning, binding, and an exemplar-based neural network. Invited talk presented at *Universität zu Köln* in Cologne, Germany.

Schmidt, J. R. (2018). A single-mechanism account of contingency learning and binding effects. Invited talk presented at *Friedrich-Schiller-Universität Jena* in Jena, Germany.

Schmidt, J. R. (2018). Modelling cognition: Practice, learning, binding, instruction following, and goals. Invited talk presented at the *Methusalem Research Day* in Ghent, Belgium.

2017 Schmidt, J. R., Liefooghe, B., & De Houwer, J. (2018). An episodic memory account of cued task switching. Invited symposium talk presented at the *Conference of Experimental Psychologists* (*TeaP)* in Marburg, Germany (with panel discussion).

Schmidt, J. R. (2017). Bridging learning and binding with a neural network of episodic memory. Invited talk presented at *Universität Trier* in Trier, Germany.

Schmidt, J. R. (2017). Attentional control and learning. Invited talk presented at *Centre National de la Recherche Scientifique (CNRS)* in Paris, France.

Schmidt, J. R. (2017). The Parallel Episodic Processing (PEP) model 2.0: Practice, learning, binding, and more. Invited talk presented at the joint meeting of the *Learning and Implicit Processes* and *Centre for the Psychology of Learning and Experimental Psychopathology (KU Leuven)* labs in Leuven, Belgium.

2016 Schmidt, J. R. (2016). The Parallel Episodic Processing (PEP) model. Invited talk presented at *Bristol University* in Bristol, UK.

Schmidt, J. R. (2016). Attentional control and learning. Invited talk presented at the *University of Groningen* in Groningen, Netherlands.

Schmidt, J. R. (2016). Attentional control and learning. Invited talk presented at the *University of Lincoln* in Lincoln, UK.

Schmidt, J. R. (2016). The Parallel Episodic Processing (PEP) model. Invited talk presented at the joint meeting of the *Learning and Implicit Processes* and *Friedrich-Schiller-Universität Jena* labs in Meinhard-Jestädt, Germany.

Schmidt, J. R. (2016). Attentional control and learning. Invited talk presented at *Centre National de la Recherche Scientifique (CNRS)* in Paris, France.

2015 Schmidt, J. R. (2015). Reconsidering the evidence for conflict adaptation: Higher-order attentional control or lower-level learning biases? Invited talk presented at *Centre National de la Recherche Scientifique (CNRS)* in Paris, France.

2014 Schmidt, J. R. (2014). Contingency learning, temporal learning, and attentional control. Invited talk presented at the *Université de Bourgogne* in Dijon, France.

2011 Schmidt, J. R. (2011). The conflict adaptation illusion: Reassessing the proportion congruent and Gratton effects. Invited symposium paper presented at *Myths and Facts about Cognitive Control* in Ghent, Belgium.

2010 Schmidt, J. R., & De Houwer, J. (2010). Contingency learning overtime: SOA analyses. Invited symposium paper presented at the *Belgian Association for Psychological Science* in Brussels, Belgium.

Schmidt, J. R. (2010). Colour-word contingency learning: A brief overview. Invited talk presented at the *University of Groningen* in Groningen, Netherlands.

2008 Schmidt, J. R., De Houwer, J., & Besner, D. (2008). Contingency learning in seven plus or minus two items: An implicit, working memory dependent process. Invited talk presented at *McMaster University* in Hamilton, Canada.

2005 Schmidt, J. R., & Cheesman, J. (2005). Locating the Stroop effect: Facilitation and inhibition in semantic and response processes. Invited talk presented at the *University of Waterloo* in Waterloo, Canada.

Schmidt, J. R., & Cheesman, J. (2005). Locating the Stroop effect: Facilitation and inhibition in semantic and response processes. Invited talk presented at the *University of Western Ontario* in London, Canada.

Schmidt, J. R., & Cheesman, J. (2005). Locating the Stroop effect: Facilitation and inhibition in semantic and response processes. Invited talk presented at *McMaster University* in Hamilton, Canada.

Schmidt, J. R., & Cheesman, J. (2005). Locating the Stroop effect: Facilitation and inhibition in semantic and response processes. Invited talk presented at the *University of Victoria* in Victoria, Canada.

Other talks

2019 Schmidt, J. R., & De Houwer, J. (2019). Cue competition and incidental learning: No blocking or overshadowing in the colour-word contingency learning procedure without instructions to learn. Forthcoming poster presented at the *European Society for Cognitive Psychology* in Tenerife, Spain.

2017 Schmidt, J. R., De Houwer, J., & Rothermund, K. (2017). Are contingency learning and binding one and the same? Paper presented at the *European Society for Cognitive Psychology* in Potsdam, Germany (symposium organizer).

2016 Schmidt, J. R., & De Houwer, J. (2016). Misprediction costs nothing: Contingency learning and proportional retrieval. Paper presented at the *Belgian Association for Psychological Science* in Antwerp, Belgium.

2015 Schmidt, J. R., & Weissman, D. H. (2015). Contingent attentional capture triggers the congruency sequence effect. Paper presented at the *Belgian Association for Psychological Science* in Brussels, Belgium.

2014 Schmidt, J. R., De Schryver, M., & Weissman, D. H. (2014). Feature repetitions, sequential congruency, and regression: A response to Notebaert and Verguts (2007). Paper presented at the *Belgian Association for Psychological Science* in Leuven, Belgium.

2013 Schmidt, J. R. (2013). Item-specific proportion congruency: Dissociating contingency and conflict adaptation. Paper presented at the *European Society for Cognitive Psychology* in Budapest, Hungary (session chair).

Schmidt, J. R., & De Houwer, J. (2013). Colour-word contingency learning: Instructions and awareness. Paper presented at the *Belgian Association for Psychological Science* in Louvain-la-Neuve, Belgium.

2012 Schmidt, J. R. (2012). The temporal retrieval hypothesis: Learning when (rather than what) to respond explains list-level proportion congruent effects. Paper presented at the joint meeting of the *Belgian Association for Psychological Science* and the *Sociedad Española de Psicología Experimental* in Liège, Belgium.

2011 Schmidt, J. R., & De Houwer, J. (2011).Now you see it, now you don't: Controlling for contingencies and stimulus repetitions eliminates the Gratton effect. Paper presented at the *European Society for Cognitive Psychology* in San Sebastian, Spain.

Schmidt, J. R. (2011). Conflict and contingency: The Parallel Episodic Processing (PEP) model. Paper presented at the *Psychology Conference of the Athens Institute for Education and Research* in Athens, Greece.

Schmidt, J. R., & De Houwer, J. (2011). Implicit contingency learning with evaluative stimuli. Paper presented at the *Belgian Association for Psychological Science* in Ghent, Belgium.

2009 Schmidt, J. R., De Houwer, J., & Besner, D. (2009). Contingency learning in the blink of an eye: A resource dependent process. Paper presented at the joint meeting of the *Experimental Psychology Society* and the *Canadian Society for Brain, Behaviour, and Cognitive Science* in York, England.

2008 Schmidt, J. R., Cheesman, J., & Besner, D. (2008). You can't Stroop a lexical decision: Combating the communication breakdown in cognitive psychology with a general model of semantics. Paper presented at the *Canadian Society for Brain, Behaviour, and Cognitive Science* in London, Canada.

2007 Schmidt, J. R., & Besner, D. (2007). The Stroop effect: Why the "proportion congruent" effect has nothing to do with congruency and everything to do with contingency. Poster presented at the *Canadian Society for Brain, Behaviour, and Cognitive Science* in Victoria, Canada.

2006 Schmidt, J. R., Crump, M. J. C., Cheesman, J., & Besner, D. (2006). Contingency learning without awareness: Evidence for implicit control. Poster presented at the *Canadian Society for Brain, Behaviour, and Cognitive Science* in Saskatoon, Canada.

2005 Schmidt, J. R., & Cheesman, J. (2005). Semantic facilitation, semantic inhibition, and response competition in the Stroop task. Poster presented at the *Banff Annual Seminar in Cognitive Science* in Banff, Canada.

Schmidt, J. R., & Cheesman, J. (2005). Semantic facilitation, semantic inhibition, and response competition in the Stroop task. Paper presented at the *Canadian Society for Brain, Behaviour, and Cognitive Science* in Montreal, Canada.

Qualification

2018	GALAXIE, maître de conférences (France)

Languages

English	Native speaker
Dutch	CEFR: B2 (upper intermediate)
French	CEFR: B1 (intermediate)

Professional Skills

Coding	Java, JavaScript, HTML, CSS, R, JQuery, PHP, Visual Basic, Perl, Python, C++, Apache
Programs	SPSS, E-Prime, Notepad++, NetBeans, Eclipse, GIMP, Audacity, RStudio, Bitbucket (GIT), Inquisit, JASP, MorePower, Statistica, Blender
Modelling	Neural networks, machine learning, statistical modelling, evolutionary algorithms

Web

Department	http://leadserv.u-bourgogne.fr/~jschmidt/
Google Scholar	https://scholar.google.ca/citations?user=LRKuBQYAAAAJ
ResearchGate	https://www.researchgate.net/profile/James_Schmidt3
ORCID	http://orcid.org/0000-0002-0412-396X

Chapter 1 – The Stroop Effect

Published Work

Brief background

Starting with research conducted as an undergraduate (licence), I have published a number of papers on the sources of conflict in Stroop and Stroop-like procedures. In the standard Stroop experiment (Stroop, 1935; for a review, see MacLeod, 1991), participants name the print colour of colour words (e.g., say “red” to the word “green” printed in red). The typical finding is that responses are slower and less accurate when the word and colour mismatch (e.g., “red” in green), termed an *incongruent* trial, relative to when the word and colour match (e.g., “red” in red), termed a *congruent* trial. Stroop, and related paradigms like the Eriksen flanker task (Eriksen & Eriksen, 1974) and Simon task (Simon & Rudell, 1967), is interesting for a number of reasons. On the one hand, we are able to roughly follow the instruction to avoid reading the word and focus attention on the colour with a reasonably small error rate. On the other hand, the interference of the distracting word on colour identification indicates that, despite our best efforts, it is difficult to fully “filter out” the distracting word and attend exclusively to the colour. The Stroop effect therefore serves as a useful tool for studying cognition, how task-irrelevant information influences our behaviour, our ability (and limits on our ability) to control attention (e.g., see Chapter 3), and so on.

Semantic Stroop

One key question in the Stroop literature is the source of the interference, which has been discussed extensively in the literature. Particular focus has been on whether the conflict between the word and colour occurs when selecting a potential response, termed *response conflict*, or whether conflict occurs between the meanings (i.e., semantic representations) of the word and colour, termed *stimulus conflict*. In Schmidt and Cheesman (2005), we used a procedure in which participants respond with key presses (rather than vocal responses), inspired by De Houwer (2003). Critically, two colours were mapped to each key (e.g., red or blue with the left key, and green or yellow with the right key). This design is illustrated in Figure 1.1. Using this *2-to-1 mapping procedure*, it is possible to generate three trial types, which allow separate measures of stimulus and response conflict. On *identity* trials, the word is both congruent in meaning with the target colour and also corresponds to the same response (e.g., “blue” in blue). On *same response* trials, the word and colour are incongruent in meaning, but are mapped to the same response (e.g., “green” in blue). Thus, comparison of these two trial types gives a measure of stimulus conflict, as both trial types are equally response congruent but differ in whether the word and print colour correspond to the same colour concept. On *different response* trials, the word and colour differ both in meaning and in the response that they are mapped to (e.g., “red” in blue). Thus, same and different response trials differ only in response compatibility (i.e., both are different in meaning) and a difference between the two therefore is a measure of response conflict.

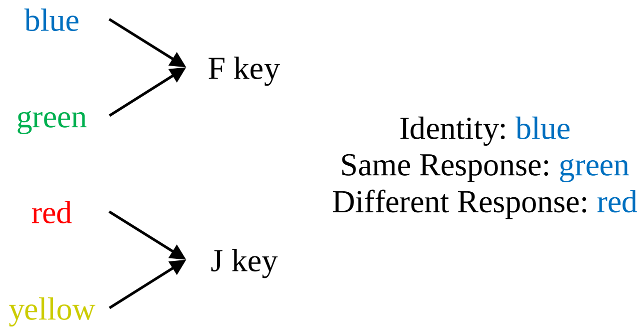

Figure 1.1. 2-to-1 response mapping and the resulting three conditions.

We observed both stimulus and response conflict with colour words, as illustrated in Figure 1.2, which replicated previous findings from De Houwer (2003; see also, A. T. Chen, Bailey, Tiernan, & West, 2011, 2004; Hasshim & Parris, 2015; Jongen & Jonkman, 2008; van Veen & Carter, 2005). We were more interested, however, in Stroop effects for colour associates (e.g., “sky,” which is related in meaning to blue). Colour associates can also be congruent (e.g., “sky” in blue) or incongruent (e.g., “sky” in red) with the print colour, and this also produces a Stroop-like interference effect (W. R. Glaser & Glaser, 1989; Klein, 1964; Mackinnon, Geiselman, & Woodward, 1985; Majeres, 1974; Posner & Snyder, 1975; Sharma & McKenna, 1998; Stirling, 1979). That is, incongruent colour associates are responded to more slowly and with more errors than congruent colour associates.

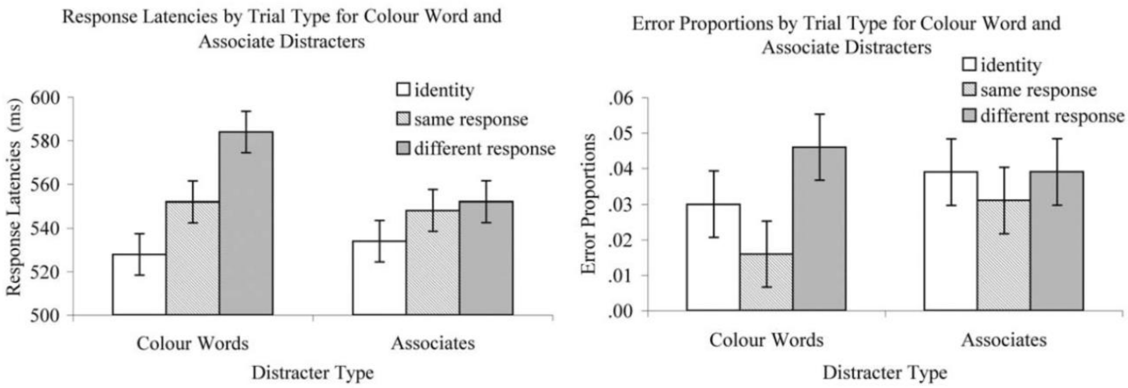

Figure 1.2. Response time (left) and error rate (right) results of Schmidt and Cheesman (2005).

The compatibility difference evidenced using colour associate distracters has often been interpreted as being the result of early, semantic processes rather than late, response competition processes (W. R. Glaser & Glaser, 1989; Mackinnon et al., 1985; Stirling, 1979). The reason for interpreting the associate effect in this way is based on the following logic. The two stimulus dimensions are associatively related and the concurrent activation of the word and the target colour ought to produce stimulus conflict. On the other hand, there does not appear to be a direct relationship between the responses for the associate words and the colour responses. For instance, “sky” is not one of the potential responses. The response sets for the target and the distracter are distinct, and therefore no response conflict should be observed. Thus, associates are generally used as a means to present the argument that the Stroop effect results, in whole or in part, from early, semantic processes.

However, not all researchers accept the early, semantic account of the colour associate effect (Klein, 1964; Posner & Snyder, 1975). Klein suggested that associates may have their effect at output by indirectly producing the colour response linked to the colour associate. Thus, when “sky” is presented in the colour green, both blue and green are generated as potential responses and response competition results. According to this account then, associates should produce response conflict rather than stimulus conflict. Finally, Sharma and McKenna (1998; see also Majeres, 1974) argued that the effects of associates are located in the lexicon (rather than semantic memory) and emerge as a result of verbal responding. They observed a compatibility effect for colour associates using verbal responding to ink colour but the effect was eliminated when manual key press responses were used (but see Brown & Besner, 2001, for a re-analysis of these data). Sharma and McKenna concluded that the influence of colour associates in the Stroop task is restricted to lexical processing and will not be evident using manual responses because the verbal system does not control motor responses. As can be observed in Figure 1.2, however, colour associates did produce conflict in key press responses. Importantly, the colour associates produce only stimulus conflict, and not response conflict, supporting the notion that associates only produce interference in semantics.

Interlingual Stroop

In research on bilingualism, it is further observed that Stroop interference is found not only for the first language (L1), but also for words from a second language (L2; Altarriba & Mathis, 1997; Atalay & Misirlisoy, 2012; H. C. Chen & Ho, 1986; Dalrymple-Alford, 1968; Dyer, 1971; La Heij et al., 1990; Mägiste, 1984, 1985; Preston & Lambert, 1969; Smith & Kirsner, 1982; Tzelgov, Henik, & Leiser, 1990). For instance, a native English speaker that also speaks French will experience conflict from incongruent French colour words (e.g., "rouge" in blue). One thing that was less clear, however, is whether this L2 Stroop effect is due to stimulus conflict, response conflict, or both. Using the same 2-to-1 mapping procedure, we also observed that both stimulus and response conflict are observed in a foreign language (Schmidt, Hartsuiker, & De Houwer, 2018). In particular, Dutch speakers with relatively weak French skills produced both a stimulus and response conflict effect, both with native Dutch colour words and with second language French colour words, as illustrated in Figure 1.3. Thus, even though French-language competency was not so high in the Flemish population, both stimulus and response interference were observed.

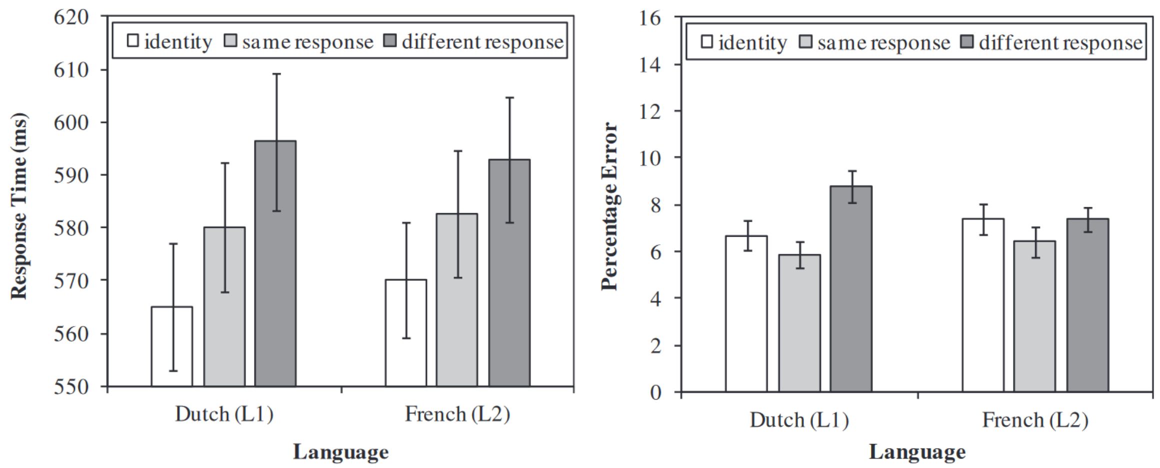

Figure 1.3. Response time (left) and error (right) results from Schmidt, Hartsuiker, and De Houwer (2018).

This result contrasts with at least one prominent model in the language learning domain, called the *revised hierarchical model*. According to this model, early on in language learning foreign-language words are only learned as lexical translations of the native language equivalent words (Kroll & Stewart, 1994), as illustrated in Figure 1.4. According to this view, foreign language words are not directly connected to semantic knowledge. That is, we only learn a foreign language word (e.g., "jaune") as a lexical translation of the same word in our first language (e.g., "yellow"). We do not learn a "direct" connection between the foreign language word and meaning (e.g., that "jaune" is related to canary, banana, etc.). If this were true, then we would not have expected a stimulus conflict effect.

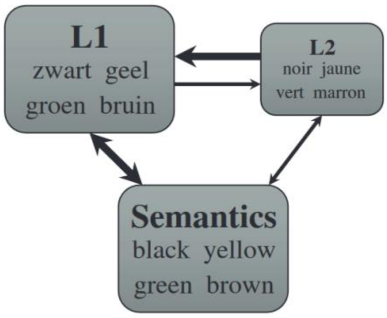

Figure 1.4. Illustration of the Kroll and Stewart (1994) model. Note that second language words (L2) are only connected strongly to first language words (L1) in the lexicon and not to semantics.

Response set and stimulus conflict

In Risko, Schmidt, and Besner (2006), we assessed colour associate Stroop effects again. In particular, we assessed the magnitude of congruency effects for colour associates that were associated either to colours in the response set (e.g., "sky," if blue was one of the potential responses) or to colours out of the response set (e.g., "fire," if red was *not* one of the potential responses). It was already known that out-of-set colour words produce less conflict than in-set colour words (e.g., Klein, 1964; Sharma & McKenna, 1998), but the same question was not assessed for colour associates. Interestingly, as observed in Figure 1.5, we observed larger congruency effects for in-set associates than for out-of-set associates in both key press and verbal task variants. These results suggest, at minimum, that colour associate congruency effects are related to responses in at least one way. Either colour associates do produce at least some response conflict (unfortunately, the 2-to-1 mapping procedure is impossible with out-of-set associates) or in-set colours are primed by virtue of being potential responses. That is, out-of-set associates might produce less interference because they facilitate the semantic representation of a colour that is not primed as a potential response.

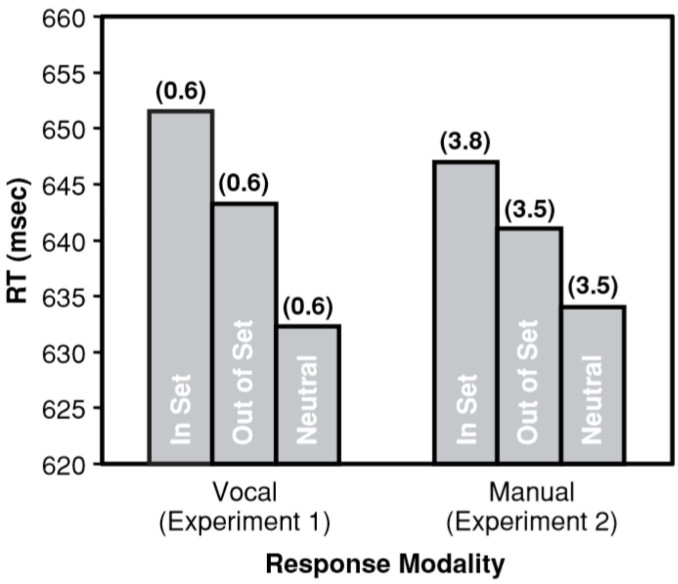

Figure 1.5. Colour associate Stroop effects from Risko and colleagues (2006).

Spreading activation versus lateral inhibition

In Schmidt, Cheesman, and Besner (2013), we noted a strange inconsistency in the way in which semantic connectivity is described in Stroop/conflict literatures and in the word reading literature. In the Stroop literature, conflict in meaning between two colours is described in terms of *lateral inhibition* (e.g., see Luo, 1999, for a semantic competition account of Stroop effects). That is, a given concept (e.g., “red”) is proposed to “spread inhibition” to closely-related concepts (e.g., “blue”). This notion is in stark contrast to the way in which semantics are described in other literatures. For instance, consider the lexical decision task (Meyer & Schvaneveldt, 1971). On each trial in a lexical decision task, the participant is presented with a letter string that they have to judge as either being a word (e.g., “nurse”) or a nonword (e.g., “silmu”). One key finding in the lexical decision literature is semantic priming: if a prime word is presented in advance of the target word that is semantically related to the target (e.g., “doctor” as a prime to “nurse”), then performance is much better than when the prime word is unrelated in meaning to the target (e.g., “chair” as a prime to “nurse”). This priming effect has been interpreted in terms of *spreading activation* in semantics. That is, a given word (e.g., “nurse”) sends *positive* activation to closely-related concepts (e.g., “doctor”). This is obviously the exact opposite of lateral inhibition: following Stroop interference type logic, “nurse” should *interfere* with identification of the related concept “doctor.” Indeed, “word-word” variants of the Stroop task exist in which a distracting word is presented as a prime to a target word (e.g., M. O. Glaser & Glaser, 1982; W. R. Glaser & Glaser, 1989), very similar in procedure to a lexical decision task. Relatedly, even “category members” have been used in lexical decision experiments (Chiarello & Richards, 1992), including occasional colour prime-probe pairs (e.g., Borowsky & Besner, 1991, 1993), and facilitation is again observed.

In a series of experiments, we explored what might explain these inconsistencies in theories (and results). First, we ruled out the notion that there is something special about colours (or categories more generally): incongruent colour associate word primes produced significantly *faster* responses to colour words in lexical decision than neutral word primes (i.e., the opposite of a Stroop effect). We found that interference is only observed when (a) the participant has to select *between* different colours (identification) and (b) there is a small repeating response set (i.e., only a few potential response options to choose from). In particular, we still observed that incongruent colour associates facilitated *identification* responses in a Stroop-like task when there was a very large, non-repeating set of associates and colour names. We also found facilitation of incongruent colour words on colour word targets with a very small set of stimuli in lexical decision. It was only in a fourth experiment with both (a) a small set of colour word targets and distracters, and (b) an identification response that we observed interference.

We interpreted these results as indicating that semantic concepts are linked positively (i.e., spreading activation) and that interference results from conflict in deciding on a potential response. For instance, the word “blue” and colour red suggest two different responses in a typical Stroop task and semantic facilitation between the concepts “blue” and “red,” perhaps unintuitively, make it even harder to determine which response is appropriate. This is because there are two highly primed response alternatives and it needs to be resolved which of the two is appropriate. In contrast, in lexical decision both “blue” and “red” are words and therefore hint at a “word” response. Facilitation between these related colours can only speed the “word” decision. These notions are illustrated visually in Figure 1.6.

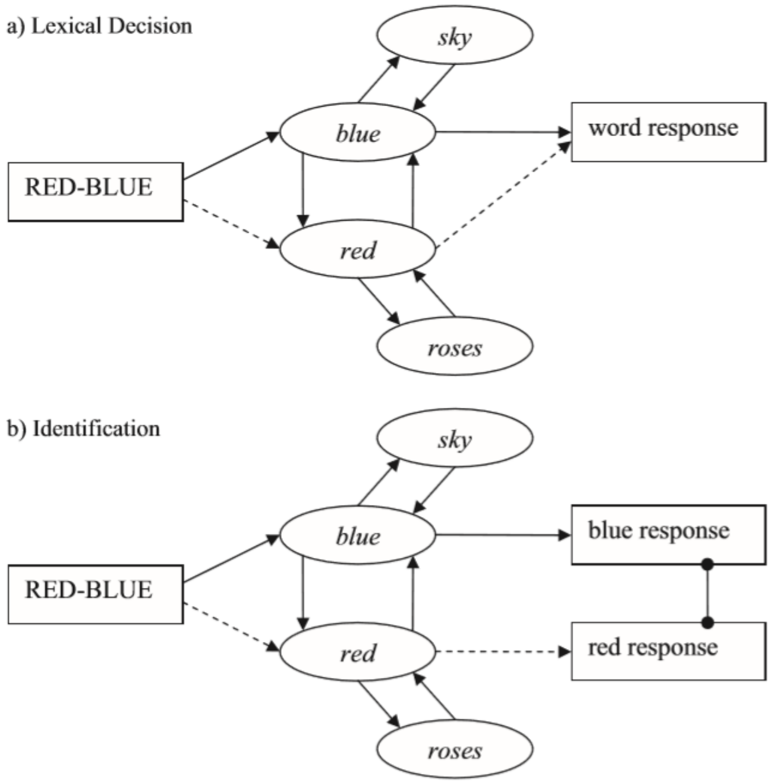

Figure 1.6. Stimulus processing with facilitative semantic connections only in (a) lexical decision and (b) identification. Response codes can compete only if identification of the specific target is necessary.

Stroop interference in development

In Lemercier, Simoës-Perlant, Schmidt, and Boujon (2017), we studied the magnitude of Stroop interference effects across a range of ages in children. Interestingly, we observed little variation in the magnitude of congruency effects from ages 6 to 18+. Prior findings have suggested that participants' ability to resist interference increases with age (e.g., Bunge, Dudukovic, Thomason, Vaidya, & Gabrieli, 2002; Carver, Livesey, & Charles, 2001; Enns & Cameron, 1987; Pennequin, Nanty, & Khomsi, 2004; Rubia et al., 2000; Tipper, Bourque, Anderson, & Brehaut, 1989). As word reading becomes increasingly automatic with age, incongruent words gradually begin to interfere with colour naming (Gerstadt, Hong, & Diamond, 1994; MacLeod, 1991; Shiffrin & Schneider, 1977; Schadler & Thissen, 1981). For instance, Schiller (1966) showed that the interference effect is minimal for children in first grade, maximal in second and third grade, and then progressively declines starting from fifth grade. These results were interpreted in the following way. When children are too young to read, word meaning does not interfere with colour naming. When their reading skills increase, word meaning interferes with colour naming. Further, it has also been argued that the inhibition mechanism is not yet mature at eight years old. As such, the magnitude of the interference effect is greater for young participants. With further development, suppression of the distracting word becomes more effective. This produces an inverted-U shaped function of interference across time, as illustrated in Figure 1.7.

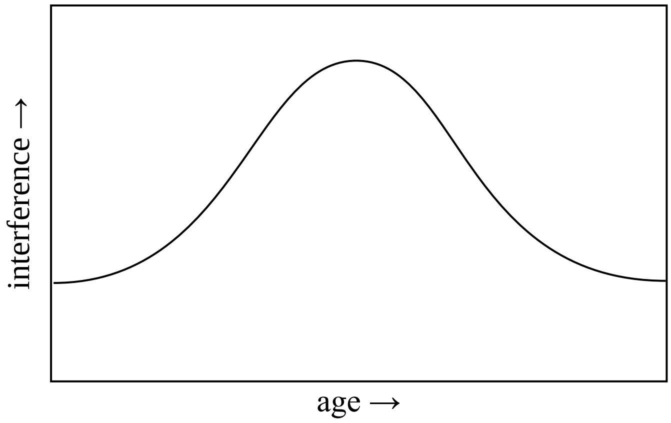

Figure 1.7. An inverted-U shaped development of interference over age. As word reading ability increases in early childhood, incongruent words interfere more with colour naming. Later on in development, the ability to control attention and suppress the word increases, thereby diminishing interference in adolescence.

This hypothesis, related to a deficit in inhibitory control, has also been advanced to explain the increase in the magnitude of the interference effect in the elderly. It has been suggested that older people have more difficulty suppressing the to-be-ignored word dimension while processing the relevant colour dimension (Carter, Mintun, & Cohen, 1995; Comalli, Wapner, & Werner, 1962). However, a meta-analysis has demonstrated that the magnitude of the Stroop effect is in fact similar from young adulthood to old age when a general slowdown in processing is taken into account (Verhaeghen & Meersman, 1998). That is, elderly show a larger Stroop effect simply because they are overall slower to respond and the response time effect "scales up." Bub, Masson, and Lalonde (2006) have also proposed a new explanation for the developmental variations in the Stroop effect starting from childhood. By studying the development of the Stroop effect from ages 5 to 12, they demonstrated that younger participants do not have more difficulty suppressing the irrelevant information, but have difficulty maintaining the coloured task set. The authors concluded that children maintain the colour-naming task set inconsistently across different trials.

A limitation with past research in this domain, however, was the way that incongruent and neutral items were tested. In particular, the neutral items were tested in separate blocks than the incongruent stimuli. There are several reasons why this is problematic. For instance, typical experiments use far less neutral stimuli (e.g., XXXX in each of five colours) than incongruent stimuli (e.g., 16 incongruent word-colour pairings) and this allows for item-specific learning (i.e., automatization of each stimulus compound) in the former but not latter case (e.g., Lemercier, 2009). We therefore tested incongruent and neutral items in a mixed block. We observed that the congruency effect was actually quite stable across all participants when tested in this way. A further experiment with neutral items only and variable stimulus set sizes demonstrated clearly that it was the exploitation of smaller stimulus sets in older participants that gave the illusion of reduced Stroop effects during later development: neutral response times became faster with smaller stimulus sets. These results concur with other work in the musical Stroop domain (see Chapter 5 for more information) that have similarly raised questions about the "inverted U-shaped" interference notion (Grégoire, Perruchet, & Poulin-Charronnat, 2015), where it has been observed that interference increases with music mastery and does not diminish again.

Derived Stroop effects

In Liefooghe, Hughes, Schmidt, and De Houwer (in press), we aimed to train Stroop-like effects with derived stimulus-stimulus relations. In particular, participants were first given conditional discrimination training via matching-to-sample. In a matching-to-sample task, participants are given a *sample* (target) *stimulus* and two or more *comparison* (response) *stimuli* that they have to choose between. By rewarding selection of a given comparison stimulus on presentation of a given sample stimulus, learning of a relation between the sample and comparison is promoted. In our experiments, participants trained one set of nonwords with colours. For instance, participants are presented with the nonword “plesk” or “klamf” as a sample stimulus and need to select between the colour words “red” and “green” as comparison stimuli on the basis of the identity of the sample stimulus. For instance, participants may be reinforced via error feedback to select the comparison stimulus “red” when the sample stimulus is “plesk” and the comparison stimulus “green” when the sample stimulus is “klamf.” In a following matching-to-sample test without error feedback, the expectation is that participants will not only select the previously-reinforced colour word for each nonword (e.g., “red” to “plesk”), but also the reverse when the colour word is the sample and the nonwords are the comparison stimuli (e.g., “plesk” most often to “red”).

The same type of training was further extended to new sets of overlapping contingencies. For instance, “smelk” might be rewarded with selection of “plesk” and “gilpt” with selection of “klamf.” Conceptually, this creates two *equivalence classes*, one between “red,” “plesk,” and “smelk,” and another between “green,” “klamf,” and “gilpt.” That is, although “red” and “smelk” are never presented together, they share the same sample stimulus (“plesk”), and similarly for “green,” “gilpt,” and “klamf.” During a subsequent test phase in which no reinforcement is provided, responses in line with the contingencies that were previously reinforced (e.g., “klamf” → “green”) as well as reversed responding will be observed (e.g., “green” → “klamf”). In addition, when combining comparison stimuli of both sets of contingencies by using one set of comparison stimuli as sample stimuli (e.g., sample stimuli “red” and “green”) with comparison stimuli “gilpt” and “smelk,” the comparison stimulus “smelk” will be more likely selected when presenting the sample stimulus “red” and the comparison stimulus “gilpt” is more likely to be selected when presenting the sample stimulus “green.” The direct reinforcement of partially overlapping contingencies in the training phase thus results in the formation of several new relations which were never directly reinforced, namely: “red” → “smelk,” “smelk” → “red,” “green” → “gilpt,” and “gilpt” → “green.” This training procedure is illustrated in Figure 1.8.

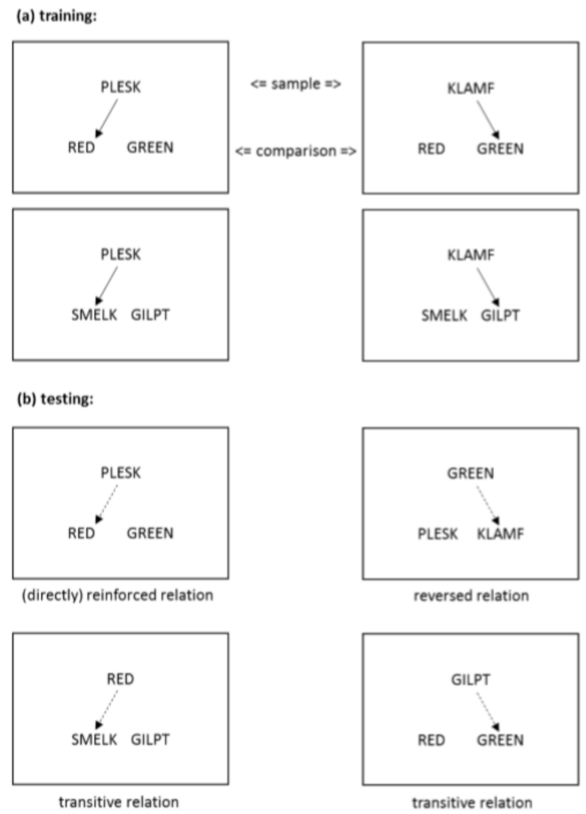

Figure 1.8. Illustration of a conditional-discrimination training and test in Liefooghe and colleagues (in press). Solid arrows point towards the reinforced contingency during training. Dashed arrows point towards the expected response pattern during testing, without reinforcement. Besides responding in accordance to the directly reinforced contingencies, responses will also be based on derived relations, namely: reversed and transitive relations.

Our main question was then: can derived stimulus-stimulus relations like these induce automatic effects? As already described, with our conditional discrimination training two non-words were directly reinforced with colours (i.e., reinforced associate), and two were only associated via derivation (i.e., derived associate). Following training, participants completed a modified Stroop task that included the colour words and their "associates" as distracters. Most critically, we were interested in whether derived associates produce a Stroop congruency effect. For instance, if "smelk" is the derived associate for "red," then a congruent stimulus like "smelk" in red should be responded to faster and more accurately than an incongruent stimulus like "smelk" in green. In addition to expected learning of the relations (including derived relations) during conditional discrimination, in our Experiment 2 we also observed congruency effects for colour words, reinforced associates, and, more critically, derived associates, as illustrated in Figure 1.9.

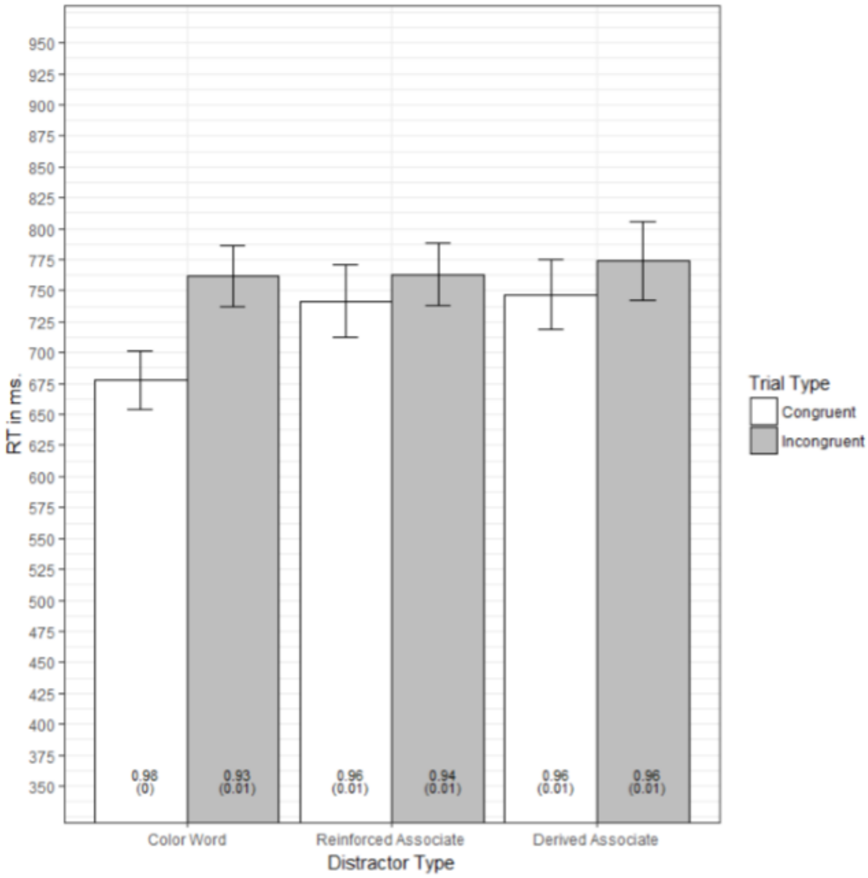

Figure 1.9. RTs of Experiment 2 of Liefooghe and colleagues (in press) as a function of trial type and distracter type. Error bars denote the standard errors. Error rates (with standard errors) presented as text.

In further studies, we used the 2-to-1 mapping procedure described earlier in order to test whether the effect of reinforced and derived associates was due to stimulus conflict, response conflict or both. The results for our Experiment 4 are presented in Figure 1.10. As can be observed, only response conflict was observed for the directly-reinforced and derived associates. That is, there was a difference between same and different response trials, which are both (associatively) incongruent in meaning, but differ in the colour response associated with the word and colour. However, there is no difference (with moderate Bayesian evidence for the null) between identity and same response trials, which differ only in the (associatively-trained) congruency in meaning.

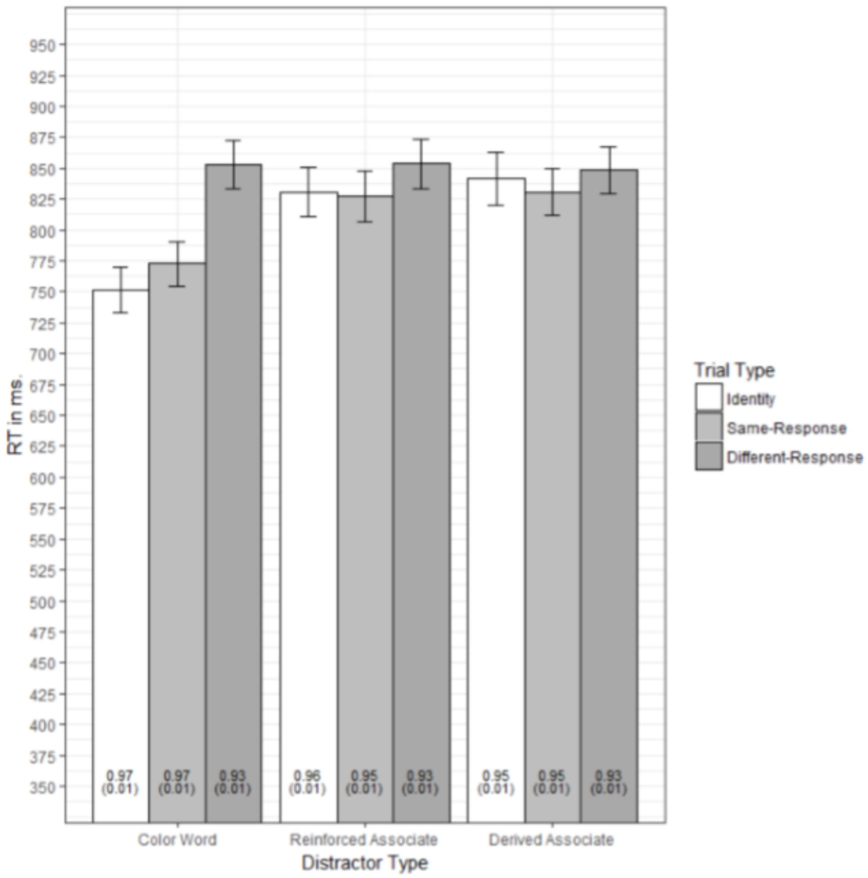

Figure 1.10. RTs of Experiment 4 of Liefooghe and colleagues (in press) as a function of trial type and distracter type. Error bars denote the standard errors. Error rates (with standard errors) presented as text.

Works in Progress and Future Directions

Language learning

In currently-ongoing research with my Ph.D. student, Iva Šaban (Šaban & Schmidt, 2019), we are further extending the work that I started in Schmidt, Hartsuiker, and De Houwer (2018). As mentioned above, in our past work we studied stimulus and response conflict in Dutch-speaking students both in their native Dutch and in French. We found both response conflict and, more interestingly, stimulus conflict even for the L2 French colour words. In our language competence measures, however, the French level of the Dutch-speaking sample was moderate. In line with the revised hierarchical model (Kroll & Stewart, 1994), then, it might be proposed that participants were fluent *enough* in French to produce stimulus conflict. This model only assumes that direct connectivity of L2 words to the semantic store is weak in early language learning. Our suspicion, however, is that the notion that L2 words are not strongly linked to semantic knowledge early on is simply wrong (see also, Duyck & De Houwer, 2008). Thus, our follow-up work aims to assess stimulus conflict even earlier on in language learning.

We have already completed one study and have nearly finished a second. The completed experiment was almost identical to the Dutch-French study, except that the study was conducted in Dijon with the L1 language of French and L2 language of English. Our a priori assumption was that English-language competence in France would be notably worse than French-language competence in the Flemish region of Belgium. This turned out to be only partially true. L2 language competence was, as anticipated, lower in our new sample, but not substantially lower. Encouragingly, however, the results exactly replicated Schmidt, Hartsuiker, and De Houwer (2018): both stimulus and response conflict for L1 (French) and L2 (English) colour words. The data are presented in Figure 10.11.

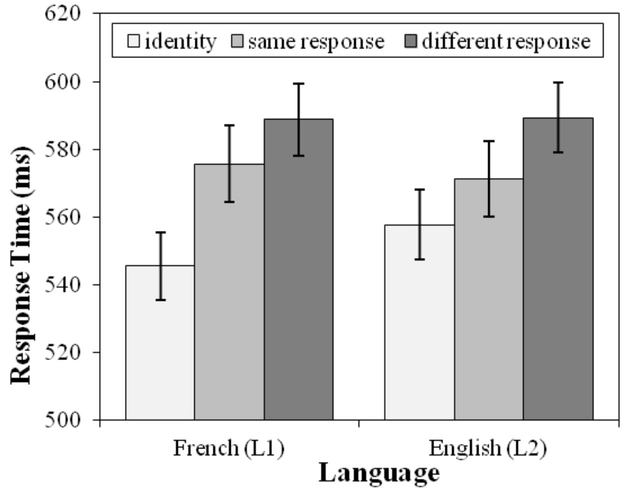

Figure 1.11. Response times with standard errors for French and English colour words from Experiment 1 of Šaban and Schmidt (2019).

Our second experiment pushes the same experimental logic to a further extreme. In particular, we are exploring whether stimulus conflict is observed in a completely unfamiliar language after a brief period of language training within the experiment. In particular, participants first learned four Croatian colour words. This was done in two initial phases. In the initial *learning phase*, participants were presented a Croatian colour word (“crvena” [red], “plava” [blue], “zelena” [green], or “siva” [grey]) along with its French (L1) translation (respectively, “rouge,” “bleu,” “vert,” and “gris”) on each trial. Participants were informed in advance that the words were Croatian and that they were to learn their meaning. Next, participants performed a *matching phase*, in which there were two types of trials. On some trials, participants were presented with one of the Croatian colour words and they had to choose which of the four French colour words were the appropriate translation. On other trials, it was the reverse, where participants were presented a French colour word and had to choose the correct Croatian translation. In a final *test phase*, participants performed the same 2-to-1 mapping Stroop task as in the previous experiments. Data collection is still ongoing, but we anticipate that a stimulus conflict effect should emerge in this experiment, even though the period of exposure to Croatian colour words was extremely short (approximately 10 minutes).

Chapter 2 – Contingency Learning

Published Work

Brief background

One of the basic requirements of the human cognitive system, if not *the* most basic, is our ability to learn regularities between events in our environment (Allan, 2005; Beckers, De Houwer, & Matute, 2007; Shanks, 2010). Contingency learning is the basic building block for causal learning, knowledge acquisition, and the formation of the expectancies that make our world feel ordered rather than chaotic. For a short (scientific) encyclopedia article on human contingency learning, see Schmidt (2012). One of my main interests is the rules via which we learn contingent regularities, especially in the context of incidental learning. That is, I am interested in the progression of learning during a task in which it is not the express goal of the participant to learn. There are many variants of incidental learning paradigms. For instance, in implicit sequence learning, participants are tasked with responding to a target stimulus, but the series of stimuli and/or responses follow a predictable sequence (Nissen & Bullemer, 1987). Learning of the sequence (e.g., as indicated by faster responses when the sequence is predictable rather than unpredictable) is therefore incidental to the main goal of simply responding to the stimulus of the current trial. My work takes a slightly different approach by having participants respond to target stimuli (e.g., print colours) while a "distracting" stimulus serves as a predictive cue.

Colour-word contingency learning

In early work as a Master's student, I introduced the *colour-word contingency learning paradigm* as a means to study incidental learning (Schmidt, Crump, Cheesman, & Besner, 2007; for related paradigms, see Carlson & Flowers, 1996; J. Miller, 1987; Mordkoff & Halterman, 2008; Musen & Squire, 1993). The structure of the task is similar to a colour-word Stroop task (see Chapter 1) in some ways. On each trial, a participant is presented with a coloured word and asked to identify the print colour. The words are neutral (i.e., colour-unrelated). The prototypical key manipulation, illustrated in Table 2.1, is that each neutral word is presented most often in one colour (e.g., "move" most often in blue). *High contingency* trials are those in which the word is presented in the expected colour (e.g., "move" in blue) and *low contingency* trials are those in which the word is presented in another colour (e.g., "move" in green). Learning of the contingencies is indicated by faster and more accurate responses to high relative to low contingency trials. Across multiple studies, we have now observed this learning effect robustly, with essentially every participant showing the expected effect in both response times and error rates.

Table 2.1. Prototypical colour-word contingency learning manipulation.

Colour	Word		
	move	sent	tell
blue	8	1	1
red	1	8	1
green	1	1	8

Interestingly, these learning effects appear very quickly in the course of the experiment. As illustrated in Figure 2.1, the effect is already present within the first blocks of trials, and only increases moderately thereafter. This rapid acquisition is not unlike the learning observed in other learning procedures. In hidden covariation detection procedures, learning has been observed as quickly as after just one consistent pairing (Lewicki, 1985, 1986; Lewicki, Hill, & Czyzewska, 1992). Similarly, learning of sequences is observed after very small training periods (Nissen & Bullemer, 1987). Also in the Hebb digit task, learning is rapid (Mckelvie, 1987).

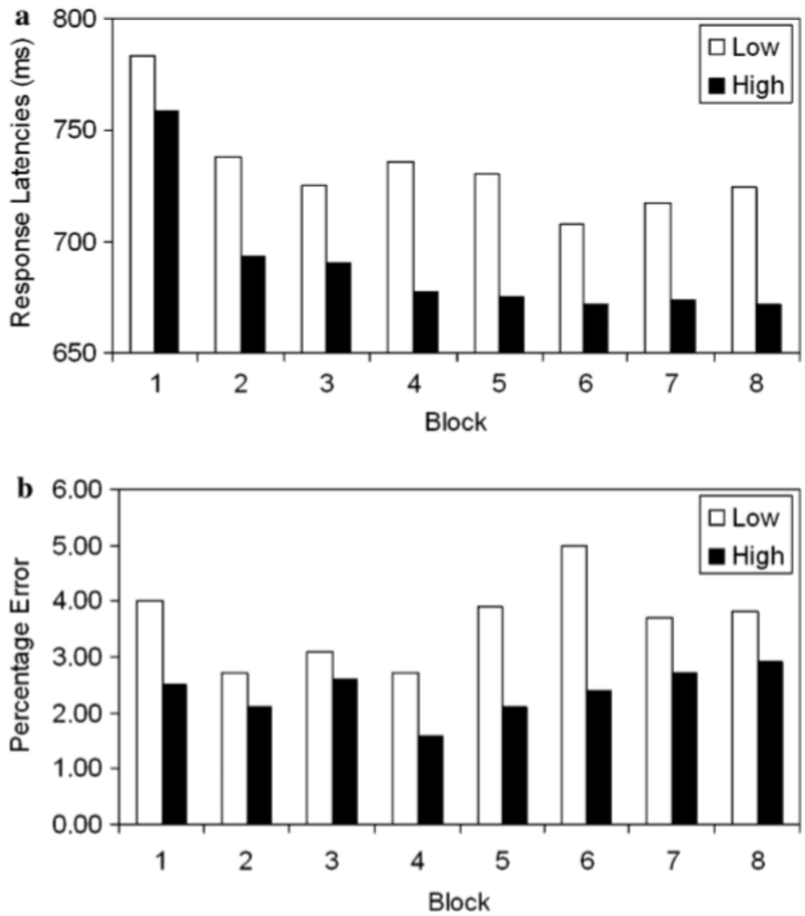

Figure 2.1. Acquisition of contingencies across blocks as observed in Schmidt and colleagues (2007).

We also observed, as illustrated in Figure 2.2, that the contingency effect does not seem to be highly dependent on contingency awareness. While some participants may become explicitly aware of the contingencies in the task (subjectively aware) and others may be sensitive to the contingencies as indicated by above-chance guessing of which words went with which colours (objectively aware), the overall magnitude of the contingency effect does not seem to be heavily influenced by awareness. Aware participants were overall quicker (albeit with more errors), but contingency effects were roughly equivalent in size across awareness groups.

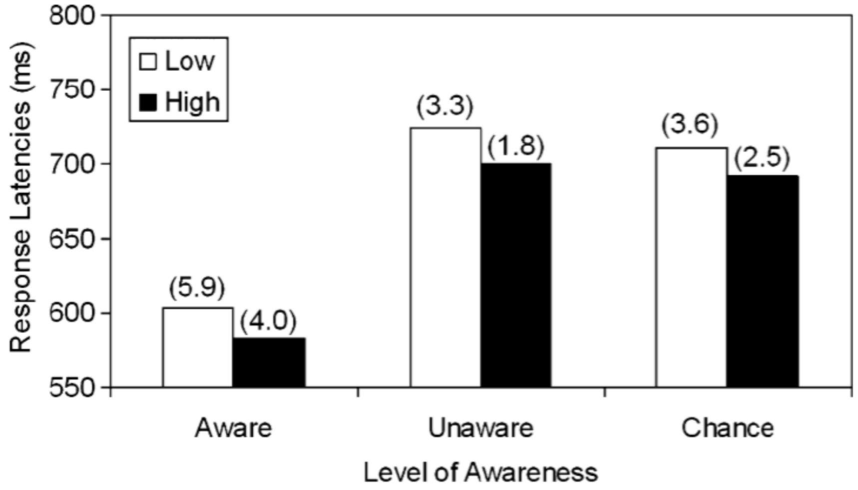

Figure 2.2. Contingency effect as a function of contingency awareness as observed in Schmidt and colleagues (2007).

We also observed that this learning effect is seemingly exclusively a stimulus-response effect, rather than a stimulus-stimulus effect, as illustrated in Figure 2.3. In particular, using the same sort of 2-to-1 mapping manipulation described in the Stroop section of this thesis (Chapter 1), we found that participants responded faster to trials in which the word was presented in the expected colour ("stimulus match") and equally faster when the word was presented in another colour associated to the same response (e.g., "move" in green, where move is presented infrequently in green, but the colour that "move" is presented in frequently is mapped to the same key as green). Responses were only slower when the word was printed in a colour mapped to a different response key. In other words, these results suggest that participants are learning which response to make (e.g., key to press) on the basis of the predictive stimulus (word) and are inconsistent with the notion that participants are learning associations (e.g., semantic) between words and colours. These findings are related to ongoing work (e.g., in language learning; see previous chapter) investigating when or with what kind of training a stimulus effect emerges.

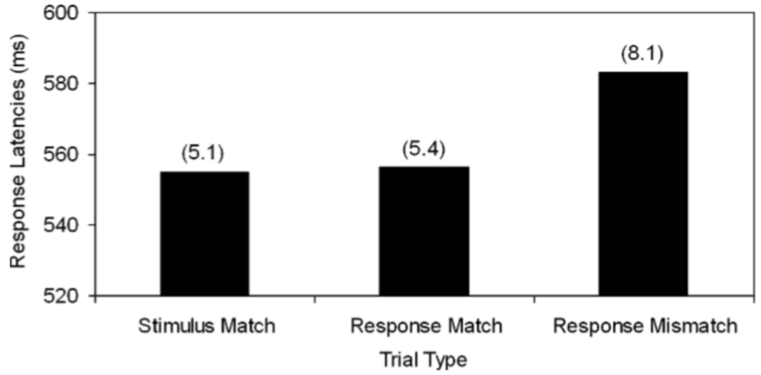

Figure 2.3. Response latencies in milliseconds according to trial type (stimulus match, response match, and response mismatch) from Experiment 4 of Schmidt and colleagues (2007). Percentage errors appear in brackets.

Rapid acquisition

In subsequent reports, we have again observed that contingency effects emerge very rapidly. That is, already within the very first trials of the experiment, the difference in performance between high and low contingency trials is already observed (e.g., the first 18 trials in Schmidt, De Houwer, & Besner, 2010), as illustrated in Figure 2.4. Interestingly, we also observed that once the contingency is removed (i.e., each word presented equally often in all colours), *unlearning* of the previously trained contingency is quite rapid. In particular, Blocks 4-12 contained no contingency in the figure. Within a relatively small number of trials, the effect gets vanishingly small. We can still observe effects that persist over a number of trials, but these results seem to suggest that *recency* of stimulus pairings is especially important.

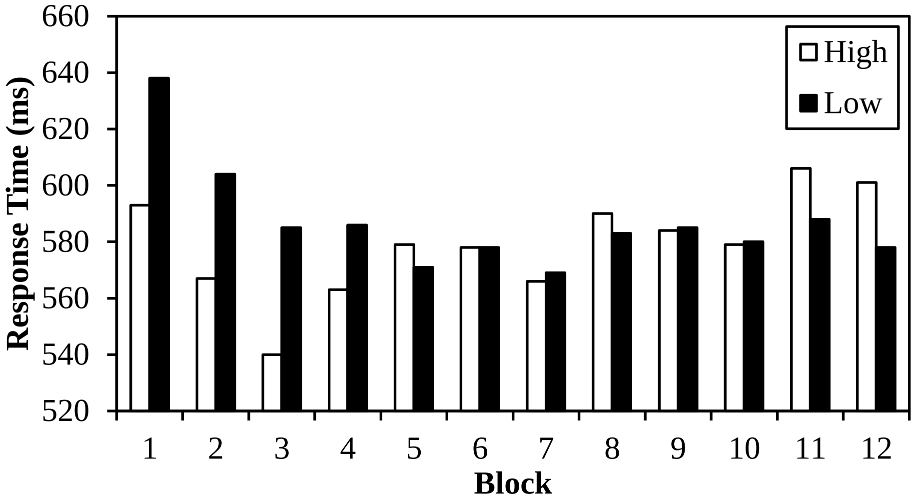

Figure 2.4. High and low contingency response latencies in milliseconds as a function of block from Schmidt and colleagues (2010).

Similarly, if contingencies are reintroduced after an unlearning phase (even a relatively extended one), reacquisition of the contingency is almost immediate (Schmidt & De Houwer, 2016b). This is illustrated in Figure 2.5. After unlearning (grey), the contingency was reintroduced and the learning effect re-emerged quickly. In fact, learning is generally so rapid that it is difficult to study "rates" of learning: the effect is there and almost at a maximum right from the start. Said differently, the acquisition curve is so steep early on that it is hard to study the development of learning across trials.

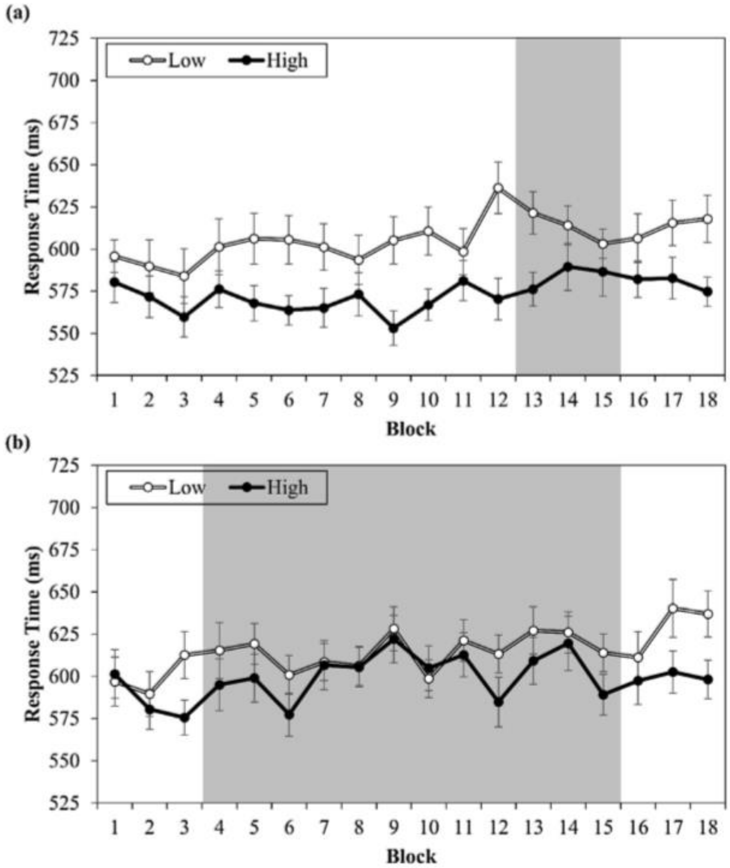

Figure 2.5. High and low contingency response times from Schmidt and De Houwer (2016b) with standard errors for (a) long learning phase (short unlearning phase) and (b) short learning phase (long unlearning phase). Unlearning phase marked in grey.

Awareness and instruction

Learning effects in the colour-word contingency learning procedure seem to be at least primarily implicit in nature. Participants oblivious to the contingency manipulation still show large and robust contingency effects. In most studies, we have not observed clear indications that contingency awareness even boosts the contingency effect. On the other hand, in a series of studies we explicitly told participants in advance what the high contingency pairings would be and asked them to remember these contingencies (Schmidt & De Houwer, 2012d). This did boost contingency effects, as shown in Figure 2.6. In a second study, we found that such instructions on their own were not enough to produce a “contingency effect.” That is, participants were told about contingencies that would supposedly exist between the words and colours, but in the following trials each word was actually presented equally often in all colours. Participants did not respond faster or more accurately to the instructed pairings. On the other hand, a third experiment did observe that false instructions can influence performance in another way. Participants were given instructions about the word-colour contingencies, but then during actual training a different contingency was present (e.g., a participant might be told that “month” would be presented most often in red, but it was actually presented most often in yellow). This did impair learning, as illustrated in Figure 2.7.

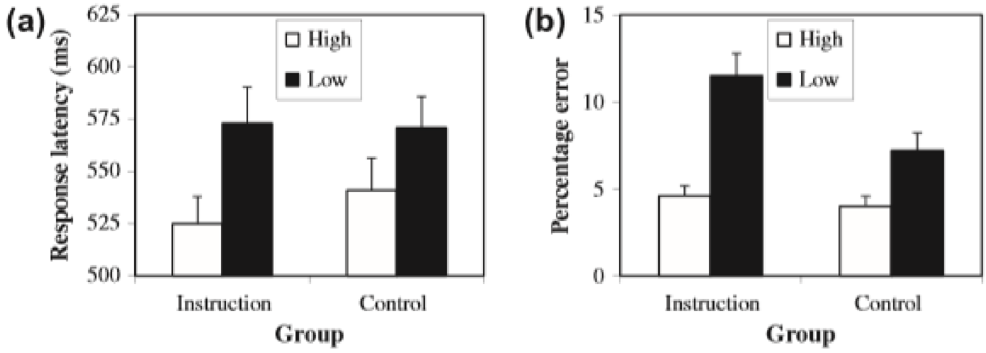

Figure 2.6. (a) response latencies and (b) error percentages as a function of contingency and instruction group from Experiment 1 of Schmidt and De Houwer (2012d). The bars represent standard errors.

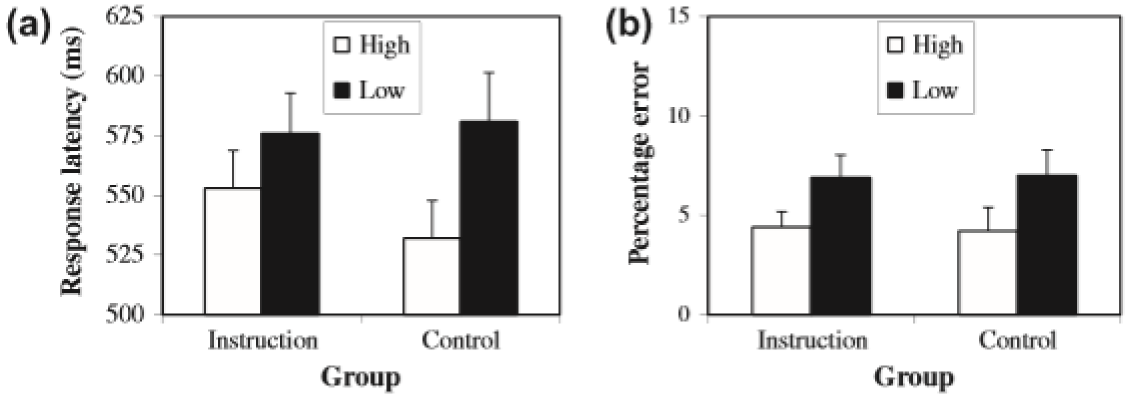

Figure 2.7. (a) response latencies and (b) error percentages as a function of contingency and instruction group in Experiment 3 of Schmidt and De Houwer (2012d). The bars represent standard errors.

Relatedly, in another study we told participants that there would be contingencies between words and colours, but not what the exact pairings would be (Schmidt & De Houwer, 2012a). That is, they were told that one word would be presented most often in red, another most often in yellow, etc. Participants were asked to try to figure out the pairings. This, too, boosted contingency effects, as shown in Figure 2.8. Thus, at least in some cases, explicit awareness may increase learning.

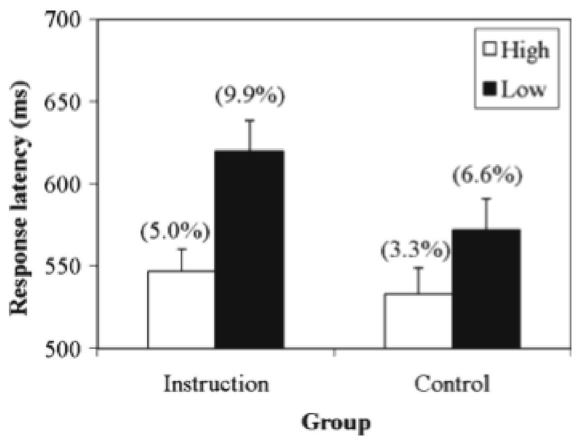

Figure 2.8. (a) response latencies and (b) error percentages as a function of contingency and instruction group in Schmidt and De Houwer (2012a). The bars represent standard errors.

Although seemingly mostly incidental in nature, we have observed that working memory resources may be required to learn and use contingency knowledge (Schmidt et al., 2010). In particular, participants were required to perform a secondary digit span task (i.e., remember some number of digits for a subsequent test of recognition), which could either be easy (2 digits) or hard (5 digits). The contingency effect is eliminated if participants had to do the hard digit span secondary task during either acquisition (i.e., learning phase) or test (i.e., contingency-absent test phase).

Category-level learning

In other work (Schmidt, Augustinova, & De Houwer, 2018), we explored whether learning in this type of contingency task is exclusively based on individual items or whether learning could be more abstract in nature. Even though in past reports many different stimulus dimensions have been used for both the task-irrelevant distracter (e.g., shapes, words, nonwords, colours) and task-relevant target (e.g., colours, colour words, neutral words, positive/ negatively-valenced words) stimuli (Forrin & MacLeod, 2017; Levin & Tzelgov, 2016; Schmidt & De Houwer, 2012b, 2012c), it was always the case that single, frequently repeated stimuli were the predictive stimuli (e.g., three words as predictive stimuli of colours). However, learning (particularly human) is often based on abstract information (Brady & Oliva, 2008; Emberson & Rubinstein, 2016). Using language, for instance, we can learn about conceptual relations without necessarily referring to specific stimuli.

We were therefore interested in exploring more “abstract,” category-level learning. In particular, we developed a variant of the colour-word contingency learning paradigm in which each word was presented once only in the task. However, each word (of which there were many) belonged to one of three categories (animals, professions, or verbs). Each *category* of words was presented most often in one colour (e.g., animals most often in blue). Thus, pretend that a participant is presented the word “rabbit” partway through the experiment. Though the participant had never been presented this word in the experiment before the current trial, many other animals were already presented in the task. Most of these animal names would have been presented in blue. Thus, while participants have not learned that “rabbit” predicts a blue response, they have learned that animal words predict a blue response.

Indeed, we observed category-level contingency effects: performance was better when the word was presented in the high contingency colour for the category (e.g., “rabbit” in blue) relative to another colour (e.g., “chicken” in red). The results of our Experiment 1 are presented in Figure 2.9. These effects were, however, notably smaller and less robust than in the standard colour-word contingency learning procedure, and emerged more clearly in the latter half of the experiment. From a memory perspective, this makes sense, as the specific stimuli (e.g., “doctor”) are new and only overlap in part (i.e., semantic association) with the stimuli used to establish the contingency (e.g., “nurse,” “firefighter,” etc.). Thus, there is a contingency, but it is a weak and indirect one.

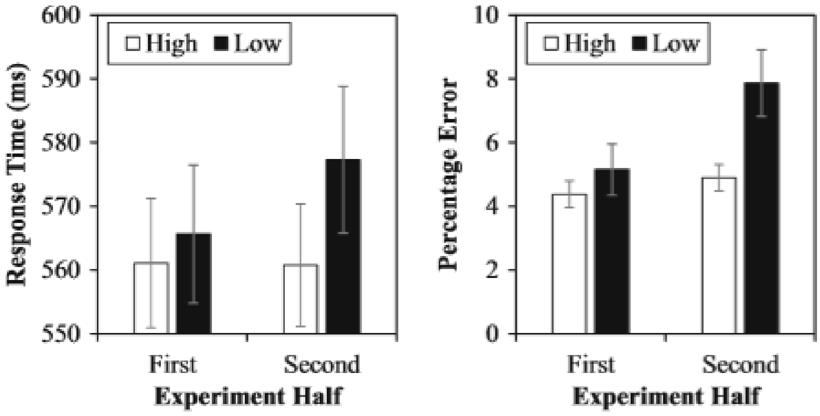

Figure 2.9. Mean response times (left) and percentage errors (right) with standard errors by experiment half and contingency from Experiment 1 of Schmidt, Augustinova, and De Houwer (2018).

Evaluative learning

In Schmidt and De Houwer (2012b), we developed a variant of an evaluative conditioning procedure based around the colour-word contingency learning procedure. Evaluative conditioning (see Hofmann, De Houwer, Perugini, Baeyens, & Crombez, 2010, for a review) refers to a change in liking of a stimulus that results from the pairing of that stimulus with another stimulus (De Houwer, 2007). In a typical evaluative condition (EC) procedure, a neutral *conditioned stimulus (CS)* is paired some number of times with a positive or negative *unconditioned stimulus (US)*. For instance, in flavour conditioning, one neutral taste might be mixed with a pleasant taste and another neutral taste might be mixed with an unpleasant taste. After conditioning, the first neutral taste is typically rated more positively than the second. That is, the valence (positive vs. negative) of a US that a CS is paired with is transferred to the CS. Similarly, if one neutral picture is repeatedly presented along with a smiling face and another neutral picture is repeatedly presented with an angry face, then participants will subsequently rate the former neutral picture more positively than the latter.

In our task, we developed an implicit learning EC procedure. On each trial, the participant was presented with a prime nonword (e.g., "alsan") and this was followed by either a positive or negative target word (e.g., "flowers" or "guns"). The task of the participant was to decide whether the target was positive or negative. Critically, some nonwords were presented most often with positive targets and other nonwords were presented most often with negative targets. Participants responded faster and more accurately to targets when the prime nonword was presented with the expected valence relative to the unexpected valence. In the first of two studies, a small set of positive and negative targets were used. Each of four nonwords was presented most often with one of these targets. In particular, we used the 2-to-1 mapping procedure discussed in the chapter on the Stroop task (Chapter 1). That is, two target positive words were mapped to one key and two negative words were mapped to another key. This allowed us to assess *stimulus match (identity) trials*, where the nonword was presented with the expected target, *valence match (same response) trials*, where the nonword was presented with an unexpected target but of the same valence (e.g., "alsan" with "hug," where "alsan" is normally presented with "flowers," also positive), and *valence mismatch (different response) trials*, where the nonword is presented with a target of the unexpected valence. As illustrated in Figure 2.10, we observed faster response to stimulus and valence match trials relative to valence mismatch trials, with no difference between the former two conditions. This indicates that participants learned the nonword-valence contingencies, but not the nonword-target contingencies. This is similar to our above-mentioned findings with the non-evaluative version of the task, where we observed that participants responded faster and more accurately to both "identity" and "same response" trials, with slower responses to "different response" trials (Schmidt et al., 2007). This again indicates that learning seems to be exclusively (or primarily) stimulus-response based.

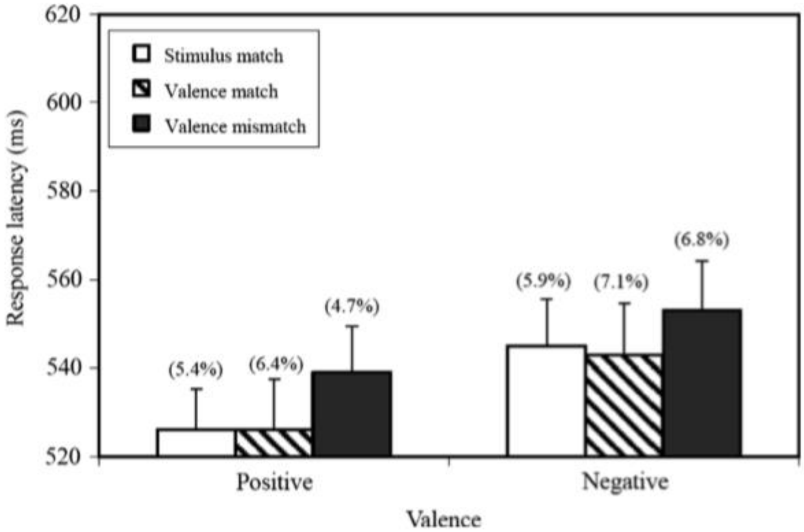

Figure 2.10. Experiment 1 response latencies with standard errors and percentage errors for trial type and valence from Schmidt and De Houwer (2012b).

In a second experiment, we abandoned the 2-to-1 mapping procedure and used a large set of positive and negative target stimuli. Instead, nonwords were presented most often with all stimuli of one valence. For instance, "alsan" may have been presented frequently with all positive stimuli, and rarely with each negative stimulus. Thus, learning, if observed, is likely based on nonword-valence associations and not associations between a nonword and a particular target stimulus. As observed in Figure 2.11, this again produced a robust contingency learning effect. This indicates that learning is not exclusively determined by highly-repeated individual stimulus pairings (related to Schmidt, Augustinova, et al., 2018). In both experiments, we also assessed explicit ratings (the typical EC dependent measure) of the nonwords after training. That is, participants were asked to rate how much the liked each nonword. Nonwords that were presented frequently with positive targets were rated more positively than those presented frequently with negative targets. This rating effect was also highly correlated with the response time and error contingency effects. In Experiment 2, we also assessed contingency awareness. We observed that participants showed poor sensitivity to the contingencies in an objective awareness test. That is, they were guessing at around chance which nonwords were presented most often with positive stimuli and which were presented most often with negative stimuli. Few claimed subjective awareness. Response time, error, and rating contingency effects were not dependent on awareness, whether assessed on a subject or item level (see Baeyens, Eelen, & Van den Bergh, 1990; Pleyers, Corneille, Luminet, & Yzerbyt, 2007; Stahl & Unkelbach, 2009). That is, learning seemed to be primarily unconscious.

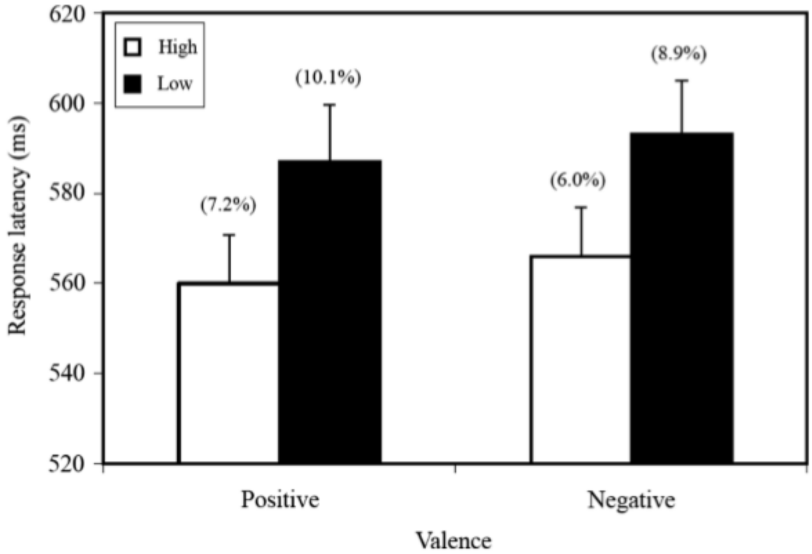

Figure 2.11. Experiment 2 response latencies with standard errors and percentage errors for contingency and valence from Schmidt and De Houwer (2012b).

Temporal contiguity

In other work, we have explored to what extent contingency effects are dependent on the *temporal contiguity* (closeness in time) between the presentation of the predictive stimulus and the target stimulus (Schmidt & De Houwer, 2012c). Contingency (or covariation) learning is, of course, important. Since the advent of associationism in philosophy (e.g., Hume, 1739/1969), temporal contiguity between events has also been considered as one of the crucial factors in detecting the relationships between events (see Buehner, 2005, for a review). Early work on causal perception, for instance, shows that the perception that two events are causally related is strongest when the stimulus onset asynchrony (SOA) between the potential cause and potential effect is very small and quickly weakens as the lag increases (e.g., Michotte, 1946/1963). Using a straightforward example, if you press a “mystery button” and a light in the room turns on almost immediately, then you are likely to attribute the light turning on to pressing the button. On the other hand, if nothing happens immediately after pressing the button, but the light turns on a minute later, you are not likely to attribute the button push as the cause of the light.

In our work, we used nonwords as the predictive stimuli and colour words as the target stimuli in a variant of the colour-word contingency learning task. In particular, we manipulated the onset of the predictive nonwords relative to the onset of the target colour words in a series of studies. In our first three studies, the distracter and target remained on the screen together, but onset at different SOAs. In Experiment 3, SOAs were negative, meaning that the distracter appeared *after* the target. In Experiment 4, presentation duration of the distracter was fixed and we manipulated the inter-stimulus interval (ISI), the time between the offset of the distracter and onset of the target. The results of these studies are presented in Figure 2.12. Globally, the results indicate that contingency effects are seemingly robust to a range of different temporal contiguities. That is, the contingency effect does not seem to vary in magnitude notably with a wide range of stimulus-onset asynchronies (SOAs) or inter-trial intervals (ITIs).

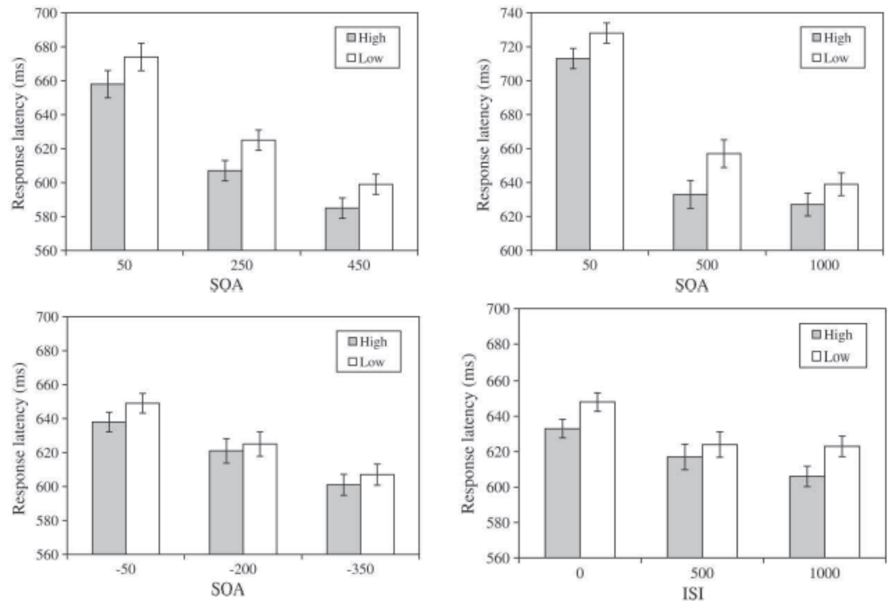

Figure 2.12. Mean response latencies and standard errors for high- and low-contingency trials as a function of stimulus onset asynchrony (SOA) or inter-stimulus interval (ISI) for Experiments 1 (top left), 2 (top right), 3 (bottom left), and 4 (bottom right) of Schmidt and De Houwer (2012c).

Item frequency

In other research (Schmidt & De Houwer, 2016a), we explored whether the contingency effect was due to facilitation for high contingency pairings, interference for low contingency pairings, or whether participants are simply responsive to individual stimulus-response proportions (see also, Lin & MacLeod, 2018). That is, the response time (and error) difference between high contingency and low contingency stimuli could in principle be due to a number of different things. For instance, consider the example of “give” presented most often in purple. First, responses might be speeded to high contingency stimuli (e.g., “give” in purple). Additionally or alternatively, responses might be slowed to low contingency stimuli (e.g., “give” in orange, which is very infrequent). Relative to, for instance, a medium (chance) contingency word presented equally often in all colours, the benefit and/or the cost might be observed.

The frequency of word-colour combinations used in our Experiment 1 is presented in Table 2.2. Two of the words (e.g., “give” and “hear”) were presented most often (60% of the time) in one colour, very rarely (6.7%) in a second colour, and in an intermediate frequency (33.3%) in a third colour. The remaining word (e.g., “make”) was presented equally often (33.3%) in all three colours. These manipulations create five unique trial types. On high contingency trials (white in Table 2.2), the word is presented in its most frequent colour (e.g., “give” in purple). On low contingency trials (red in Table 2.2), the word is presented in its least frequent colour (e.g., “give” in orange). Critically, the manipulation allowed for three types of medium contingency trials. On *biased-word* trials (orange in Table 2.2), a word that is usually predictive of one high contingency response is presented in a medium contingency colour (e.g., “give” in grey, because “give” is normally presented in purple). On *biased-colour* trials (blue in Table 2.2), the word is unpredictive of the correct response, but the colour is most often associated with a particular word (e.g., “make” in purple, given that purple is normally presented with “make”). Finally, on *unbiased* trials (green in Table 2.2), neither the word nor the colour is predictive of any other stimulus (e.g., “make” in grey).

Table 2.2. Adapted colour-word contingency learning manipulation from Experiment 1 of Schmidt and De Houwer (2016a).

Colour	Word		
	give	hear	make
purple	9	1	5
orange	1	9	5
grey	5	5	5

We considered four possible accounts of how contingency knowledge is retrieved and impacts responding. The first we called the *prediction benefit* account (e.g., Schmidt & Besner, 2008). According to this account, a response is anticipated if one of the potential responses is highly likely (e.g., greater than chance), and accurate response prediction benefits performance. As such, high contingency trials will be faster than all other trials. Critically, the prediction benefit account assumes that predicting a response does not impair the ability of the system to make any of the remaining responses (i.e., the predicted response does not compete with the non-predicted responses). Thus, there should be no costs for low contingency trials and resultantly no differences between the low, biased-word, biased-colour, and unbiased trial types.

Another contender we called the *misprediction cost* account. According to this account, if the distracting word is strongly predictive of one response, then (a) making that response will be facilitated (i.e., as in the prediction benefit account) and (b) making any other response will be impaired via response competition. That is, contingency information is used to activate the anticipated (i.e., high contingency) response and this activated response then competes with all other contending responses. Both low contingency and biased-word trials should be slowed by this sort of interference, whereas biased-colour and unbiased trials should not be.

A third possibility we called the *bidirectional cost* account, which is identical to the misprediction cost account, except that it is additionally assumed that it is harder to make a colour response that is frequently associated with a specific word that is not present on the current trial. For instance, if "give" is presented most often in purple, then participants might be hesitant to make a purple response if they do not see the (expected) word "give." Thus, biased word, biased colour, and low contingency trials should be slowed relative to unbiased trials.

Finally, the fourth possibility we called the *pure proportion* account. Unlike the preceding three accounts, the pure proportion account suggests that response time will be determined by the proportion with which a given distracting word is presented with a given response. Thus, we should expect fast responses for the high contingency trials, slow responses for low contingency trials, and intermediate and similar response times for all three medium contingency conditions. As can be observed in Figure 2.13, it was this fourth pattern of results that we observed.

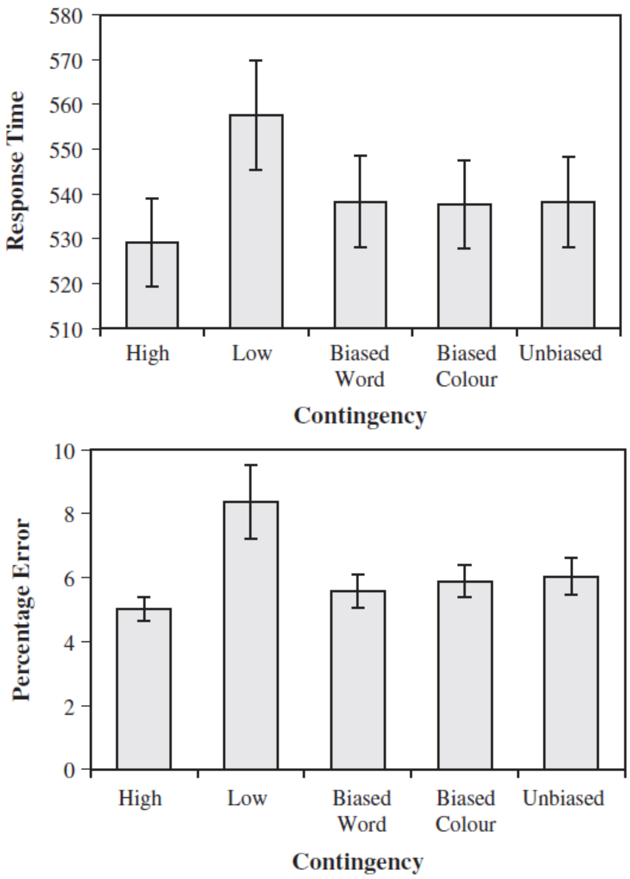

Figure 2.13. Mean response times (top) and percentage errors (bottom) with standard errors as a function of trial type from Experiment 1 of Schmidt and De Houwer (2016a).

Thus, Experiment 1 in Schmidt and De Houwer (2016a) seems to support the notion that response times simply speed up relative to the frequency of co-occurrence of a target and distracting stimulus. This might further imply that low contingency trials are not slowed down by virtue of their low frequency, but are rather sped up slightly by virtue of prior exposure (just not as much, of course, as more frequent pairings). As further support for this notion, we conducted a second experiment in which we compared responding to high contingency, low contingency, and once-presented novel word trials. Based on the proportion/frequency account discussed above, we predicted that low contingency trials would actually be responded to (slightly) faster than a novel word control. As observed in Figure 1.14, this is exactly what we observed. This account does, however, predict more errors for low contingency trials, which was also observed.

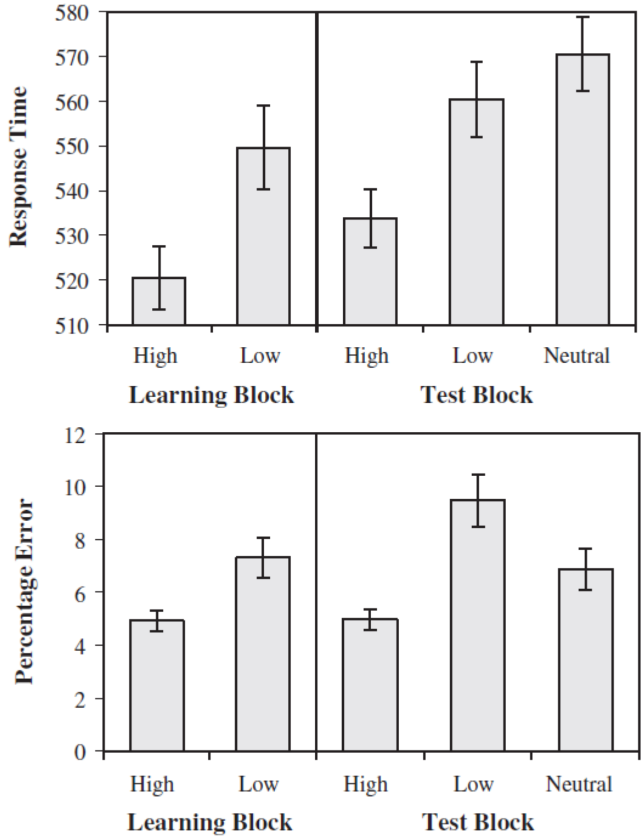

Figure 2.14. Mean response times (top) and percentage errors (bottom) with standard errors as a function of trial type from Experiment 2 of Schmidt and De Houwer (2016a).

Cue competition and incidental learning

In a recent series of experiments, we explored whether cue competition effects can be observed during incidental learning (Schmidt & De Houwer, 2019), specifically, overshadowing and blocking. *Overshadowing* is the observation that when two stimuli, termed Stimulus A and Stimulus X, are presented together and followed by an outcome (i.e., AX+ trials), evidence for learning of the X-outcome relation is weaker compared to a condition in which only Stimulus X is paired with the outcome (i.e., X+ trials; Pavlov, 1927). For instance, rats can easily learn that a light or a tone predicts a food reward, but when a light and a tone are presented together with the reward, the rat may only weakly learn the individual light-food and tone-food relations. Thus, the light and tone "overshadowed" each other (or alternatively, one cue overshadows the other, but not the reverse). *Blocking* (Kamin, 1969) is the observation that after learning that Stimulus A (e.g., light) predicts an outcome (e.g., food; A+), presentation of Stimulus A along with a new Stimulus X (e.g., tone) with the same outcome (food; i.e., AX+) weakens learning of the Stimulus X-outcome relation as compared to a condition with only AX+ trials (overshadowing). That is, even though Stimulus X and the outcome co-occurred during the compound AX learning phase, little learning of this regularity is observed. Thus, Stimulus A "blocks" learning about Stimulus X.

Several theoretical accounts of blocking and overshadowing have been presented over the years (e.g., Mackintosh, 1975; Pearce & Hall, 1980; Sutherland & Mackintosh, 1971). For instance, the Rescorla-Wagner model (Rescorla & Wagner, 1972) postulates that associative connections are only updated to the extent that an outcome was unexpected. This can account for blocking: the A-outcome association is learned early on, because the outcome is initially unexpected. When Stimulus A and Stimulus X are subsequently presented together with the same outcome (AX+), the outcome is already expected on the basis of the presence of Stimulus A. As a result, very little is learned about the Stimulus X-outcome relation. The

Rescorla-Wagner model can also account for overshadowing: the first time that A and X are presented together and followed by an outcome (AX+), the outcome is unexpected. Therefore, learning (i.e., association formation) occurs for both stimuli. On subsequent AX+ trials, however, both stimuli contribute to the prediction of the outcome, resulting in less prediction error and thus less strengthening of associations compared to a condition in which only X was present on all trials (X+; i.e., prediction error is lower with two predictive stimuli, weakening further learning for both; see R. R. Miller, Barnet, & Grahame, 1995).

It was less clear whether cue competition can be found in incidental learning tasks, that is, tasks in which predictive cues were not task relevant (targets). In most past reports, learning the contingency was the explicit goal and participants had ample time to reflect on the events that they saw (Chapman & Robbins, 1990; Dickinson, Shanks, & Evenden, 1984; Gluck & Bower, 1988; Le Pelley & McLaren, 2001). Thus, in our report we utilised the colour-word contingency learning paradigm for studying overshadowing and blocking. For the "Stimulus A" and "Stimulus X" we used neutral words and shapes (respectively or vice versa), with a target print colour. This allowed us to have: (a) word-only trials (coloured word, no shape), (b) shape-only trials (coloured shape, no word), and (c) compound stimulus trials (coloured word and shape). The procedure and example compound stimuli are presented in Figure 1.15.

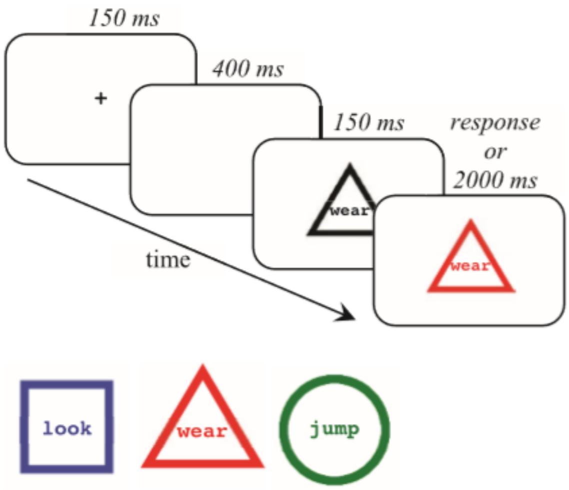

Figure 1.15. General trial procedure (top) and example compound stimuli (bottom) from Schmidt and De Houwer (2019). After a fixation cross and blank screen, a word-shape compound (or just a word or shape) is presented briefly in black, then changed to one of the target colours.

In an overshadowing experiment, participants were presented with compound words and shapes in colour (overshadowing) or with just coloured words (words-only) or just coloured shapes (shapes-only). Each word, shape, or word-shape compound was presented most often in one colour, as in the typical colour-word contingency learning paradigm. The results of this Experiment 1 are presented in Figure 1.16. Notably, overshadowing was not observed in the test phase. That is, there was a robust contingency effect for both words and shapes in the overshadowing condition, and these contingency effects for the individual words and shapes were not smaller than the contingency effects observed for stimuli trained alone (i.e., words in the words-only condition and shapes in the shapes-only condition).

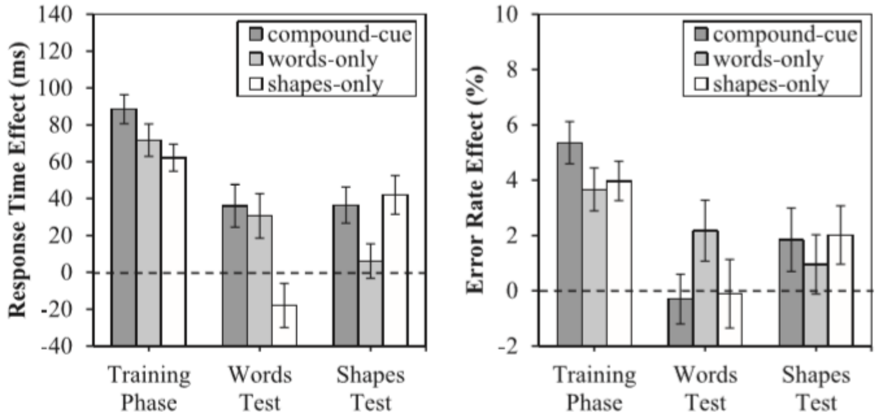

Figure 1.16. Experiment 1 response time (left) and percentage error (right) contingency effects (low – high contingency) from Schmidt and De Houwer (2019) as a function of group and phase, with standard error bars.

In a second experiment, we tested for blocking. One group of participants were initially trained with coloured words only (words-first) and another group was initially trained with coloured shapes only (shapes-first). Both then proceeded to a compound cue training (i.e., coloured word-shape compounds). If blocking occurs, then we would expect larger contingency effects for the initially-trained dimension (e.g., shapes in the shapes-first condition) than for the "blocked" dimension (e.g., shapes in the words-first condition). We also included an overshadowing control group, who always saw compound word-shape stimuli. In addition, we assessed the intentionality of learning by instructing half of the participants in advance about the presence of contingencies, which they should intentionally try to learn. The results are presented in Figure 1.17. Notably, the non-instructed participants did not show blocking. The contingency effects for the "blocked" dimension were not smaller than the contingency effects for the "blocking" dimension. Instructed participants, however, did produce a blocking effect. Similar blocking effects were also observed in explicit judgements about the contingencies, which were robustly larger in instructed participants. Collectively, the results suggest that cue competition effects, like blocking and overshadowing, require explicit reasoning about the contingencies in the task. When learning is purely incidental and we test automatic influences of cue competition (e.g., in response times and errors), cue competition does not seem to be present.

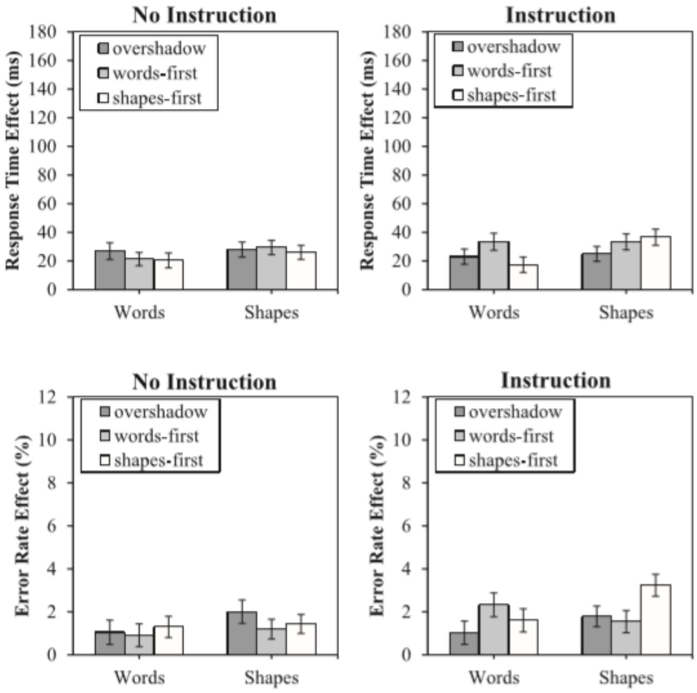

Figure 1.17. Experiment 2 response time (top) and percentage error (bottom) contingency effects (low – high contingency) in Schmidt and De Houwer (2019) as a function of group and stimulus type during the test phase, with standard error bars.

Works in Progress and Future Directions

Unitary mechanism of learning and binding

In two recent papers (Schmidt, Giesen, & Rothermund, 2018; Giesen, Schmidt, & Rothermund, 2019), we have been exploring whether contingency learning effects and shorter-term binding effects might be coherently explained by one mechanism. In addition to the learning of regularities across many events, our ability to bind our experiences into memory traces for later retrieval is also fundamental (Hommel, 1998, 2004; Hommel, Müsseler, Aschersleben, & Prinz, 2001; Logan, 1988). Especially recent experiences can have a particularly potent influence on our behaviour (Grant & Logan, 1993). In the binding literature, researchers study the influence of recently-experienced events on performance. Although there are several variants of S-R binding (or feature integration) procedures (Hommel, 1998), consider the distracter-response binding paradigm (Frings, Rothermund, & Wentura, 2007; Rothermund, Wentura, & De Houwer, 2005). Participants respond to a target (e.g., print colour) while ignoring a distracter (e.g., word). Unlike a contingency learning experiment, the distracters are *not* correlated with targets/responses (e.g., each distracting word is presented equally often in all colours/with all responses). Instead, we assess performance on the second of two trials as a function of whether (a) the distracter repeats (e.g., “find” followed by “find”) or changes (e.g., “find” followed by “walk”), and (b) the target colour (and therefore response) repeats (e.g., blue followed by blue) or changes (e.g., blue followed by red). The standard finding is that when the (target) response repeats, participants are faster to respond when the distracter also repeats (sometimes termed a *complete repetition*), relative to when the distracter changes (*partial response repetition*). However, when the (target) response changes, participants are (a little) slower to respond when the distracter repeats (*partial word repetition*), relative to when the distracter and response both change (*complete alternation*). Globally, response repetitions are faster than response changes, but the Stimulus Relation × Response Relation interaction is most crucial.

We previously (Schmidt et al., 2016) made the argument that, at least in principle, these *binding effects* might be due to the same mechanism as contingency learning (see Chapter 4 for more information). In particular, we might think of the binding interaction as a short-term consequence of learning. For instance, if “find” was just presented in blue, then presentation of “find” again will bias another blue response, for a similar reason as why “find” would bias a blue response if it was presented *frequently* (rather than recently) in blue. This will speed us up if we do need to make a blue response, but slow us down if we need to make a different (e.g., red) response. Thus, binding effects could be regarded as a short-term consequence of learning (or, conversely, learning effects could be regarded as a long-term consequence of many bindings).

On the other hand, there could be more to binding effects than just short-term learning and there could be more to learning than just the accumulation of many bindings. In our recent papers, we have explored to what extent learning effects in the colour-word contingency learning procedure might be explained as a summation of many prior bindings. Globally, our results have indicated that, at minimum, contingency learning effects from the colour-word contingency learning paradigm are mostly accounted for by a summation of recent bindings in memory, providing some early support for the “unitary mechanism” view.

Counterconditioning and habit formation

In other recent work (Schmidt, De Houwer, & Moors, 2018), we have been exploring whether contingencies do become more “stable” with *extended* practice. In some multiple day training studies, we explored how an *overtrained* contingency would be influenced by introduction of a new contingency. For instance, if “move” was presented most often in blue early on, then after two days of training “move” is suddenly presented most often in green, would we still observe fast responses to “move” in blue, or would we observe that the “old” contingency is quickly erased and participants quickly learn the move-green contingency? Early results are mixed. In one experiment, there was some (albeit inconsistent) evidence that overtrained contingencies do seem to persist and learning of a new contingency seemed weaker. In subsequent studies, however, we found that both “overtrained” and “weakly trained” contingencies continue to influence participants during counterconditioning. That is, the contingency from early training continues to influence participants even after the contingency no longer applies, even for a contingency that was not trained for so long initially. We also observe acquisition of the *new* contingency as well. That is, relative to low contingency stimulus pairings that were never high contingency during the task, responding is facilitated for both the pairings consistent with initial (but no longer applicable) contingency and for the newly-introduced contingency.

Chapter 3 – Attentional Control

Published Work

Brief background

I have also conducted much research on the domain of cognitive and attentional control. In the attentional control literature, one particularly influential theory is known as the *conflict monitoring* or *conflict adaptation* account (Botvinick, Braver, Barch, Carter, & Cohen, 2001). According to this theory, each time we experience conflict (e.g., an incongruent colour word in a Stroop task), control is upregulated and attention is directed away from distracting information (e.g., the word) and/or toward target information (e.g., the colour). This is to avoid further conflict. Relatedly, when conflict is low or not present (e.g., on a congruent trial), control is downregulated and attention is less focused on the target information.

There are two key phenomena that are frequently used to make the case for conflict monitoring. The first is the *proportion congruent (PC) effect* (Logan & Zbrodoff, 1979; Logan, Zbrodoff, & Williamson, 1984). The PC effect is the finding that the congruency effect (incongruent – congruent) is substantially reduced when most of the trials in the experiment are incongruent (e.g., 75% incongruent, 25% congruent) relative to when most of the trials are congruent (e.g., 75% congruent, 25% congruent). The typical pattern of results is presented in Figure 3.1. Although initial reports were interpreted in a very differently, the standard explanation of the PC effect is in terms of attentional control (e.g., Lowe & Mitterer, 1982). In particular, it is argued that attentional control is stronger when most of the trials are incongruent, thereby reducing the influence of the distracting word on performance, as illustrated in Figure 3.1. The result is a smaller congruency effect. In contrast, when trials are mostly congruent, attentional control is weaker and the congruency effect is larger. Together, this produces an interaction between PC and congruency, which is referred to as the PC effect.

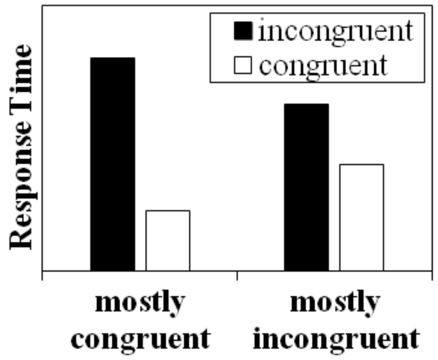

Figure 3.1. Standard proportion congruent (PC) effect. The congruency effect is smaller when most trials are incongruent.

A second phenomenon used to make the case for attentional control is the *congruency sequence effect (CSE)*, sometimes also called the Gratton effect, sequential congruency effect, or (conflating interpretation with behavioural effect) the conflict adaptation effect (Gratton, Coles, & Donchin, 1992). The CSE is the observation that the congruency effect is substantially reduced following an incongruent trial relative to a congruent trial. The typical finding is illustrated in Figure 3.2. Although initially interpreted in terms of expectancies, the CSE is typically interpreted in terms of conflict monitoring (e.g., Botvinick, Nystrom, Fissell, Carter, & Cohen, 1999). In particular, it is argued that after experiencing a conflicting incongruent trial, control is increased. Thus, attention to the distracter is diminished on the following trial, thereby reducing the congruency effect. In contrast, after a congruent trial (where conflict is low or absent), control is diminished and the word has a larger impact on performance.

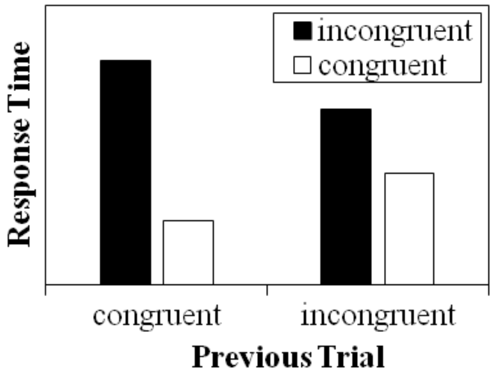

Figure 3.2. Example congruency sequence effect. The congruency effect is smaller on the current trial if the previous trial was incongruent.

In my research, I have argued that evidence for conflict monitoring should be taken with caution. Both the PC and CSE effects are plagued with confounding factors unrelated to conflict or attention. Related to this, I have provided two literature reviews (Schmidt, 2013b, in press), in which I have outlined evidence against the conflict monitoring perspective. One of these is very recent, but also receiving attention rapidly. The older has already accumulated a large number of citations. In addition to these two review articles outlining evidence for or (mostly) against conflict monitoring, I was also the lead editor for a special issue on the topic (Schmidt, Notebaert, & Van den Bussche, 2015).

Contingency learning confound and ISPC

In Schmidt and Besner (2008), we showed that the PC effect is confounded by simple stimulus-response contingency biases. In particular, we investigated the *item-specific PC (ISPC)* effect. Jacoby, Lindsay, and Hessels (2003) manipulated proportion congruency for each item (i.e., each colour word) such that some words were presented most often in their congruent colour (e.g., "blue" most often in blue) and other words were presented most often in a particular incongruent colour (e.g., "orange" most often in yellow). This design is illustrated in Table 3.1. A proportion congruent effect was still observed, even though high and low proportion congruent stimuli were intermixed in the same block of trials. As Jacoby and colleagues pointed out, this ISPC effect is difficult to accommodate within the conflict monitoring framework, because it would have to be assumed that participants are modulating attention to the word on a trial-by-trial basis depending on the identity of the word (e.g., if the word is BLUE, then the word is attended, but if the word is ORANGE, then the word is ignored). In essence, to defend the conflict monitoring account it would have to be maintained that participants decide whether to attend to the word after they have already read it. This is the position that has been taken by most (e.g., Blais, Robidoux, Risko, & Besner, 2007; Verguts & Notebaert, 2008).

Table 3.1. Item-specific proportion congruent manipulation.

Colour	Word			
	blue	red	yellow	orange
blue	3	1		
red	1	3		
yellow			1	3
orange			3	1

However, we pointed out that there is a simple stimulus-response contingency confound in this design, much comparable to the deliberately manipulated contingencies discussed in Chapter 2. For the mostly congruent stimuli (e.g., “blue”), each word is presented most often in the congruent colour (e.g., “blue” 75% of the time in blue) and very infrequently in each incongruent colour (e.g., “blue” only 25% of the time in red). Ignoring congruency altogether, this means that the word is strongly predictive of the likely response that needs to be made to the colour. This prediction will be correct on congruent trials (e.g., when “blue” is presented in blue, as expected), speeding responding, but incorrect on incongruent trials (e.g., when “blue” is presented in an unexpected colour, such as red). In the mostly incongruent condition, depending on the exact manipulation, the word is either unpredictive of the likely response (e.g., “blue” presented 25% of the time in blue, red, green, and yellow) or predictive of a specific *incongruent* response (e.g., “yellow” 75% of the time in orange and 25% in yellow, as in Table 3.1). This biases the congruency effect in the reverse direction. Note that this “contingency account” can explain the PC effect without making any assumptions about cognitive control, conflict, or attention. Instead, simple stimulus-response learning explains the interaction. This is illustrated in Figure 3.3. That is, there is a main effect of congruency and a main effect of contingency, but nothing else.

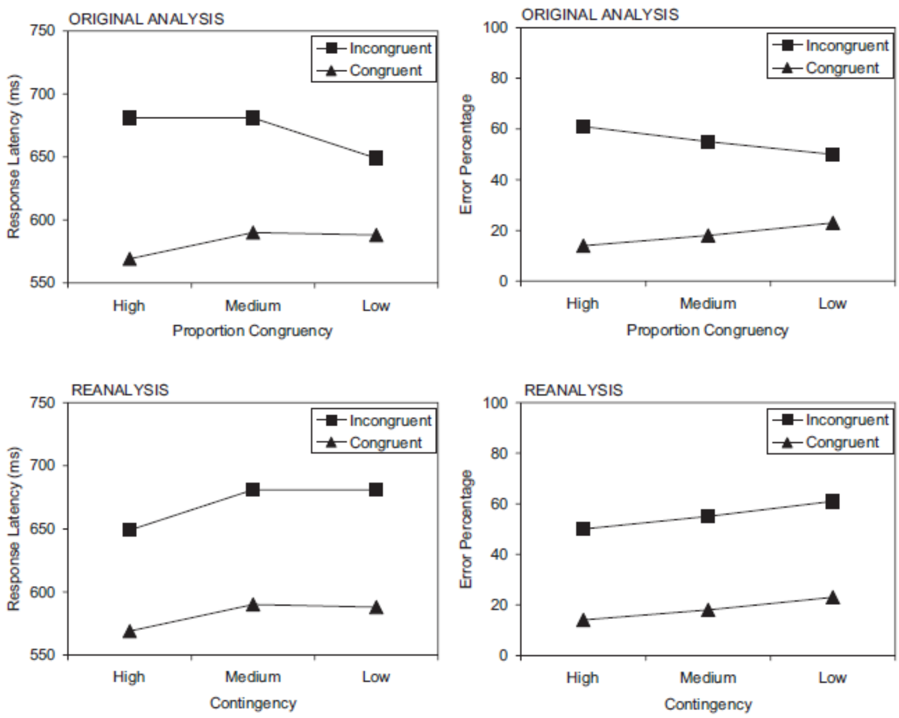

Figure 3.3. Bottom: mean response latencies in milliseconds from Jacoby, Lindsay, and Hessels (2003) for congruent and incongruent trials with high, medium, and low contingencies, as reanalyzed by Schmidt and Besner (2008). Top: original organization of the data. Error data on the right.

Further dissociation work

Subsequent research by myself and others has lent even more credence to the contingency account of the ISPC effect. For instance, if one separately manipulates contingency and attentional control biases using dissociation procedures (e.g., Hazeltine & Mordkoff, 2014; Schmidt, 2013a), very robust evidence of contingency learning biases are observed. No evidence for an additional attentional control contribution to the effect was observed. The manipulation from Experiment 1 of Schmidt (2013a) is illustrated in Table 3.2. Here, we again have two mostly congruent words (“blue” and “green”) and two mostly incongruent words (“red” and “yellow”). However, there are three types of incongruent trials.

In light grey are incongruent trials with a mostly congruent word that is low contingency (i.e., infrequent word-colour pair). In medium grey are incongruent trials with a mostly incongruent word that is also low contingency. These two trial types have an equal word-colour contingency, but vary in PC. The conflict monitoring account would therefore predict slower responses to the former condition (i.e., more conflict from incongruent mostly congruent words). Finally, the dark grey cells are trials with a mostly incongruent word that is high contingency (i.e., frequent word-colour pairing). The contingency account would therefore predict faster responses to *these* mostly incongruent trials relative to the low contingency mostly incongruent trials. We therefore have a direct dissociation, with one measure of contingency learning and another measure of attentional control.

Table 3.2. Modified ISPC manipulation in Schmidt (2013a).

Colour	Word			
	blue	green	red	yellow
blue	7	1	1	1
green	1	7	1	1
red	1	1	1	7
yellow	1	1	7	1

The results from Schmidt (2013a) are presented in Figure 3.4. As can be observed a very large and robust contingency learning effect is observed when comparing trials with equivalent PC (mostly incongruent) but differing contingencies (high vs. low). In contrast, there is no difference at all (with high power to detect an effect) between mostly congruent and mostly incongruent stimuli of equal contingencies. This, along with other results, strongly suggests that the ISPC effect is due to word-response contingency learning and not due to item-specific attentional control.

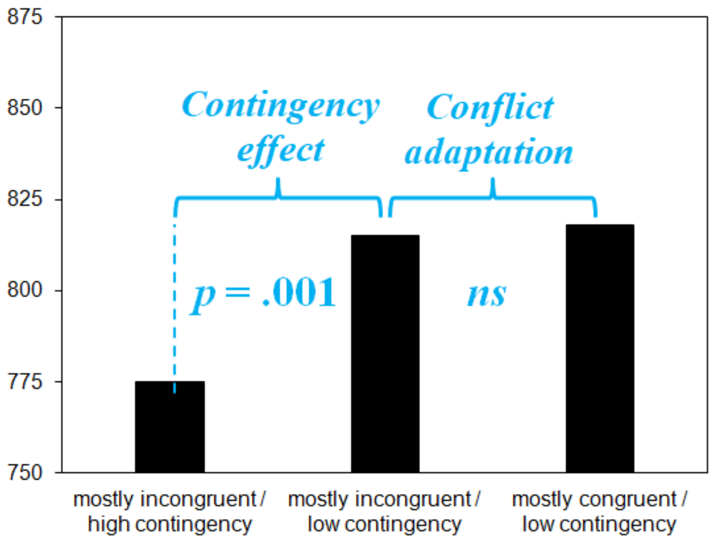

Figure 3.4. Mean response times as a function of condition in Schmidt (2013a). There are mostly incongruent trials with high (left) and low (middle) contingencies and mostly congruent trials with low word-colour contingencies (right). Only evidence for a contingency effect is observed.

Stimulus informativeness

In Schmidt (2014a), I presented the argument that it is important to match stimulus informativeness when studying the item-specific proportion congruent effect. While some studies, as described above, found exclusive evidence for contingency learning biases and no evidence for conflict monitoring in the ISPC, others have presented evidence seemingly inconsistent with this. Most critically, Bugg and Hutchison (2013) argued that contingencies do dominate processing when words are strongly predictive of colours. However, if contingencies are weakened, conflict monitoring will take over as a "last resort" (see also, Bugg, 2014b; Bugg, Jacoby, & Chanani, 2011; Chiu, Jiang, & Egner, 2017). To achieve this, mostly incongruent words were presented equally often in many incongruent colours (e.g., "green" 25% of the time in each of the colours red, blue, and orange) rather than frequently in one incongruent colour (e.g., "green" 75% of the time in red). Mostly congruent words (rather by necessity) were nevertheless still presented frequently in one colour (i.e., the congruent colour). With this manipulation, results were seemingly consistent with a conflict monitoring account (mostly interference-driven effects) and inconsistent with a (simple) contingency learning account.

Comparing a contingent mostly congruent condition to a non-contingent mostly incongruent condition might produce results that seem harder to interpret from a contingency learning perspective, but this might also be like comparing apples to oranges. It is known that a contingency-laden dimension must be attended in order to learn the correlation (e.g., Jiang & Chun, 2001). More importantly, it is also known that when a contingency is detected for a given distracting stimulus, attention is attracted to this stimulus (e.g., Chun & Jian, 1998; Cosman & Vecera, 2014). The notion that contingent stimuli attract attention only stands to reason: predictive stimuli in our environment are attended because they can help guide our behaviour (see also, Hutcheon & Spieler, 2014). Thus, correlated mostly congruent distracting words should attract more attention than uncorrelated mostly incongruent words. Because of this, we should anticipate more attention to mostly congruent words than to mostly incongruent words in designs such as those from Bugg and Hutchison (2013), even without conflict monitoring. This account, of course, shares in common with the conflict monitoring account the notion that attention is better focused on the target in the mostly incongruent condition. However, the proposed reason why such an attentional difference exists is different.

Temporal learning confound and LLPC

In other variants of the PC procedure, researchers have been interested in the PC of the task as a whole, rather than the effect for individual items (Cheesman & Merikle, 1986; M. O. Glaser & Glaser, 1982; Kane & Engle, 2003; Lindsay & Jacoby, 1994; Shor, 1975; West & Baylis, 1998). This I will refer to as the *list-level PC (LLPC) effect*. Typically, LLPC is assessed by manipulating the PC of the list (e.g., averaged across all items for one group of participants or block of trials) with some *biased* (or *inducer*) *items*. For instance, "blue" and "red" might be mostly congruent (e.g., "blue" most often in blue) in one condition and mostly incongruent (e.g., "blue" most often in red) in another condition. Intermixed with these biased items are some other *transfer* (or *diagnostic*) *items* that are non-manipulated. For instance, "green" and "brown" might be presented equally often in green and brown for all participants (i.e., the same congruent:incongruent ratio in both PC conditions). It is the PC effect for these transfer items that we term the LLPC effect.

Notably, a LLPC effect cannot be explained by contingency learning (as the transfer items are contingency-unbiased), but could, in principle, be explained by transfer of control from the manipulated items to the transfer items. That is, because conflict is overall more frequent in the mostly incongruent condition, attention is minimized to all distracting words, including those that were not directly manipulated. Some of the first, most straightforward manipulations of LLPC produced no effect (Blais & Bunge, 2010; Bugg, Jacoby, & Toth, 2008). That is, the congruency effect for transfer items was exactly the same in the mostly congruent and mostly incongruent lists. However, later reports have observed effects in a variety of tasks (e.g., Stroop, Simon, picture-word, prime-probe; Bugg, 2014a; Bugg & Chanani, 2011; Bugg, McDaniel, Scullin, & Braver, 2011; Gonthier, Braver, & Bugg, 2016;

Hutchison, 2011; Schmidt, 2017), including across tasks (Funes, Lupiáñez, & Humphreys, 2010; Torres-Quesada, Funes, & Lupiáñez, 2013; Wühr, Duthoo, & Notebaert, 2015).

However, there remains another account of LLPC effects. Schmidt (2013c) first presented the notion that the LLPC effect might be due, wholly or in part, to *temporal learning* biases (for a related idea in masked priming, see Kinoshita, Mozer, & Forster, 2011). The idea is not necessarily easy to grasp if one is used to thinking about the content of the items we manipulate (e.g., congruent vs. incongruent, high vs. low frequency, etc.). However, many times more systematic variance in response times is explained by how we time our responses than the factors themselves (see Grosjean, Rosenbaum, & Elsinger, 2001). In particular, we are highly biased to time our responses in a rhythmic way. That is, my response time (RT) on the current trial will tend to be highly similar to my RTs on very recent trials. This systematic variability in response times can be observed in *pink noise* (also referred to as 1/*f* or flicker noise): ignoring some random "white" noise and systematic effects of factors, current RT is likely to be increasingly less similar to a given prior RT the further back in time it occurred. These autocorrelations in RTs are omnipresent in a broad range of cognitive paradigms, including mental rotation, lexical decision, visual search, and speeded classification (Gilden, 1997, 2001). It has been repeatedly observed in a number of domains that timing biases produce interactive effects between study factors that have relatively little to do with the factor manipulations themselves (Kinoshita, Forster, & Mozer, 2008; Kinoshita & Lupker, 2003; Kinoshita et al., 2011; Lupker, Brown, & Colombo, 1997; Mozer, Kinoshita, & Davis, 2004; Schmidt, 2014c, 2016c). Indeed, Kiger and Glass (1981; see also, Kinoshita et al., 2011) stress that such decision-related (rather than content-related) effects "will continue to be rediscovered in many circumstances... and will be mistakenly attributed to a multiplicity of causes" (p. 697).

Rhythmic timing biases can produce a LLPC effect because such biases can affect congruent and incongruent trial types differentially in conditions with a faster versus slower task pace. Naturally, the task pace in a mostly congruent list will be much faster than in a mostly incongruent list (i.e., more fast congruent trials in the former). Schmidt (2013c) argued that timing biases will benefit response speed selectively for trials in which participants have sufficient evidence to select a response at the expected time. A simplified illustration is presented in Figure 3.5. In particular, the threshold for selecting a response is decreased (i.e., the trigger to respond is loosened) at the expected time, allowing for faster responses if the task pace can be maintained (i.e., if there is sufficient evidence to cross the temporarily-decreased threshold). When the task pace is fast (e.g., mostly congruent), congruent trials will tend to benefit from temporal expectancies. That is, participants will have enough evidence to select a response at the expected time and maintain their task pace. For the occasional incongruent trial, however, there will typically not be enough evidence for a response at the expected time (e.g., due to ongoing resolution of conflict), and responding will therefore be delayed. The net effect is an inflated congruency effect. In the mostly incongruent condition, the situation is largely reversed. The task pace is slower and an early response is therefore not expected. Expectancy for a later response might therefore benefit incongruent trials. The occasional congruent trials, however, do not benefit in the same way as in the mostly congruent condition. The net effect is a smaller congruency effect.

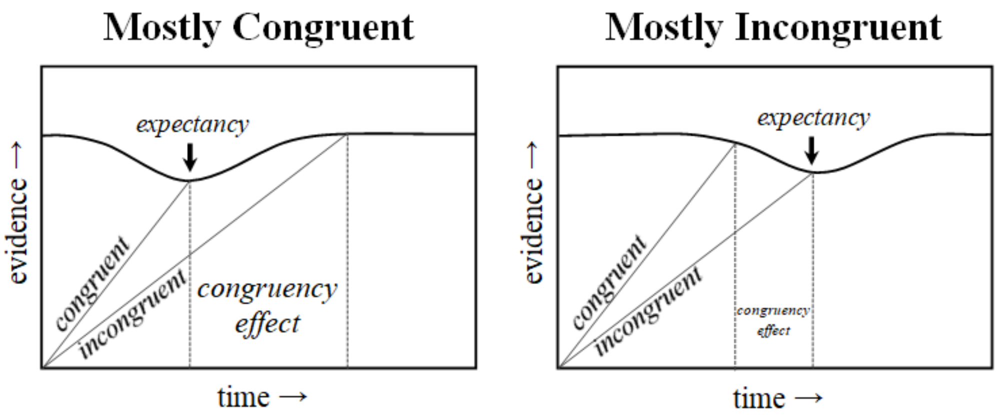

Figure 3.5. Simplified illustration of how temporal expectancies can produce a LLPC effect. *Notes:* dotted line = response threshold, slopes indicate rate of evidence accumulation for the correct response.

To test this notion, previous trial RT was used as a rough proxy for "pace," with the prediction that the congruency effect should be overall larger the faster the previous RT (Schmidt, 2013c). In a *linear mixed effects (LME)* regression on LLPC data from Hutchison (2011), this exact finding was observed. That is, the faster the previous trial RT, the larger the congruency effect on the current trial. Additionally, accounting for this timing bias reduced the LLPC effect. Although a suboptimal measure of rhythmic timing on its own, these results lend credence to the notion that temporal learning, at minimum, is likely to contribute to the LLPC effect. Subsequent work (described below) adds further support to this notion.

Non-conflict temporal learning effects

In addition to the LME data discussed above and some neural network modelling data (see Chapter 4), I also tested the notion that a LLPC like interaction should be observed even without a manipulation of conflict. For instance, in Schmidt (2013c) I used a simple letter identification task. On each trial, participants saw only a letter (D, F, J, or K) and were simply required to press the corresponding key on the keyboard. Unlike a conflict task (e.g., Stroop), there were no distracting stimuli and thus no conflict. The only manipulations were the contrast of the target digit on a given trial (high vs. low) and the proportion of high versus low contrast trials (mostly easy vs. mostly hard). Of course, participants respond faster to high contrast (easy to see) targets than to low contrast (slightly harder to see) targets, but this contrast effect was also moderated by *proportion easy*. Just like a LLPC effect, the contrast effect was larger in the mostly easy context relative to the mostly hard context. This is exactly what one would expect on the basis of the temporal learning account: conflict is not relevant, only the pace, and the pace is faster in the mostly easy condition. The conflict monitoring account, of course, cannot explain this finding: there is no conflict to monitor or adjust to. Schmidt (2014c) further confirmed that this proportion easy effect is not specific to items by using the same sort of biased/transfer item design as described earlier for the LLPC procedure. The data from Experiment 1 are presented in Figure 3.6. What these results illustrate is a relatively pure example of why we should expect a PC-like interaction in a LLPC procedure even without conflict monitoring.

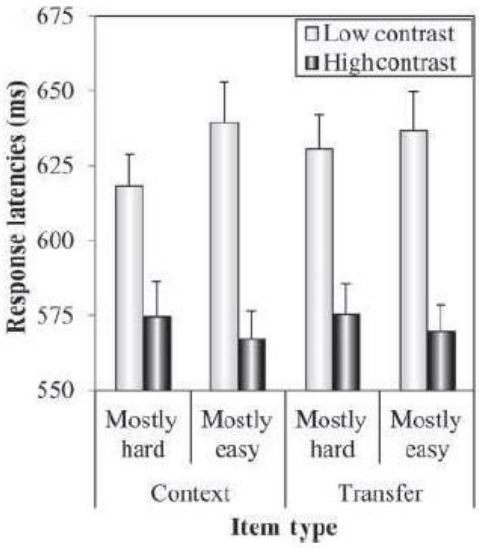

Figure 3.6. Experiment 1 response latencies in milliseconds from Schmidt (2014c) for contrast, proportion easy, and item type, with standard error bars.

Further dissociation work

In a recent series of experiments, I aimed to more clearly adjudicate between a pure temporal learning view and conflict monitoring. In this even more compelling approach, I aimed to eliminate temporal learning biases in the LLPC effect by directly manipulating task pace (Schmidt, 2017). In particular, prime-probe conflict tasks with direction word distracters and targets were used in place of colour-word Stroop. Distracting location words (e.g., “left”) are presented as primes to target probe words (e.g., “right”), which can be congruent or incongruent (essentially word-word direction Stroop). The typical LLPC design was used. That is, some biased words (e.g., “up” and “down”) were manipulated for PC and some intermixed transfer items (e.g., “left” and “right”) were not manipulated, as illustrated in Figure 3.7. In a control condition, this produced a robust LLPC effect. In the critical “long wait” condition, however, task pace was manipulated by presenting “wait cues” on some of the biased item trials. Participants had to wait for a brief amount of time (until the cue disappeared) before making a response. This, at least roughly, served to match response speed and accuracy in the mostly congruent and mostly incongruent conditions. This eliminated the LLPC effect, as shown in Figure 3.8. Note that in the control (short wait) condition, the same wait cues were presented but more briefly. These experiments provided a clear dissociation between the pure temporal learning and control views. According to the temporal learning account, only the pace of responding matters. Thus, the LLPC effect should be eliminated. According to the conflict monitoring view, however, conflict matters. The long wait manipulation preserved the conflict proportions, so a LLPC effect still should have been observed.

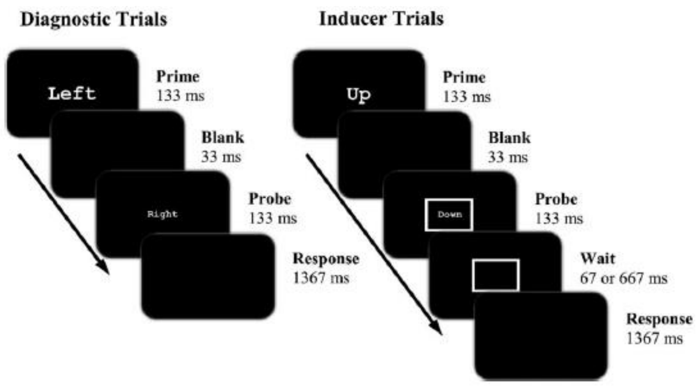

Figure 3.7. Experiment 1 trial procedure from Schmidt (2017) for diagnostic (and biased) items and inducer items.

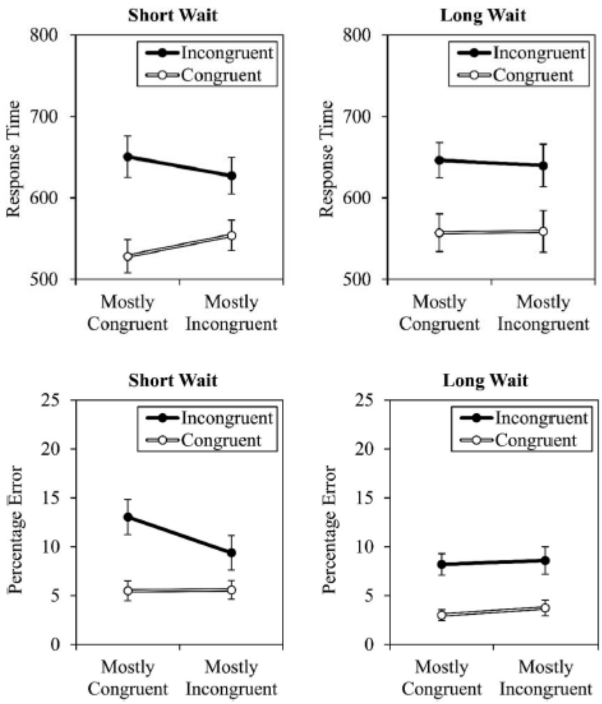

Figure 3.8. Experiment 1 response times (top) and percentage errors (bottom) from Schmidt (2017) for short and long wait diagnostic items.

Context-specific proportion congruency

Another line of evidence for conflict monitoring comes from *context-specific proportion congruent* (*CSPC*) *effects* (Bugg et al., 2008; Corballis & Gratton, 2003; Crump, Gong, & Milliken, 2006; Crump, Vaquero, & Milliken, 2008; Heinemann, Kunde, & Kiesel, 2009; Lehle & Hubner, 2008; Wendt & Kiesel, 2011). A CSPC procedure typically involves two contexts, such as two stimulus display locations (e.g., above or below fixation) or fonts. The same (randomly intermixed) stimuli are mostly congruent in one context (e.g., above fixation) and most incongruent in the other context (e.g., below fixation). The CSPC effect is the observation that the congruency effect is smaller in the latter context relative to the former. One thing that is particularly interesting about CSPC effects is that mostly congruent and mostly incongruent stimuli are randomly intermixed. Thus, at the start of the trial, the participant has no knowledge of whether the upcoming stimulus will be mostly congruent or mostly incongruent. Thus, if attention is really being controlled, then the control signal cannot, by definition, be triggered until the stimulus context (e.g., location) has already been observed. Given that the target stimulus is presented concurrently with the context, this means that there is zero advanced preparation time to adjust attention. It has nevertheless been proposed that attentional control is quickly engaged from stimulus onset, with an upregulation of attentional control for the mostly incongruent context and a downregulation for the mostly congruent context.

An alternative view is that CSPC effects, in whole or in part, are due to contingency learning (Schmidt & Lemercier, 2019), just like with the ISPC effect discussed above. An example CSPC design is illustrated in Table 3.3. What will be noted is that, task-wide, words are only moderately predictive of the congruent colour. Also, as each word is presented in both the mostly congruent and mostly incongruent contexts, the word-colour contingencies alone cannot explain CSPC effects. However, if we make the reasonable assumption that participants can combine location and word information together to anticipate the likely response (e.g., see Mordkoff & Halterman, 2008; see also, Holland, 1992, for a background on occasion setting), then the word + location is, in fact, strongly predictive of the congruent response in the mostly congruent condition (e.g., "green" + up indicates a likely green response), and unpredictive in the mostly incongruent condition (e.g., "green" + down is uninformative about the likely colour response). Thus, *compound-stimulus contingency learning* can potentially explain the CSPC effect.

Table 3.3. Example context-specific proportion congruent manipulation.

	Up				Down			
Colour	brown	blue	green	red	brown	blue	green	red
brown	9	1	1	1	3	3	3	3
blue	1	9	1	1	3	3	3	3
green	1	1	9	1	3	3	3	3
red	1	1	1	9	3	3	3	3

In our studies, we made use of the font version of the CSPC paradigm. This is identical to the location-based CSPC design described above, except that the font in which coloured colour words were presented served as the contextual cue (Bugg et al., 2008). In order to dissociate between contingency and attentional control biases, we used a slightly modified stimulus matrix, illustrated in Table 3.4. As you will notice, two words are mostly congruent (MC) in one font and mostly incongruent (MI) in the other font. For the remaining two words, this was reversed. Most importantly, high contingency (HC) and low contingency (LC) trials are not, however, completely confounded with proportion congruency in this novel design, at least for incongruent items.

Table 3.4. Experiment 1 contingency manipulation from Schmidt and Lemercier (2019).

	italic Georgia				roman Arial			
Colour	brown	blue	green	red	brown	blue	green	red
brown	9	1	1	1	1	9	1	1
blue	1	9	1	1	9	1	1	1
green	1	1	1	9	1	1	9	1
red	1	1	9	1	1	1	1	9

Notes: light grey = HC/MI, mid grey = LC/MI, dark grey = LC/MC, white = congruent

Most critical to this design is that it produces three types of incongruent trials. First, there are *high contingency, mostly incongruent (HC/MI)* trials (e.g., "brown" in blue in Arial font; light grey in Table 3.4), which have a strong contingency bias toward the correct response. Next, there are *low contingency, mostly incongruent (LC/MI)* trials (e.g., "red" in blue in Georgia font; mid grey in Table 3.4), which are also mostly incongruent, but low contingency. Thus, a difference between HC/MI and LC/MI trials cannot indicate conflict adaptation (as the words are equally mostly incongruent), and must therefore indicate a contingency learning effect (i.e., high contingency < low contingency). Finally, there are *low contingency, mostly congruent (LC/MC)* trials (e.g., “green” in blue in Arial font; dark grey in Table 3.4). Like the LC/MI trials, these are also low contingency, but are mostly congruent. As such, a difference in performance between LC/MI and LC/MC conditions cannot indicate a contingency learning bias, but could indicate an attentional control effect (mostly incongruent < mostly congruent). As can be observed in Figure 3.9, a robust contingency effect was observed in both response times and errors. The attentional control contrast was, if anything, in the “wrong direction.” Thus, there was strong evidence against for the contingency learning view, in addition to strong evidence against the conflict monitoring view (with Bayesian support for a true null).

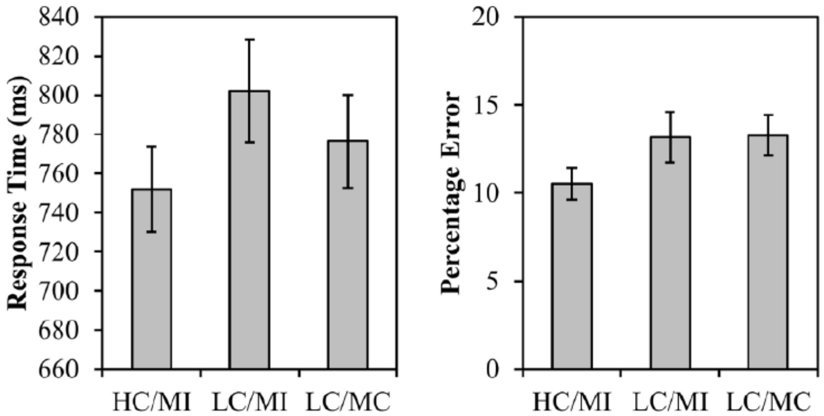

Figure 3.9. Experiment 1 dissociation analysis from Schmidt and Lemercier (2019) for response times (left) and percentage errors (right), including standard error bars. Only a contingency effect is observed.

It is also important to point out that the design of Experiment 1 departed in an important (and interesting) way from typical CSPC procedures. In particular, each font context was *not* consistently associated to one level of PC. For instance, Georgia font was mostly congruent for “brown” and “blue,” but mostly incongruent for “green” and “red,” in the Table 3.4 example. According to the compound-stimulus contingency learning view this design feature is irrelevant, as participants only learn word-font-colour correspondences. According to the attentional control view, however, it might be proposed that no CSPC effect should be observed at all if learning about conflict is fully specific to the font (i.e., both fonts have the same number of congruent and incongruent trials, averaged across the four words). However, a second experiment with a more “traditional” CSPC setup (i.e., one context consistently mostly congruent and the other consistently mostly incongruent) produced exactly the same results. Thus, our results were not only inconsistent with the conflict monitoring view of CSPC effects, but were also inconsistent with the idea that learning in the procedure is context-specific. Instead, learning seems to be related to word-colour-font compounds.

Context-specific temporal learning

In other work (Schmidt, Lemercier, & De Houwer, 2014), we explored the possibility that at least some CSPC effects may be due to context-specific temporal learning. As described above regarding the temporal learning account of LLPC effects, congruency effects could be larger in a mostly congruent list relative to a mostly incongruent list simply because of the differing task paces in the two conditions. The same might be true of task contexts, whereby participants learn to time their responses differently in one context than another. In that vein, we used the proportion easy manipulation described earlier (i.e., with high vs. low contrast letters), but manipulated “proportion easy” for two context locations. We observed larger stimulus contrast effects in the mostly easy location (e.g., up) relative to the mostly hard location (e.g., down). Context-specific contingency learning could, in principle, not only account for CSPC effects for manipulated (contingency-biased) items, but also for non-manipulated transfer items (e.g., Crump, Brosowsky, & Milliken, 2017; Crump & Milliken, 2009; but for failures to replicate transfer, see Hutcheon & Spieler, 2017). It is noteworthy, however, that the failure to observe CSPC effects in Schmidt and Lemercier (2019) after controlling for contingency biases is just as inconsistent with a temporal learning account as with an attentional control account. I have also presented neural network demonstrations of how contingency and temporal learning biases can produce CSPC effects (see Chapter 4).

Binding and other confounds in the CSE

In Schmidt and De Houwer (2011), we explored binding and contingency learning biases in the CSE. The assumption behind the CSE as a measure of attentional control is that the effect emerges because participants adjust attentional control in response to conflict during response selection. Following an incongruent trial, control is high and the congruency effect is resultantly reduced. Following a congruent trial, control is lower and the word resultantly interferes more. However, there have been a number of confounds identified in this domain that bring into question the attentional control (or conflict monitoring) interpretation of the effect. One such bias comes from binding confounds (Hommel, Proctor, & Vu, 2004; Mayr, Awh, & Laurey, 2003). In particular, the series of stimulus repetition types systematically confounds the CSE. For example, it is only possible to have a "complete repetition" (i.e., same word and same colour) on a congruent trial following a congruent trial or on an incongruent trial following an incongruent trial. A complete repetition (which is responded to very fast) is never possible when congruency changes. We further identified sequential contingency confounds (for followup work, see Mordkoff, 2012), as congruent trials (on the current and/or previous trial) are always high contingency if words are presented most often in their congruent colour. In Stroop and flanker experiments with no contingency bias, we presented a "binding decomposition" of the CSE. As can be observed in Table 3.5, within each of the four cells of the CSE interaction, there are different types of feature repetitions possible. When restricting the analyses to the "complete alternations" (where no features repeat), there is no remaining CSE. Similar results were observed with the flanker task (i.e., target letter, instead of colour, with distracting flanking letters, instead of words) in a second experiment.

Table 3.5. Trial type examples with response times and errors from Schmidt and De Houwer (2011).

Trial Type	Repetition type W–W	C–C	W–C	C–W	RT	Error rate
Congruent–congruent						
(1) BLUE$_{blue}$→RED$_{red}$					***702 ms***	***8.3%***
(2) BLUE$_{blue}$→BLUE$_{blue}$	x	x	x	x	494 ms	3.8%
Congruent–incongruent						
(3) BLUE$_{blue}$→RED$_{green}$					***787 ms***	***16.7%***
(4) BLUE$_{blue}$→BLUE$_{red}$	x			x	749 ms	16.0%
(5) BLUE$_{blue}$→RED$_{blue}$		x	x		570 ms	10.7%
Incongruent–congruent						
(6) RED$_{blue}$→GREEN$_{green}$					***702 ms***	***10.2%***
(7) RED$_{blue}$→RED$_{red}$	x		x		696 ms	11.9%
(8) RED$_{blue}$→BLUE$_{blue}$		x		x	559 ms	4.4%
Incongruent–incongruent						
(9) RED$_{blue}$→GREEN$_{yellow}$					***785 ms***	***14.2%***
(10) RED$_{blue}$→RED$_{green}$	x				754 ms	13.5%
(11) RED$_{blue}$→GREEN$_{blue}$		x			571 ms	8.6%
(12) RED$_{blue}$→RED$_{blue}$	x	x			520 ms	5.2%
(13) RED$_{blue}$→GREEN$_{red}$			x		773 ms	14.5%
(14) RED$_{blue}$→BLUE$_{green}$				x	755 ms	12.1%
(15) RED$_{blue}$→BLUE$_{red}$			x	x	759 ms	12.7%

Notes: Word indicated in all caps, colour in subscript. W–W=word–word; C–C=colour–colour; W–C=word–colour; C–W=colour–word. The conditions in bold and italics do not contain repetitions.

Modelling binding biases with factor nesting

One downside with assessing the CSE by only analysing complete alternation trials is that one must delete a large portion of the observations. One alternative method to “removing” binding biases from CSE designs was suggested by Notebaert and Verguts (2007). This involved coding for some of the binding confounds as variables that are inserted in a regression along with previous and current trial congruency. That is, we test for the main effects of congruency and previous trial congruency along with their interaction (CSE) while also controlling for various binding types (e.g., word repetitions) occurring on individual trials. The hope, then, is that the binding confounds are “regressed out” of the data and the CSE factor will only code for the “true” attentional control effect.

Unfortunately, this approach does not work well. In Schmidt, De Schryver, and Weissman (2014) we showed that this regression approach deals inadequately with a factor “nesting” problem. In short, one cannot account for all types of bindings (e.g., word-word repetitions, colour-colour repetitions, word-colour repetitions, and colour-word repetitions) and all of their interactions (and orthogonal to congruency and previous trial congruency), because there are far too many missing cells. For example, if the current trial is congruent and the current colour (e.g., blue) is the same as the previous trial word (i.e., “blue”), then it is impossible that the word has not repeated. This means that we simply cannot code for all potential binding influences on the data (as the number of factors would greatly exceed the number of data points). The variance in the CSE that we do not model (omitted factors) can be “stolen” by the CSE interaction, giving the illusion that a CSE exists independent of binding effects even if this is untrue. This is termed *omitted variable bias*. We demonstrated that this bias does exist and suggested that the only appropriate way to assess the CSE is to discard feature repetitions and restrict the analysis to complete alternation trials.

Further CSE concerns

In one paper, Blais, Stefanidi, and Brewer (2014) contested the notion the contingency and/or congruency switch biases influence the CSE (as discussed in Schmidt & De Houwer, 2011). Eschewing discussion of congruency switch biases, the notion of a contingency bias in the CSE is that we should expect that the CSE interaction should emerge when distracting stimuli are presented most often in the congruent colour. This is because congruency (and previous trial congruency) becomes conflated with contingency and contingency effects are larger following a predictive, high contingency stimulus (Hazeltine & Mordkoff, 2014; Schmidt et al., 2007). Blais and colleagues argued against such a contingency bias based on the finding that the CSE did not (significantly) increase with a higher proportion of congruent trials (and therefore higher contingency). In Schmidt (2014b), however, I demonstrated that their data was severely underpowered and that the observed trend in their data was very strongly consistent with the learning view of the CSE, as illustrated in Figure 3.10. In particular, the observed CSE did increase notably as the PC (and thus contingency) increased (solid line), but the sample was so small that this did not come out as significant. Their study only had enough power to detect an influence of contingency on the CSE if said effect was enormous in magnitude (dashed line). Strangely, the same slope was significantly positive in their error data, but seemingly ignored.

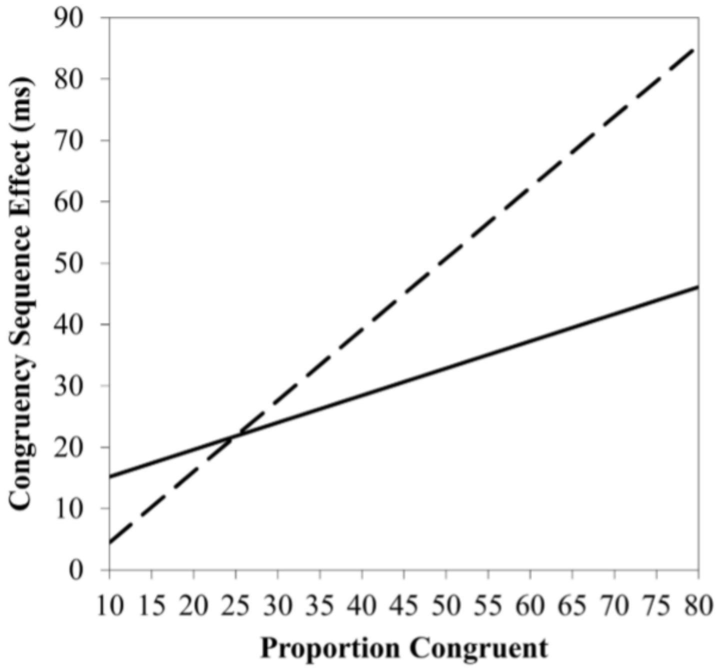

Figure 3.10. Congruence sequence effect as a function of proportion congruency in Blais, and colleagues (2014) as reanalyzed by Schmidt (2014b), with observed trend line (solid line) and trend line that would have been required for a high power test given the sample size and error (dashed line).

Improved CSE design

In another report, we introduced a method for eliminating the above-mentioned contingency and binding biases by design (Schmidt & Weissman, 2014). In particular, instead of running an experiment and then deleting the majority of observations to assess only complete alternations, one can devise an experiment in which feature repetitions never occur to start out with. We achieved this by alternating (i.e., on odd and even trials) between two stimulus sets. For example, left- and right-pointing arrows can be presented on odd trials and up- and down-pointing arrows can be presented on even trials. Thus, congruency can repeat or change from one trial to the next (as usual), but it is impossible that any of the stimuli repeat from one trial to the next. Additionally, we can have an equal number of congruent and incongruent trials without inducing a contingency bias (i.e., because we alternate between two two-choice tasks). In these studies, we observed robust CSEs for both an arrow version of prime-probe (i.e., with distracting and target arrows) in addition to a direction word version of the same paradigm (as described for the LLPC earlier).

Interestingly, however, we also observed in a follow-up report that the presence of these highly robust CSEs depends in a major way on the similarity between targets and distracters (Schmidt & Weissman, 2015). In a first experiment, some participants were presented either with arrows as distracters and direction words as targets or the reverse (different format). Yet other participants were presented with either direction words or with arrows as both targets and distracters (similar format). The results are presented in Figure 3.11. As can be observed, congruency effects are observed in both cases, but the CSE is only observed with similar format distracters.

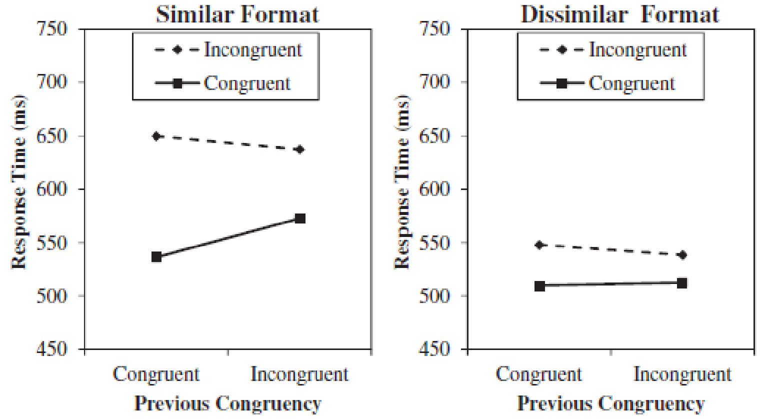

Figure 3.11. Mean RT in each of the main conditions of Experiment 1 in Schmidt and Weissman (2015).

In a second experiment, we explored the reason for this format effect. It could, on the one hand, be that the mechanism responsible for the CSE (whatever that might be) is dependent on attentional capture of the distracting stimulus. When the distracter is the same perceptual format as the targets, it captures attention (i.e., because it looks like a potential target). On the other hand, it could simply be the case that the CSE is driven by perceptual conflict. With same format stimuli, the distracter and target either perceptually match (e.g., two left pointing arrows) or mismatch (e.g., one left and one right pointing arrow). With different format stimuli, the distracter and target always mismatch visually (e.g., a left-pointing arrow and the word "left" mismatch visually, even though congruent in meaning). To test these two notions, we randomly varied the perceptual format (word, arrow) of both the distracter and the target on a trial-by-trial basis. Since the target in each trial could be either an arrow or a word, participants had to adopt an attentional set for both perceptual formats. According to the *attentional capture* account, distracters in both perceptual formats should capture attention, because both formats are goal-relevant. This account therefore predicts equivalent CSEs on both same and different perceptual format trials. According to the *perceptual conflict* account, the degree to which incongruent stimuli engender greater perceptual conflict than congruent stimuli should always be greater when those stimuli are presented in the same as compared to different perceptual formats. This account therefore predicts a larger congruency effect on same perceptual format trials than on the different perceptual format trials. It therefore also predicts larger CSEs following trials in which the distracter and target appear in the same as compared to different perceptual formats. The results are presented in Figure 3.12. As can be observed, the attentional capture account was supported, with equivalent CSEs in both conditions. As observed in Figure 3.13, this was also true when considering the perceptual format of the *preceding* trial.

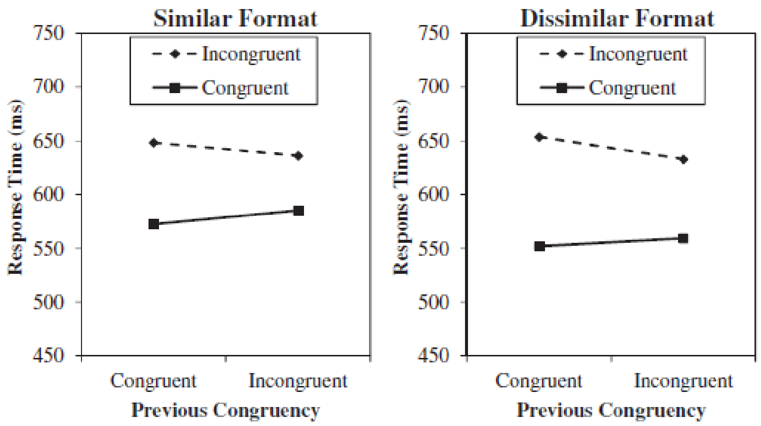

Figure 3.12. Mean RT in each of the main conditions of Experiment 2 in Schmidt and Weissman (2015).

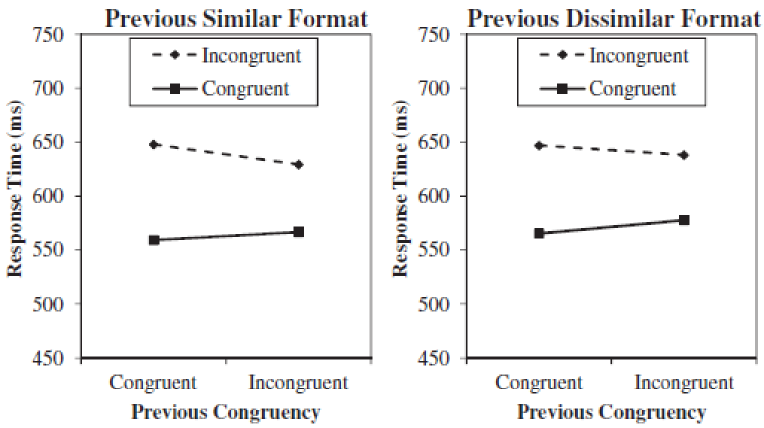

Figure 3.13. Mean RT in each of the main conditions of Experiment 2 in Schmidt and Weissman (2015) as a function of previous trial perceptual format.

Interestingly, our new procedure does allow one to observe a CSE even when binding/learning confounds are excluded. However, subsequent work (by others) does not support well the idea that this CSE is due to conflict monitoring. For instance, the size of the CSE is largely unrelated to the size of the congruency effect, which it should be if amount of conflict determines the CSE, and the congruency effect can even reverse after incongruent trials with some manipulations, which should never be the case according to the attentional control view (Weissman, Colter, Drake, & Morgan, 2015; Weissman, Egner, Hawks, & Link, 2015; Weissman, Hawks, & Egner, 2016; Weissman, Jiang, & Egner, 2014).

Works in Progress and Future Directions

Temporal learning defense

In Schmidt (2019), I respond to a recent critique of the temporal learning account from Cohen-Shikora, Suh, and Bugg (in press). As mentioned above, one of the many lines of converging evidence for a temporal learning bias in the LLPC effect comes from statistical modelling. In particular, in addition to the standard autocorrelation in response times, it was observed that (a) faster previous-trial response times are associated with larger congruency effects on the current trial, and (b) controlling for previous trial influences decreases the LLPC effect. The LME analyses that I originally reported followed a standard procedure (copied exactly from Kinoshita et al., 2011) for analysing individual trial response times. Because response times are distributed in a heavily skewed (ex-Gaussian) fashion, performing an LME on raw response times will violate the distributional assumptions of the test. A typical procedure is therefore to transform raw response times, usually with an inverse transform (such as −1000/RT), or with a log or Gamma transform (e.g., Andrews & Lo, 2012; Kinoshita et al., 2011; Kliegl, Masson, & Richter, 2009; Masson & Kliegl, 2013).

Cohen-Shikora and colleagues (in press) took issue with the transformation of raw response times. There are scenarios in which transforming data might be undesirable (Stevens, 1946), such as when assessing additivity of two main effects (Balota, Aschenbrenner, & Yap, 2013; Lo & Andrews, 2015). They therefore used a generalized linear mixed model (GLMM) on raw response times and modeled the skew (with a Gamma distribution and identity link function) and reproduced the LME and GLMM analyses on three datasets. Although the exact same findings were observed in all datasets with LME (i.e., consistent with the temporal learning account), results with GLMM were inconsistent. Combined with some additional analyses, the authors argued that a temporal learning contribution to the LLPC effect is not well supported.

In my response article, I highlight several important problems with the claims of Cohen-Shikora and colleagues (in press). For instance, I point out that the concerns that can exist with data transformations are not applicable to crossover-type interactions (Loftus, 1978), as in the case of the LLPC effect, or a simple main effect (Kliegl et al., 2009). I also demonstrate that the relationship between previous-trial response times and current-trial response times is better described in an inverse (not raw) scale. With this, I provide a clear demonstration of why analyses on the inverse scale (with LME) and raw scale (with GLMM) seem to provide incompatible results. Indeed, if a true temporal learning effect does not exist, then it should not appear in the inverse or raw scales, so the robust effects in the inverse scale remain inconsistent with the null temporal learning view. I further demonstrate that a temporal learning bias *is* observed in the raw scale if previous-trial response times are allowed to predict current-trial response times in a non-linear (inverse) scale. In short, the paper demonstrates that the challenge to the temporal learning account was unwarranted.

Consensus paper

I am also a coauthor on a paper that outlines a general consensus on the more optimal ways to assess adaptive control while eliminating (or at least reducing) other types of biases (Braem et al., 2019). In this paper, we consider approaches to studying adaptive control in LLPC, ISPC, CSPC, and CSE procedures. The main aim of the paper is to highlight the various limitations of approaches in these paradigms and the best ways of dealing with confounds. More concretely, we propose that control is best assessed by manipulating some items and testing for transfer to unbiased (non-manipulated items). The global goal is to provide readers with a "user guide" for attentional control paradigms, which is useful for those wanting to study adaptive control but who are less intimately familiar with the intricacies of the work in the domain.

This work is particularly important given how extensively “attentional control” procedures have been applied to a variety of clinical, development, and individual differences populations (E. Abrahamse et al., 2017; Bugg, 2014b; Hutchison, 2011; Iani, Stella, & Rubichi, 2014; Lansbergen, Kenemans, & van Engeland, 2007; Lansbergen, van Hell, & Kenemans, 2007; Liu, Gehring, Weissman, Taylor, & Fitzgerald, 2012; Praamstra & Plat, 2001; Steudte-Schmiedgen et al., 2014; Tulek, Atalay, Kanat, & Suerdem, 2013). The standard logic has been that if a PC effect or CSE is found to be diminished in a given population (e.g., Parkinson’s patients), then this indicates an impairment in attentional control. Unfortunately, however, proponents from both sides of the control vs. learning debate strongly agree that the wrong versions of these paradigms have been systematically used. That is, more applied researchers have consistently used versions of these paradigms that are known to be heavily confounded by simple learning biases, rather than using more “confound-minimized” designs. Similar concerns exist in neuroscience work with these paradigms (Blais & Bunge, 2010; Botvinick et al., 1999; Kerns et al., 2004; MacDonald, Cohen, Stenger, & Carter, 2000; Sheth et al., 2012). Further consensus-based experimental research is currently being planned with the same author group.

Chapter 4 – Neural Networks

Published Work

Parallel Episodic Processing (PEP) model

I have also conducted neural network research, primarily with exemplar based models of memory. In particular, I programmed the *Parallel Episodic Processing (PEP)* model (Schmidt, 2013a, 2013c, 2016a, 2016b, 2018; Schmidt et al., 2017; Schmidt et al., 2016; Schmidt & Weissman, 2016). In this and related exemplar (also known as episodic or instance) models (Hintzman, 1984, 1986, 1988; Logan, 1988; Medin & Schaffer, 1978; Nosofsky, 1988a, 1988b), each experienced event (e.g., trial in an experiment) is stored as a new memory trace. That is, on each trial, the stimulus (or stimuli) that were experienced and the response that was selected are coded into a new memory trace. For instance, if a participant sees a blue stimulus on 50 trials and presses the J key each time (e.g., in accordance with instructions), then there will be 50 memory traces linking blue to the J key. When a stimulus is presented, retrieval of similar memory traces will occur. For example, if blue is presented again, then memories of seeing a blue stimulus will be retrieved, which will in turn reactivate the responses that were made on these past trials. Using these simple memory storage and retrieval processes, we have been showing over a series of papers how a broad range of findings from various sub-literatures can be accounted for. As much of what has been simulated in the PEP model has been discussed in the prior two chapters, some redundancy is avoided in this chapter by assuming that the reader is either familiar with the research domain in question or has read Chapters 2 and 3.

A visual depiction of the PEP model is presented in Figure 4.1, as applied to a simple Stroop task. There are input nodes for each stimulus that can be "presented" to the model, identity (or decision) nodes that represent the internal classification of the stimulus, and response nodes for each potential response a participant can make (in this example, key presses). More critical is the episodic store. After each trial, a new episode is created that links to the stimuli and responses that were active on the trial. In later versions of the model, also decisions and goals are encoded in memory traces. On subsequent trials, memory traces are "retrieved" when they become active via activation sent from input or other nodes. For instance, if the red input node becomes active, then it will send activation to episodes that coded for red. This, in turn, leads to retrieval activation for the other nodes encoded in the memory traces (e.g., activation of linked responses). Although further details of the model will be discussed below, the key concept is that events are encoded into memory traces, and that these traces can be retrieved to influence responding. The localist event encoding (i.e., one trace per event) should be regarded as conceptual only, and need not be viewed as implying that the underlying "code" in neurons and synapses in the brain is so locally organised.

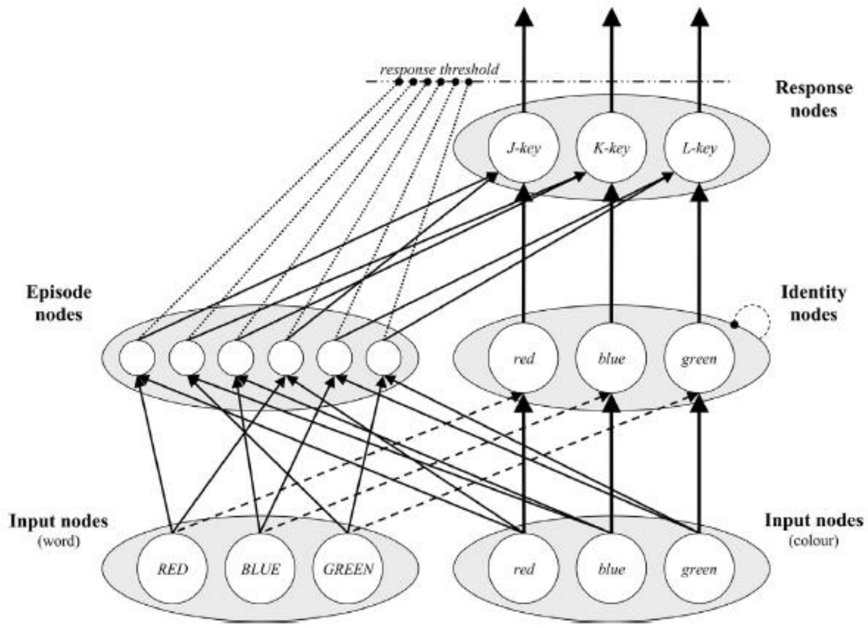

Figure 4.1. A representation of the Parallel Episodic Processing (PEP) model. Input nodes feed activation through Identity nodes and Response nodes in an algorithmic route. Learning occurs via Episode nodes, which encode the stimuli and response for each trial.

Skill acquisition

I have applied the PEP model widely to a diverse range of domains. First, consider skill acquisition. When performing a novel task, performance tends to follow a predictable practice curve. This has often been referred to as the power law of practice, as participant-averaged, blocked data fits a power curve, whereby performance rapidly improves early on, then continues to improve at ever diminishing rates toward asymptote (Logan, 1988; Newell & Rosenbloom, 1981), though the curve better fits an exponential decay function in individual-trial, within-participant analyses (Heathcote, Brown, & Mewhort, 2000; Myung, Kim, & Pitt, 2000). The reason for this shape is simple: with a novel task, we are initially quite slow. With a small amount of practice, we can speed up greatly. However, the more and more we have practiced, the less and less it is possible to speed up even more (e.g., if after some amount of practice we halve our performance from 1000 ms per trial to 500 ms it is impossible to speed up another 500 ms). The gains from further practice are therefore smaller and smaller the more and more we have already improved.

The PEP model produces practice curves for a simple reason: the more and more exemplars there are linking a given stimulus (e.g., blue) to a given response (e.g., J key) the faster and faster the appropriate response can be retrieved (this differs a bit from the practice simulations of Logan, 1988, where memory traces race and the winning trace determines the response). Figure 4.2 shows simulated data from a Stroop experiment (Schmidt et al., 2016). As can be observed, incongruent, congruent, and neutral trials all improve with practice, and this follows a simple practice curve. Incidentally, these data also illustrate why many *effects*, such as the Stroop effect here, decrease with extra practice: as performance improves toward asymptote with extended practice, initially slow trial types (e.g., incongruent trials) stand to benefit more from practice than initially fast trial types (e.g., congruent trials). Thus, the model not only explains practice curves, but also (and for the same reason) why Stroop effects decrease with practice (Dulaney & Rogers, 1994; Ellis & Dulaney, 1991; MacLeod, 1998; Simon, Craft, & Webster, 1973; Stroop, 1935).

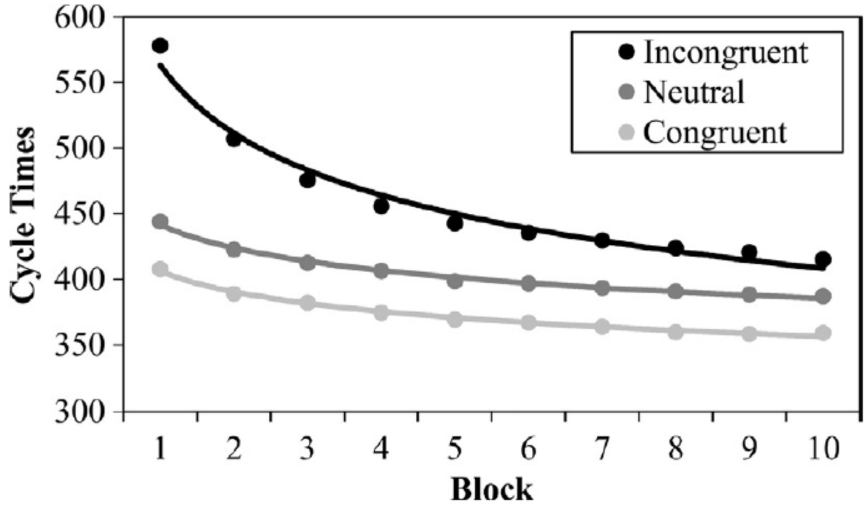

Figure 4.2. PEP-simulated practice curves of a Stroop task from Schmidt and colleagues (2016), Simulation 1.

Contingency learning

For essentially an identical reason, the PEP model also learns contingencies. For instance, in the colour-word contingency learning paradigm (discussed in the Chapter 2 on contingency learning) neutral distracting words are predictive of the likely colour response. Participants respond faster to high contingency trials relative to low contingency trials. This is, in the model, due to exactly the same storage and retrieval processes that produce practice curves. In particular, each time that a distracting stimulus (e.g., “move”) is presented and linked with a response to the target colour (e.g., the key for blue), there will be a new trace linking the word to the response. If “move” is presented most often in blue, then most “move” memories will be linked to a blue response. Thus, presentation of “move” to the network will bias a blue response via memory retrieval. As such, retrieval of the high contingency response is faster than retrieval of a low contingency response. The model not only simulates the simple two-condition difference between high and low contingency trials, but also simulates a range of findings from the learning domain.

First, we observed that the model produces the rapid acquisition curves that we observe in the colour-word contingency learning procedure. As illustrated in Figure 4.3, there is an initial practice curve in the practice phase (where only colours are presented). In the learning phase (where predictive words are presented in colours), learning is extremely rapid in both the PEP model and in the participant data. Contingency effects do increase slightly with continued training, but learning effects are observed almost immediately from the start of training.

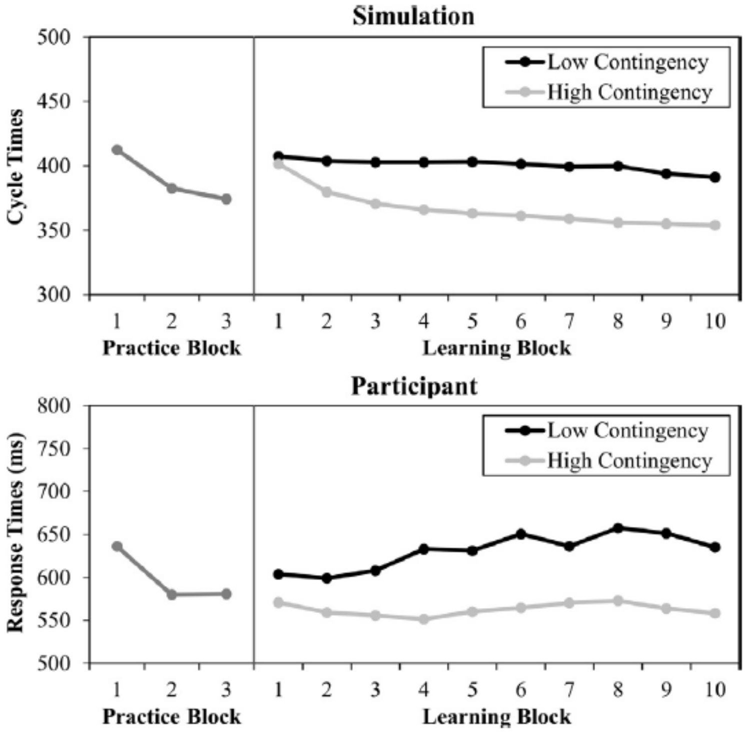

Figure 4.3. Simulation 2 cycle times from Schmidt and colleagues (2016) of initial practice trials (left) and the following high and low contingency trials across (right) across training blocks, with original data (Schmidt & De Houwer, 2016b, Experiment 2).

In Simulations 4 and 5 of the same paper, we simulated the two experiments from Schmidt and De Houwer (2016a) assessing facilitative effects of contingencies (discussed in the *item frequency* section of Chapter 2). The results of Simulation 4 are presented in Figure 4.4. As can be observed, the model produces the fastest responses for the highest-frequency pairings of stimuli (high contingency), the slowest responses for the lowest-frequency pairings (low contingency), and intermediate response times to the three types of medium contingency trials (see Chapter 2). The results of Simulation 5 are presented in Figure 4.5. Again, the model produces a similar pattern of means as in the participant data: fastest responses to high contingency items, intermediate response times to low contingency items, and the slowest performance to once-presented novel-word trials.

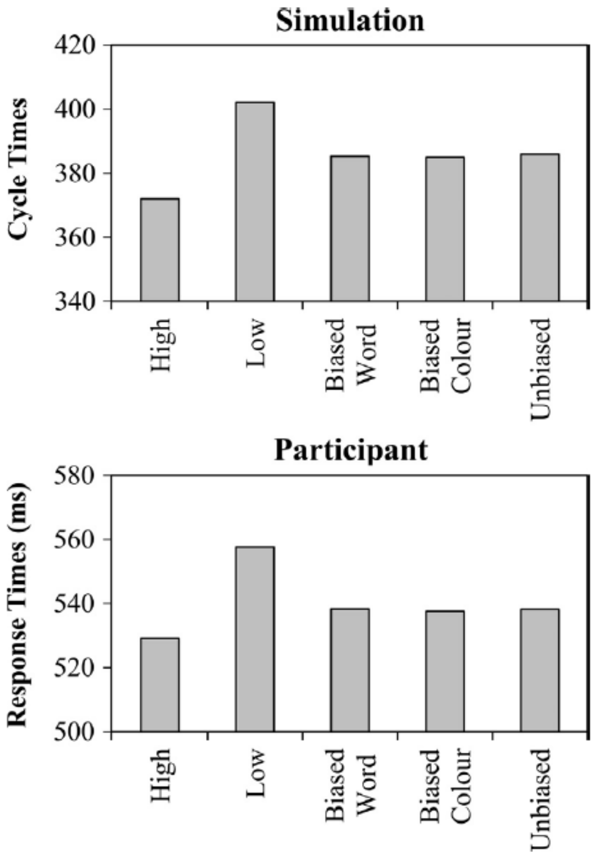

Figure 4.4. Simulation 3 cycle times from Schmidt and colleagues (2016) of high contingency, low contingency, and three types of medium contingency trials, with original data (Schmidt & De Houwer, 2016a, Experiment 1).

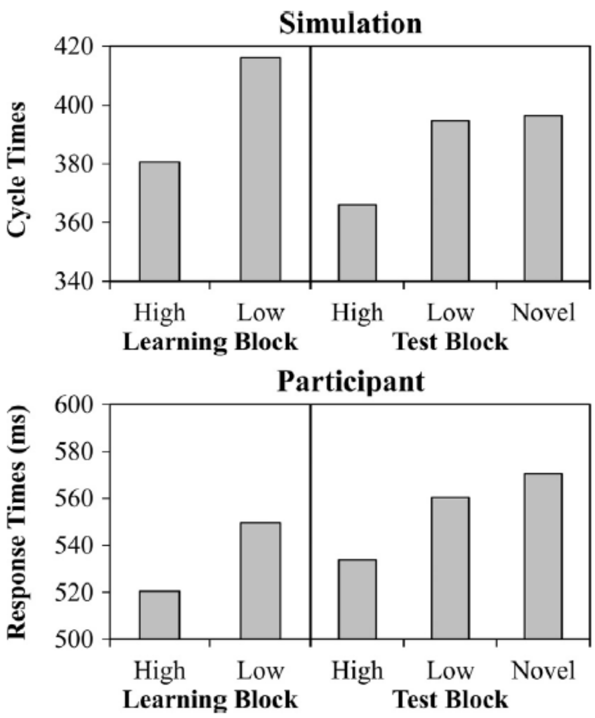

Figure 4.5. Simulation 4 cycle times from Schmidt and colleagues (2016) of high and low contingency trials during training (left) and high contingency, low contingency, and novel word trials during test (right), with original data (Schmidt & De Houwer, 2016a, Experiment 2).

In a subsequent paper (Schmidt, 2018), I modelled findings from a report of Forrin and MacLeod (2017). In their experiments, they assessed asymmetries in colour-word contingency learning effects across different response modalities (word reading, colour naming, and key press) and across differing stimulus onset asynchronies. Their main motivation was to assess a "horserace" model of colour-word contingency learning effects, similar to related ideas that had previously been considered (and discarded) in the Stroop literature (Dunbar & MacLeod, 1984; Dyer, 1973; Klein, 1964; Morton & Chambers, 1973; Palef & Olson, 1975; Warren, 1972). The notion, illustrated in Figure 4.6, is that words are read faster than colours are named, such that contingencies associated with distracting words should influence colour identification to a greater extent than contingencies associated with distracting colours should influence word identification. With verbal responding, this was observed, with contingency effects larger in colour naming than in word reading. However, the same asymmetry was not observed in manual, key press responses. The authors were also able to "reverse" the verbal asymmetry by presenting colours in advance of words. The horserace notion is that giving the "slower" dimension (colour) a "head start" should allow the colour to have more influence on words.

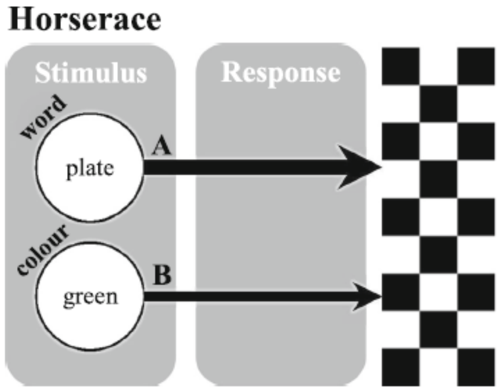

Figure 4.6. A simple horserace model as it applies to contingency learning paradigms. The "word horse" runs faster to the response "finish line" (checkered) than the "colour horse," producing an asymmetry in the magnitude of colour and word identification contingency effects.

In my response article, I considered a slightly modified version of the horserace model, illustrated in Figure 4.7. Although true that past work has shown that word reading is faster than colour naming (Cattell, 1886; Fraisse, 1969), this does not mean that word stimuli are processed faster than colour stimuli (Melara & Algom, 2003). Rather, the time between stimulus presentation and verbalisation is faster for words than for colours. If we consider stimulus identification and the translation (Sugg & McDonald, 1994; Virzi & Egeth, 1985) of that identified stimulus to a vocal response as two different things, then it may actually be the case that words are not (visually) processed especially fast, but only that the identified word can be rapidly converted to a vocal output. That is, word (lexical) to pronunciation is much more direct than the path from a colour representation (e.g., pictorial) to the appropriate colour label pronunciation. Indeed, reading words is much more heavily practiced than naming colours. Indeed, word detection does not seem to be especially fast (Fraisse, 1969). Thus, I proposed that the advantage that words have over colours with a vocal response (reading/naming) is not a benefit in stimulus-processing speed but a benefit in the compatibility between targets and responses (i.e., response-selection speed), inspired by the dimensional-overlap model (Kornblum, Hasbroucq, & Osman, 1984; Kornblum & Lee, 1995; Kornblum, Stevens, Whipple, & Requin, 1999; Zhang & Kornblum, 1998; Zhang, Zhang, & Kornblum, 1999).

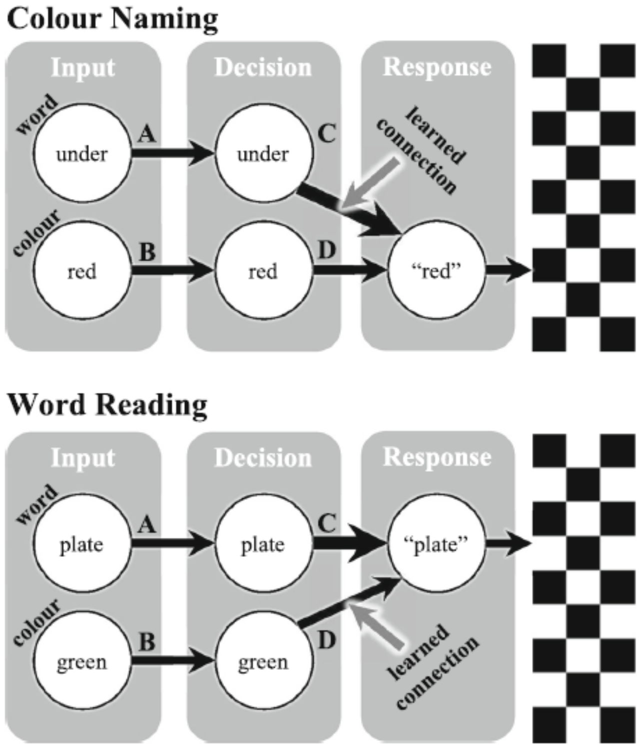

Figure 4.7. An expanded horserace model as it applies to colour-naming and word-reading contingency learning paradigms. Most critically, it is unclear why words should influence colour naming at a particularly strong rate when the word is not a potential response (i.e., why C in the top panel should be stronger than D in the bottom panel, indicated as learned connections).

In one simulation, I showed that the size of the contingency effect should be determined by how automatic the target can be translated into a response, as illustrated in Figure 4.8. When less automatic (small decision-response weighting in the figure), responding should be slow and a contingency for the task-irrelevant dimension should have more time to influence responding, thereby producing a large contingency effect. This will be the case for both colour and word identification with key presses, because colour-to-key and word-to-key mappings are novel. This also explains why there is no asymmetry in key press responses. However, in verbal tasks, word reading is more automatic than colour naming, so the contingency effect should be smaller for the former case relative to the latter. In a second simulation, it was further shown that this asymmetry can reverse if the colour is presented in advance of the word, as illustrated in Figure 4.9. The reason for this, of course, is similar to what Forrin and MacLeod (2017) propose: the word gets less time to influence colour naming and the colour gets more time to influence word reading.

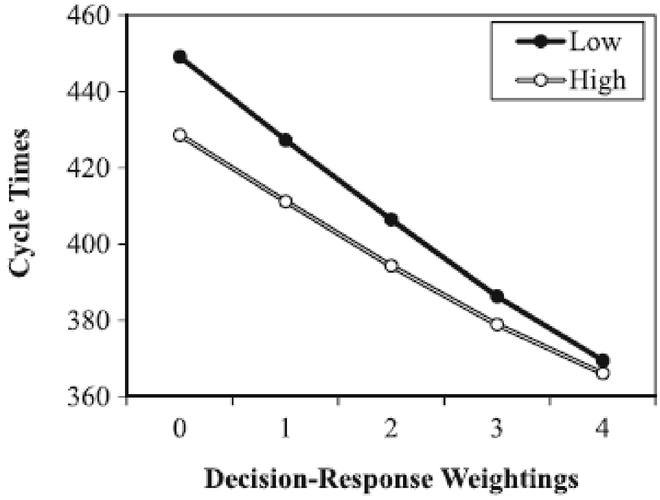

Figure 4.8. Simulation 1 cycle times from Schmidt (2018) for high-contingency and low-contingency trials as a function of overtrained decision-response weightings.

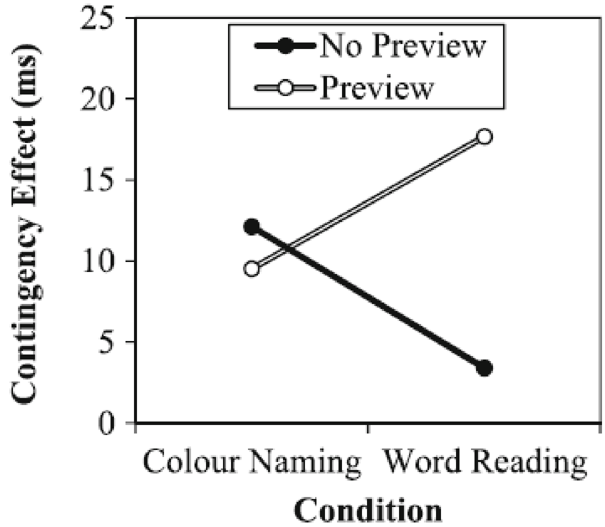

Figure 4.9. Simulation 2 contingency effect from Schmidt (2018) as a function of colour preview and target dimension.

Stimulus-response binding

Within the memory store of the PEP model, there is a retrieval-induced decay mechanism. In short, recently-encoded memories are more strongly retrievable than older memories. What this means is that events that were encountered recently have more influence on behaviour than older events. This is simply a normal product of a learning rate, as would also be present in other types of models. For instance, each time that weights are updated in a distributed store, learning based on older events are effectively weakened when encoding something new (even too strongly, as indicated by catastrophic forgetting; see French, 1999). This property of the model means that performance is influenced particularly strongly by just-encountered bindings. As a result, the model can simulate stimulus-response binding effects

(Schmidt et al., 2016). For instance, as discussed in Chapter 2, in the distracter-response binding literature (Frings et al., 2007) it is observed that there is a distracter repetition benefit when the same response is required as in the immediately-preceding trial. For instance, if a participant just saw “K” as a flanker to the target letter “J,” the participant will be faster on the current trial to respond to a “J” target again if the distracter is (again) “K” relative to if a new distracter (e.g., “D”) is presented. In contrast, if a new target is presented (e.g., “F”) then there is a small distracter repetition cost. This full pattern of behaviour is accounted for by the PEP model. In particular, if one just responded to a trial with a “K” flanker by pressing the “J” key (i.e., to the target “J”), then presentation of “K” again as a distracter will bias another “J” response. This will help if the response does need to repeat, but will harm performance slightly if a new response is required. Simulated data are presented in Figure 4.10.

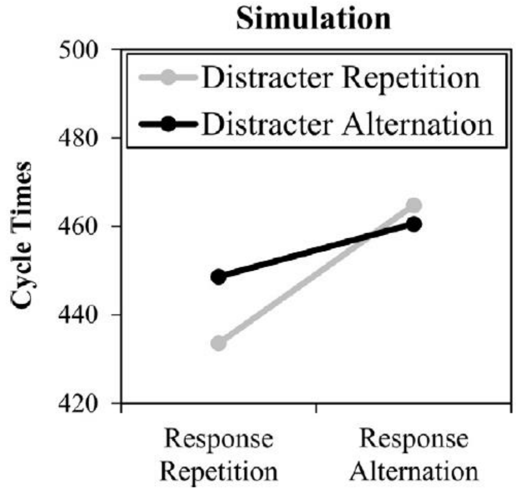

Figure 4.10. Simulated distracter-response binding effect from Schmidt and colleagues (2016).

Rhythmic timing

The PEP model also simulates rhythmic timing processes. As currently implemented (although this is currently undergoing revision), the model stores timing information in each exemplar. More precisely, the model stores how long it took to respond to a given stimulus (i.e., response time). Using this information at retrieval, the model anticipates *when* to respond, not merely what to respond. This allows the model to simulate rhythmic timing (i.e., autocorrelated response times), which is ubiquitous in cognitive paradigms (Gilden, Thornton, & Mallon, 1995; Gilden, 1997, 2001). Using this mechanism, the model was able to simulate *mixing costs* (Los, 1994, 1996, 1999b, 1999a; Lupker et al., 1997; Van Duren & Sanders, 1988), the observation that response speed to easy (normal target) and hard (degraded target) items are both slowed (but especially to easy items) when easy and hard items are intermixed in one procedure (relative to when only easy or only hard stimuli are presented). The data are presented in Figure 4.11.

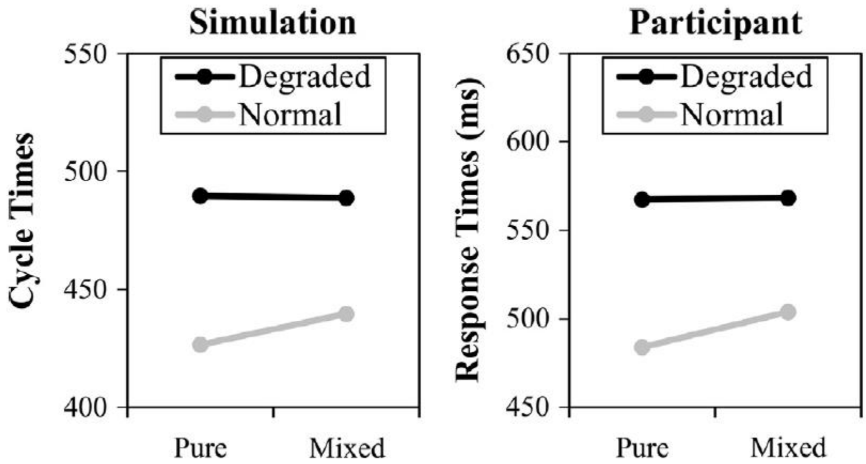

Figure 4.11. Simulated mixing cost (left) with participant data (right) from Schmidt and colleagues (2016).

Attentional control paradigms

Using the same model, I have shown how the simple memory encoding and retrieval processes discussed above can also explain behaviour in a range of so-called attentional control paradigms. As discussed in Chapter 3 on attentional control, I have frequently argued that many measures of conflict monitoring, such as proportion congruent (PC) and congruency sequence effects (CSE), are explainable in terms of simple learning biases. In addition to experimental work aimed at dissociating learning "confounds" from true attentional control effects, I have also used the PEP model to demonstrate why observed results in the attentional control domain may have little to do with attentional control at all. That is, it is shown how the PEP model can reproduce key findings even though the model does not monitor conflict or control attention at all. Given extensive discussion of most of the modelled phenomena in Chapter 3, I present only a brief summary of the PEP simulations below.

In Schmidt (2013a), I showed that stimulus-response contingency learning can explain the item-specific PC effect. In particular, the PEP model does not have a conflict monitor and does not adjust attention in response to conflict. However, the model does learn contingencies. Words that are mostly congruent are predictive of the congruent response (e.g., because "blue" is presented most often in blue). This speeds congruent trials, inflating the congruency effect. In contrast, words that are mostly incongruent are (depending on the manipulation) predictive of an incongruent response (e.g., because "green" is presented most often in yellow). This speeds incongruent responses, thereby decreasing the congruency effect. The net result is a small congruency effect in the mostly incongruent condition, as illustrated in Figure 4.12.

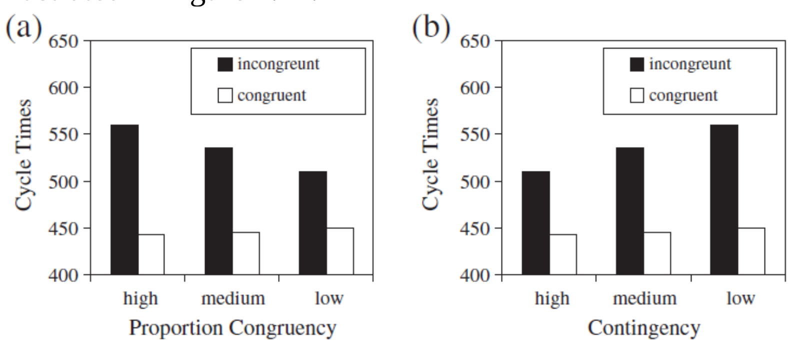

Figure 4.12. Simulation 1 cycle time data from Schmidt (2013a) with congruency as a function of (a) proportion congruency and (b) contingency.

In Schmidt (2013c), I further showed that temporal learning biases can explain the list-level PC effect. That is, list PC is manipulated with some (contingency biased) items and tested with some intermixed transfer items that are not contingency biased. The PC effect for the transfer items results from the faster task pace in the mostly congruent condition relative to the mostly incongruent condition, similar to the mixing cost explained above (see also Chapter 3 for more information). The simulated data are presented in Figure 4.13.

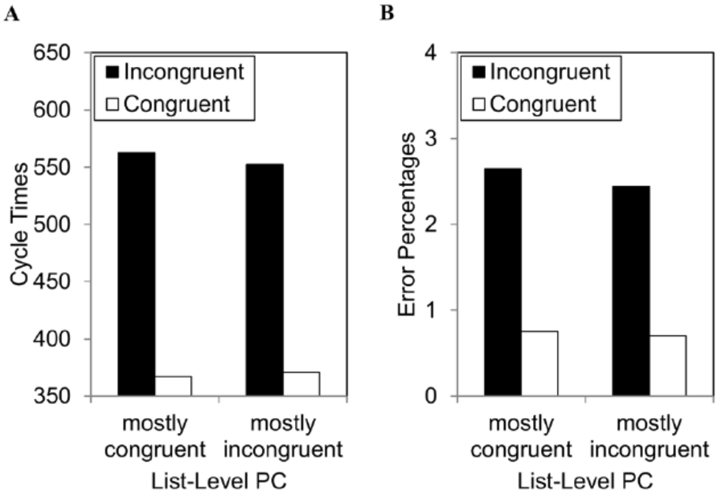

Figure 4.13. Analysis 1 data from Schmidt (2013c) for congruency and proportion congruency. Model-simulated (A) cycle times and (B) error percentages of Hutchison (2011).

For a similar reason as why the same memory mechanism explains both contingency learning and binding, the model also predicts an influence of *recent* events on rhythmic timing. For this reason, the model predicts that congruency effect should be larger following (fast) congruent trials than following (slow) incongruent trials. In Schmidt and Weissman (2016), we showed that the PEP model therefore also simulates a CSE, even after controlling for contingency and binding confounds. Simulated data for a latter version of the model are presented in 4.14.

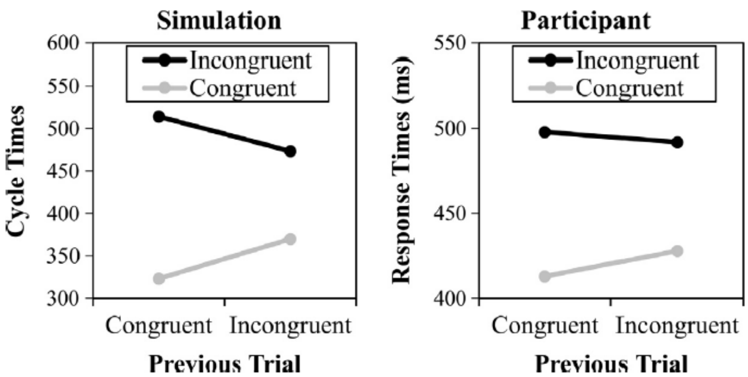

Figure 4.14. Simulation 9 cycle times from Schmidt and colleagues (2016) for congruent and incongruent items following congruent and incongruent trials, with original data (Schmidt & Weissman, 2014, Experiment 1).

In Schmidt (2016b), I further investigated *asymmetric list shifting* effects. As described in a prior chapter, an asymmetric list shifting effect is the observation that the congruency effect diminishes more drastically when switching from a mostly congruent block of trials to a mostly incongruent block of trials relative to the increase in the congruency effect when switching from a mostly incongruent block to a mostly congruent block (E. L. Abrahamse, Duthoo, Notebaert, & Risko, 2013). As also described earlier, this effect is normally interpreted as evidence for attentional control, but can be better accounted for by a practice bias. Indeed, the PEP model also produces asymmetric list shifting effects due to the contingency and practice confounds discussed in Chapter 3. The results from the first of two simulations are presented in Figure 4.15.

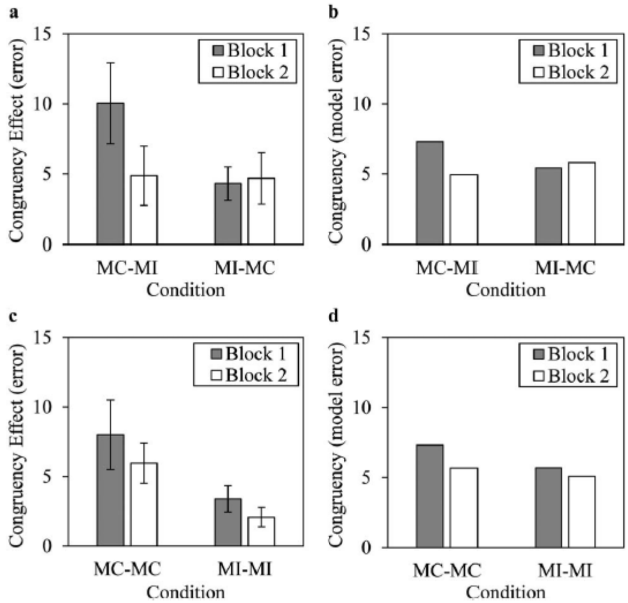

Figure 4.15. Experiment 1a (a) percentage errors and (b) model errors, and Experiment 1b (c) percentage errors and (d) model errors for Abrahamse and colleagues (2013) as modeled in Schmidt (2016b). MC = mostly congruent; MI = mostly incongruent.

In yet another modelling investigation (Schmidt, 2016a), I demonstrated that context-specific proportion congruent effects can also be explained by simple contingency learning biases (as was later confirmed in experimental research; see Schmidt & Lemercier, 2019). The modelling results also indicated that transfer effects (Crump & Milliken, 2009; Crump et al., 2017; cf., Hutcheon & Spieler, 2017, for failures to replicate transfer) can be explained by (context-specific) temporal learning. Interestingly, these modelling results for "context-specific" PC effects require no added assumptions. The model simulates context-specific effects for exactly the same reason as for item-specific and list-level PC effects.

Works in Progress and Future Directions

Task switching and instruction following

In currently ongoing research, we have been applying the PEP model to cued task switching and instruction following (Schmidt et al., 2017). We have been able to show that the PEP model is able to reproduce a broad range of key findings from the cued (and uncued) task switching domain, including the switch cost, task-rule congruency effects (and their asymmetry with task switch), response repetition effects (and their asymmetry with task switch), cue repetition benefits, and a full ten-condition binding decomposition of cued task switching (Schmidt & Liefooghe, 2016; see Chapter 5 for more information).

Further applications to binding

So far, the PEP model has been applied directly to distracter-response binding paradigms, as described above, and indirectly to other phenomena, as in the task switching example above. However, a future goal is to apply the PEP model more widely to a range of findings within the binding and action control domains. This work will be carried out in the context of a German Research Foundation (DFG) grant spearheaded by Christian Frings.

Action sequencing

Other research will aim to extend the PEP model beyond its current breadth to learning across sequences. The aim will be to simulate performance across a range of sequencing domains, such as implicit sequence learning, action sequencing, skilled typing, and music learning. The tentative solution to the “sequencing problem” that I will be exploring will also form a coherent account of event timing.

Chapter 5 – Other Research

Published Work

Notes

Some additional research that I have conducted over the years does not fit as neatly into one of the categories in the four preceding sections. Some of these other research lines also make up a relatively small percentage of my work and perhaps do not warrant an entire chapter on their own. As such, the present section presents some of my smaller research projects. This includes research on formal reasoning, temporal learning (outside of the attentional control domain; cf., Chapter 3), and cued task switching.

Formal reasoning

I have published one paper, from work as an undergraduate student, on formal reasoning. Consider immediate inference and syllogistic reasoning tasks, illustrated in Figure 5.1. In an *immediate inference* task, participants are given a single premise that they are asked to assume is true (e.g., "Some of the chemists are beekeepers") and are then asked whether a given conclusion statement (e.g., "All of the chemists are beekeepers") follows from this premise. A *syllogistic reasoning* task is similar, except that there are two premises. In the immediate inference example, the reasoner can decide "Yes" if the conclusion is necessarily true on the basis of the premise, "No" if the conclusion is necessarily false on the basis of the premise, or "Maybe" if the conclusion is neither necessarily true nor necessarily false (note that the distinction between "No" and "Maybe" is typically not considered in the philosophy of logic, both of which are simply considered "false"). Because "some" means "at least one and possibly all" in logic, this particular problem is indeterminate (i.e., "Maybe").

Immediate Inference

IF IT IS TRUE THAT:
Some of the chemists are beekeepers
THEN IS IT THE CASE THAT:

All of the chemists are beekeepers	Y__	N✗	M✓
None of the chemists are beekeepers	Y__	N✓	M__
Some of the chemists are beekeepers	Y✓	N__	M__
Some of the chemists are not beekeepers	Y✗	N__	M✓

Syllogistic Reasoning

IF IT IS TRUE THAT:
Some of the chemists are not beekeepers
All of the beekeepers are musicians
THEN IS IT THE CASE THAT:

None of the musicians are chemists	Y__	N✗	M✓
Some of the musicians are chemists	Y✗	N__	M✓

Figure 5.1. Example problems for the immediate inference and syllogistic reasoning tasks. Correct responses are indicated with a checkmark. Incorrect responses that seem to follow from a pragmatic interpretation of "some" are marked with a cross.

The reader may already notice a strange problem with this example. In everyday usage (pragmatics), "some" means something entirely different. We would normally interpret "some" as something like "some but not all" or "some are, but some are not." Thus, "all" chemists definitely cannot be beekeepers if *only* "some" of the chemists are beekeepers, right? Well, according to formal logic rules, this can be true. Relatedly, if James tells Christelle "Some of the employees are part of the union," then Christelle will likely infer that some employees are *not* part of the union (because only *some* of the employees are). Christelle would reasonably assume that if James had meant "all," then he would have said "all." This is termed the Gricean maxim of quantity (or informativeness; Grice, 1975). Thus, some syllogistic reasoning "errors" may not be errors at all. Rather, the error may reflect a Gricean, rather than a logical, interpretation of the premises.

In the formal reasoning research domain, there are many developed models that attempt to explain the regularities in the errors that participants make when making logical inferences (Henle, 1962; Johnson-Laird, 2010). However, what we argued in Schmidt and Thompson (2008) is that a substantial proportion of the errors that participants make in formal reasoning tasks can actually be due to misunderstanding of the "logical" meaning of the word "some." In logic, proper inferences can be inferred from the *square of opposition*, illustrated in Figure 5.2. However, we proposed that participants might instead represent the "moods" in a *triangle of opposition*, where "some" and "some...not" are treated as equivalent and both contrary (mutually exclusive from) "all" and "none." Thus, for many problems (like the examples above), reasoners may actually be making appropriate inferences, but based on a misunderstanding of how they should interpret "some."

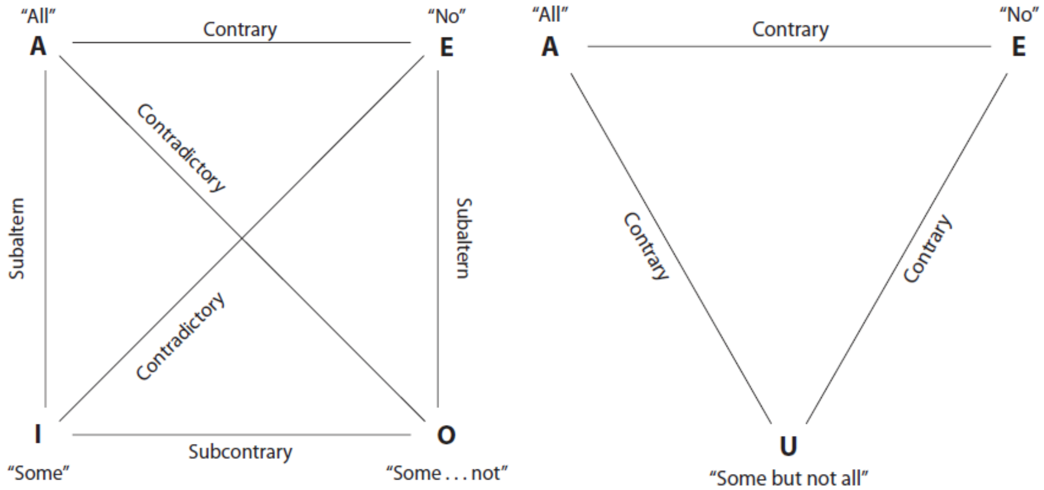

Figure 5.2. There are four moods in the square of opposition (left): (A) universal affirmative: "All of the As are Bs," (E) universal negative: "None of the As are Bs," (I) particular affirmative: "Some of the As are Bs," and (O) particular negative: "Some of the As are not Bs." However, participants might interpret premises in accordance with the three moods of a triangle of opposition, replacing I and O with (U) partition: "Some but not all of the As are Bs," which is assumed to be equivalent pragmatically to "Some of the As are Bs" and "Some of the As are not Bs."

To test this idea, we changed "some" to "at least one" in the problems, and reasoning errors were drastically reduced. More specifically, participants given the "some" quantifier responded in the way one would expect if they had interpreted "some" to mean "some, but not all," whereas participants given the "at least one" quantifier responded in the "correct" way. In a first study, participants were given only positive premises (i.e., "assume the premise is true") in an immediate inference task, with either standard (e.g., "some"), clarified (e.g., "at least one"), or "pragmatically-clarified" (e.g., "some but not all") quantifiers. Performance on the critical problems was best with clarified quantifiers and worst with pragmatically-clarified quantifiers. In a second study, participants were given both positive and negative premises (e.g., "assume the premise is *false*"). Again, reasoning was largely consistent with the "logical" interpretation with "at least one" quantifiers and with the "pragmatic" interpretation with "some" quantifiers. A third study used a syllogistic reasoning task with similar results. The data from the third study are presented in Figure 5.3. The correct response for the key problems was "Maybe," with some that pragmatically suggest a "No" response (pragmatically false) and others that suggest a "Yes" response (pragmatically true). There is certainly much more to reasoning errors than just misinterpretation of quantifiers, but it can be observed that a large percentage of errors are accounted for by quantifier misinterpretation.

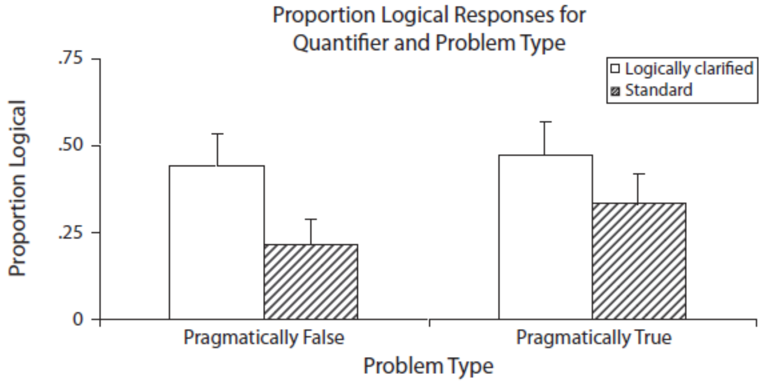

Figure 5.3. Proportion of logical responses and 95% confidence intervals for pragmatically true and pragmatically false syllogisms as a function of premise type in Experiment 3 from Schmidt and Thompson (2008).

Temporal learning

I have conducted several experiments on temporal learning (rhythmic timing). Many of these were in the context of temporal learning "confounds" in measures of attentional control. These studies were discussed in Chapter 3 on attentional control. However, I have also done some work on timing that is less directly related to attentional control. In particular, I published a paper (Schmidt, 2016c) in which I explored whether rhythmic timing is impaired when *response-stimulus intervals* (RSIs) were variable. That is, we know that participants time their responses rhythmically (i.e., similar response times from one trial to the next), but might rhythmic timing be disrupted if the time between the end of one trial and the start of the next is randomly varied on a trial-by-trial basis? According to one hypothesis, one might propose that rhythmic timing is due to participants pressing keys at similar intervals. That is, participants might be learning to anticipate making a response relative to the time of the last response. If so, then random variations in the time between the end of one trial and the start of the next should impair rhythmic timing. This is illustrated in Figure 5.4. On the other hand, if participants are learning to time responses relative to stimulus onset, then changes to the RSI are irrelevant.

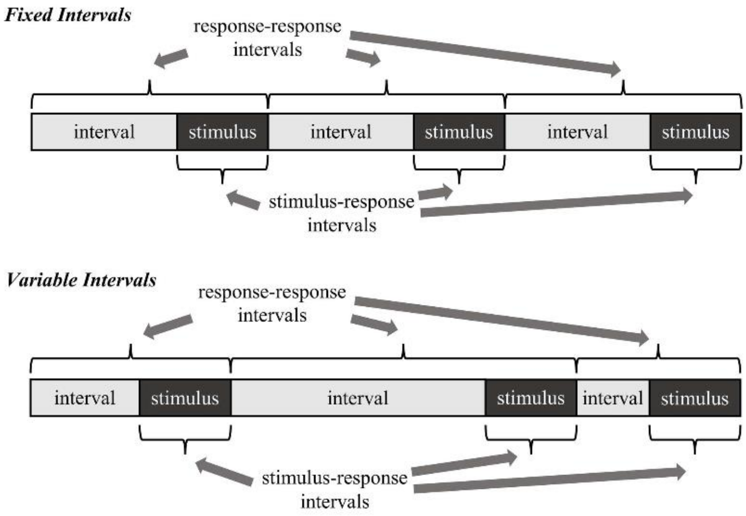

Figure 5.4. Illustration of how RSI manipulations influence response-response and stimulus-response intervals. Note that only the response-response intervals become inconsistent with variable intervals.

The experiment used a similar "proportion easy" procedure as described earlier in Chapter 3. In particular, a given participant was either presented with mostly high contrast target digits or mostly low contrast. As before, the temporal learning prediction is that the contrast effect should be larger in the "mostly easy" condition relative to the "mostly hard" condition. Interestingly, the proportion easy effect was equally robust in response times both when the RSIs were kept constant and when they were variable, as illustrated in Figure 5.5. These results therefore suggest that rhythmic timing is planned relative to stimulus onset. Subsequent analyses further showed that proportion easy effects were influenced by task rhythms in the expected way, with shifts in the distribution of response times in the high and low contrast conditions within the skewness and kurtosis of the distributions that were different in the mostly easy and mostly hard conditions.

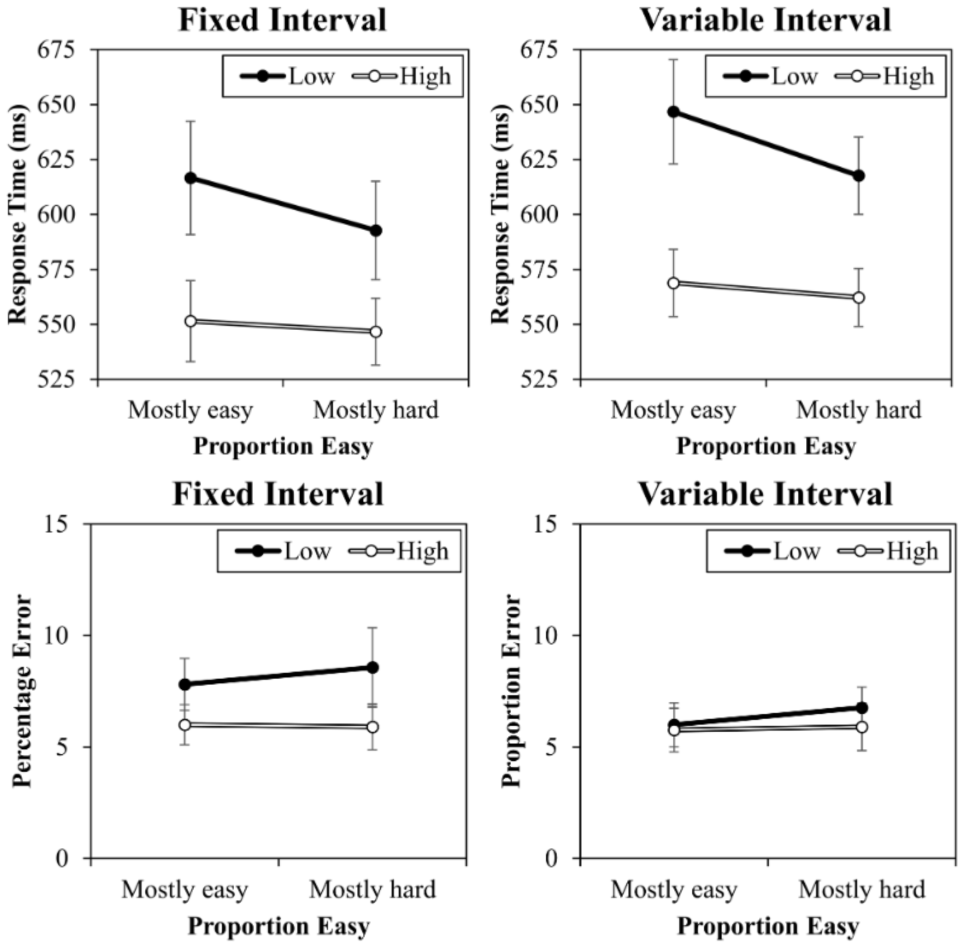

Figure 5.5. Results from Schmidt (2016c). Response times (top) and error rates (bottom) with standard errors for the fixed interval (left) and variable interval (right) conditions.

Cued task switching

In Chapter 3, it was explored how learning biases confound purported measures of attentional control. Another example of simple learning biases in tasks aiming to study executive control comes from the task switching domain (Jersild, 1927; for reviews, see Kiesel et al., 2010; Monsell, 2003; Vandierendonck, Liefooghe, & Verbruggen, 2010). Consider the *cued task switching* procedure. In one fairly typical version of this procedure, illustrated in Figure 5.6, participants are presented a digit from 1-9, excluding 5, on each trial and are asked to complete one of two tasks. On some trials, participants have to decide whether the digit is odd or even (parity). On other trials, they need to decide whether the digit is large (>5) or small (<5; magnitude). The colour of a cue informs participants which of two tasks to perform on a given trial. The typical finding is a *switch cost*, where performance is substantially slower and less accurate when the task alternates (e.g., a parity decision after a magnitude decision) relative to when the task repeats (e.g., a parity decision after a parity decision). This switch cost is typically interpreted as an index of cognitive control over "task sets." Though accounts vary slightly (Allport, Styles, & Hsieh, 1994; Meiran, 1996; Meiran, Kessler, & Adi-Japha, 2008; Rogers & Monsell, 1995; Yeung & Monsell, 2003a, 2003b), the central notion is that the cognitive system has to "reprogram" itself for a task switch, and this produces the switch cost.

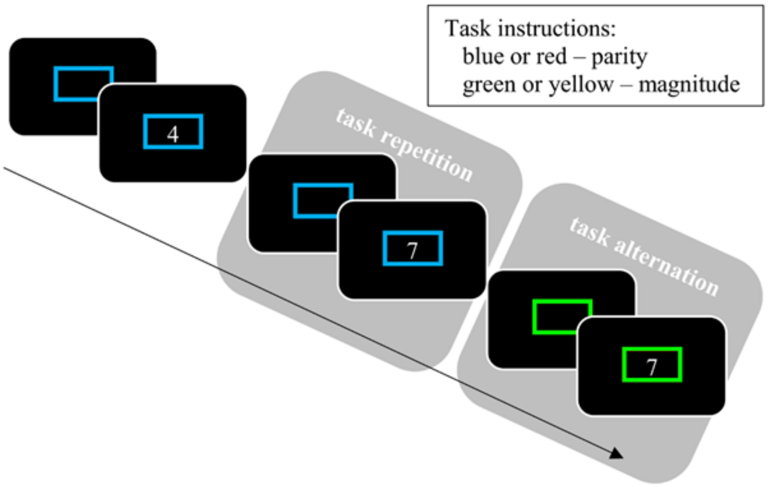

Figure 5.6. Typical cued task switching procedure. Participants have to either identify the parity or magnitude of a digit on the basis of a cue colour.

Contrary to this, we have recently shown that the majority of this switch cost is due to simple stimulus-response binding biases (Schmidt & Liefooghe, 2016). For instance, if the task repeats, then it is possible that the stimulus (e.g., 3), cue (e.g., blue), decision (e.g., "odd"), and required response (e.g., left key) all repeat from the immediately preceding trial. As such, performance can be fast and accurate simply because a memory of the just-encoded event can be re-retrieved, allowing for quick selection of the appropriate response. This sort of "complete repetition" is never possible on a task alternation. In fact, it can even be possible that participants are required to make a different response to the same stimulus on a task alternation (e.g., left key for "odd" and right key for "large" to 7), which causes significant interference in memory retrieval. Table 5.1 presents ten unique trial types that emerge out of this design from our Experiment 2. As can be observed, a sequence of two trials can vary considerably in what features of the task do or do not repeat from the prior trial.

Table 5.1. Ten trial types in Experiment 2 of Schmidt and Liefooghe (2016).

	Repetition Type				
Condition	Task	Cue	Stimulus	Conceptual Response	Physical Response
cue-RR	✓	✓	✓	✓	✓
cue-AR	✓	✓	✗	✓	✓
cue-AA	✓	✓	✗	✗	✗
rep-RR	✓	✗	✓	✓	✓
rep-AR	✓	✗	✗	✓	✓
rep-AA	✓	✗	✗	✗	✗
alt-RR	✗	✗	✓	✗	✓
alt-RA	✗	✗	✓	✗	✗
alt-AR	✗	✗	✗	✗	✓
alt-AA	✗	✗	✗	✗	✗

Binding biases are not orthogonal to the main effect of task switching. In fact, binding biases like these systematically work to the favour of task repetitions and to the detriment of task switches. We can, however, separate these simple binding biases from "true" switch costs by assessing trials (for both task repetitions and switches) in which none of the elements of the task repeat (marked in grey in Table 5.1). With this method, we were able to show that the vast majority of the switch cost is due to these types of "binding" biases alone, and not to executive control processes. The data are presented in Figure 5.7. Similar findings were observed in a non-cued version of task switching in Experiment 1.

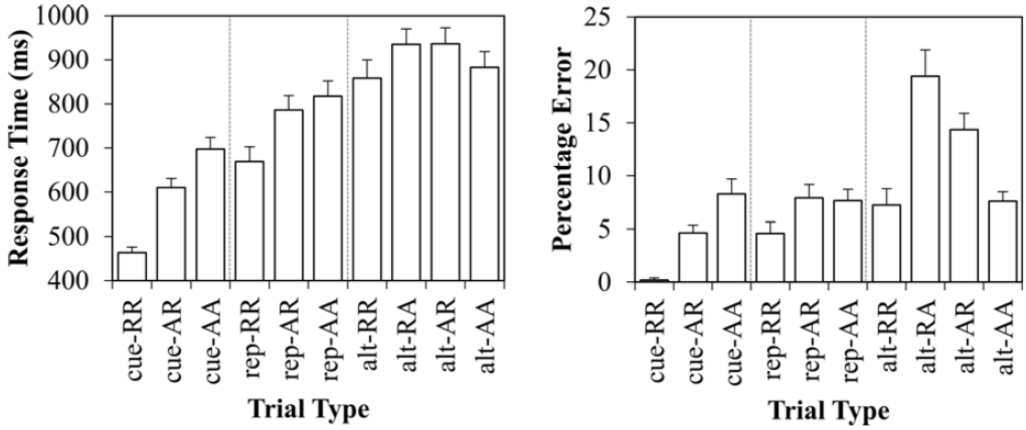

Figure 5.7. Experiment 2 responses times in milliseconds (left) and percentage errors (right) with standard error bars from Experiment 2 of Schmidt and Liefooghe (2016).

In-progress modelling work (Schmidt et al., 2017) has also shown that a number of key findings from the task switching domain are a logical consequence of these binding biases (e.g., task rule congruency effects, asymmetric response repetition biases, etc.). In future research, I will experimentally explore which findings from the task switching domain can be explained by binding biases and which by control.

Works in Progress and Future Directions

Musical Stroop

In another line of studies, I will explore a potential application of some of the contingency learning principles that I have studied over the years (see Chapter 2) to music training. In particular, the aim will be to develop a method to improve music scale acquisition. Initial work will begin with the musical Stroop task (Grégoire, Perruchet, & Poulin-Charronnat, 2013, 2014a, 2014b; Grégoire et al., 2015), illustrated in Figure 5.8. In this task, participants are presented with a note on a musical scale on each trial. Inside the note is the name of a note ("do," "re," "mi," etc.), and the task is to read the note name (i.e., ignoring the actual location of the note). Critically, the note name is either congruent with the actual note (e.g., "do" written in the note for "do") or incongruent (e.g., "do" written in the note for "mi"). Participants with substantial musical training read the notes slower and with more errors on incongruent trials. That is, musically-trained participants simply cannot completely avoid translating the note location into the corresponding note name, similar to how it is difficult to avoid reading the word in the Stroop task (see Chapter 1). Of course, the musical Stroop effect indicates that, after substantial overtraining, note naming becomes automatized in musicians.

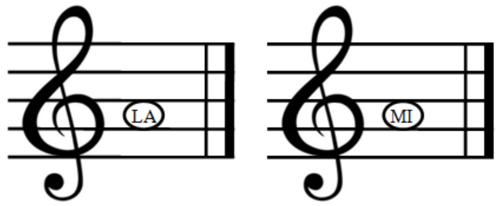

Figure 5.8. Example congruent (left) and incongruent (right) musical Stroop trials.

Not surprisingly, non-musicians do not produce a musical Stroop effect. The note location does not influence their performance because they do not know the note names to begin with. Of course, if a novice musician wants to learn the music scale, they could do this the typical way by studying the scale on paper. However, we know that contingencies can be learned rapidly and implicitly in (non-musical) contingency learning procedures. The same might prove effective in music learning. To test this notion, I will use modified versions of the musical Stroop task in which I present notes most often (or always) with the correct note name written inside of it (i.e., only occasionally or never incorrect). I can then assess whether this leads to rapid note name learning in non-musicians. Based on non-musical learning results, it is unlikely that this will not work. I will further explore how amount of training, contingency proportions (e.g., 80% vs. 100% congruent), and type of training (musical Stroop vs. explicit training) influences the acquisition speed and persistence of acquired knowledge of the musical scale. Potentially, such training procedures could prove to be a useful supplement to traditional music scale learning. If so, I may further pursue development of a free-to-use learning app for novice musicians.

Teaching statement

Past experience in teaching at university level

I find teaching to be a rewarding experience. Since the later years of my undergraduate studies, I have been involved in teaching (in addition to administration) in various capacities. Already as an undergraduate at the University of Saskatchewan, I worked as a teaching assistant, primarily for introductory psychology. In this role, I assisted with exam supervision, exam organization (head teaching assistant), and grading. I also served as a tutor during this period for advanced cognitive science.

During my Master's and Ph.D. at the University of Waterloo I had even greater teaching responsibilities. I taught a weekly statistics tutorial (the complement to the lecture component of the course). In this position, I was responsible for preparing and giving lectures for a group of around 30 psychology students. In addition to strengthening key concepts in the companion course (i.e., reviewing), I provided sample statistical problems that I prepared each week for students that we worked through interactively. For both this and the variety of other courses I served as teaching assistant for, I had yet further responsibilities. My role included everything from creating exam questions, grading, proctoring exams, handling student emails, holding office hours for student questions, various administrative course tasks (e.g., coordination with the Office for Persons with Disabilities for students with special needs), organizing make-up exams, and managing other teaching assistants. I was also involved as a "teaching assistant" for the Honour's thesis course. In this role, I served to oversee theses, and to assist students with statistics, methodology, and writing. I also gave guest lectures for a third year cognition course.

During my postdoc at Ghent University, I was more involved in administration (e.g., I managed the academic bibliography for the Department of Experimental Clinical and Health Psychology), but I was also involved in teaching duties. These duties included exam surveillance and organisation of exams for a variety of courses (e.g., learning, health, clinical, and developmental psychology). I also assisted in the grading of Master's theses. Although I did not have the opportunity to supervise Master's or Ph.D. students as a postdoc, I am currently "co-supervising" a Ph.D. student, Iva Šaban (Croatia), that I hired with my UBFC funds. I also supervised a stage student (Anna Jondot) and I am in the process of hiring a second Ph.D. student. I have only moved to France recently. Although technically not a requirement for my current post, I will likely give lectures in the coming year. I also gave a neural network modelling workshop in Trier in 2019.

Teaching methodology at university level

I hold the view that it is important for an instructor to engage students in learning. I believe that it is not enough for the instructor to know the material well. It is crucial that an instructor is dynamic and open enough to motivate students to participate in class. The instructor must remain the leader of the class, but can also allow for dialogue with students. As such, I use a variety of teaching approaches. I seize any opportunity I can to connect the material to the lives of students or to pop culture. I also prefer to use interactive demonstrations whenever possible. For instance, the Stroop effect can be demonstrated by having students name coloured colour words. This is much more effective and visceral than simply explaining the phenomenon. Similarly, students can be polled for their solutions to the Monty Hall illusion before the correct answer is explained. The inevitable protests from students afterwards can facilitate the explanation of conditional probability. Taking inspiration from exceptional academic speakers, I also work to have a good slide deck with clear visualisations to supplement what I am saying. To me, this is critical for effective communication. In contrast, reading pages of bullet-point text is boring to students and will not assist in student comprehension.

I also like to actively engage students with classroom “micro-experiments,” preferably in small working groups. As one example, a difficult concept like the standard normal distribution can be explained to students with dice. If each group of students averages a set number of die rolls, then it can be demonstrated how, across groups, the averages produce a normal distribution. Interactive examples like this provide a good starting point for explaining key concepts and also encourage interaction between students. Keeping the course interactive has another benefit. I believe that it is important to gauge how well students comprehend the foundational concepts in the course. As an instructor, student feedback helps to determine which concepts require more or less attention. It is therefore necessary to foster a classroom environment in which students feel comfortable expressing their thoughts on the material. Students should never be put on the spot in a way that makes them uncomfortable or anxious about being in class, but open questions to the class as a whole can foster feedback. For larger class sizes, modern technology can help with the same end. For instance, interactive polling with phone apps can be used as both mini “quizzes” to see whether students have understood the lecture and for micro survey experiments.

In the case of seminar courses, I also feel active student involvement is key. My preferred format involves assigning one to three articles per week for students to read. Afterwards, rather than having students simply listen to me lecture about these articles, I prefer a different approach. As long as the class size allows, I prefer to have students take turns as the weekly discussion leader. This achieves two goals. First, it helps to strengthen presentation skills, which become increasingly important over time in an academic career (and also for those leaving academia). Second, it encourages students to “dig deeper” into the material they present: one never learns more about material than when they need to teach it to another. In my preferred format, students will also prepare a weekly two-page summary and critique of the readings. I feel that this approach is useful, because it helps to motivate students to keep up with the literature assigned in the course. In addition, students enter the classroom prepared with useful ideas to contribute to the discussion. Relying exclusively on an end-of-term exam or paper, on the other hand, leaves room for student slacking (e.g., “I will read it later”). For student assessment in other course types, multiple choice is the easiest but least optimal approach. Thus, as long as class size permits, I always prefer written tests or papers. Both in academia and industry, writing skills are one of the most useful transferable skills, so I like to involve writing as much as possible in the course.

Regarding Ph.D. students, I believe that it is crucial to foster research independence as early as possible. Though thankfully not my experience as a Ph.D. student, I think that many young graduate students are used by principal investigators to collect data to further their own research agendas. To me, this is a suboptimal use of young, curious minds. As such, while I will certainly involve Ph.D. students in collaborative projects, my first goal with a young Ph.D. student will be to have them read as much as possible in a research domain with one key goal: “Tell me what you want to do.” The sooner that a Ph.D. student can answer this question and forge their own direction forward, the sooner they will escape the shadow of their supervisor. Throughout the course of the Ph.D., I feel it is important that the student maintains regular contact with me. This should include, at minimum, a weekly meeting to discuss current progress and regular active involvement in lab activities (e.g., attendance and participation lab meetings).

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