



**Universität
Zürich**^{UZH}

**Habilitation
Besprechung an Fachbereichssitzung**

**Medizinische Fakultät
Dekanat**

Universität Zürich
Medizinische Fakultät, Dekanat
Bereich Fakultätsgeschäfte
Pestalozzistrasse 3/5
CH-8091 Zürich
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Angaben zur Fachvertreterin / zum Fachvertreter:

Wer meldet die Habilitandin / den Habilitanden an?
Prof. Dr. Erich Seifritz

Angaben zur Habilitandin / zum Habilitanden:

Vollständiger Name und akademische Titel:
Dr. med. univ. Philipp Homann, PhD

Tätigkeit:
Oberarzt

Arbeitsort:
Universitätsklinik für Psychiatrie, Zürich

Lehrgebiet, deutsche Bezeichnung:
Psychiatrie und Psychotherapie

Lehrgebiet, englische Bezeichnung:
Psychiatry and Psychotherapy

Bürgerort:
bei Schweizer Staatsbürgern Bürgerort,
ansonsten bitte Geburtsort:
Regensburg

Geburtsdatum:
13.02.1980

Wohnort:
Zürich

Total der Originalpublikationen:
29

Unterschrift der Fachvertreterin / des Fachvertreters: 

Datum Besprechung im Fachbereich:
10.09.2019

v20180723



**Universität
Zürich^{UZH}**

**Medizinische Fakultät
Dekanat**

Unterschrift der Fachbereichsleiterin / des Fachbereichsleiters: _____



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Prof. Dr. Rainer Weber
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Direktor Klinik für Psychiatrie, Psychotherapie und
Psychosomatik:
Prof. Dr. med. Erich Seifritz

Zürich, 30. August 2019

**Habilitationsantrag von Herrn Dr. med. univ. Philipp Homan, PhD, geb.
13.02.1980**

Sehr geehrter Herr Dekan, lieber Rainer

Gerne möchte ich Herrn Homan zur Habilitation an der MeF der UZH anmelden.

Beruflicher Werdegang

Philipp Homan ist in Wien aufgewachsen, wo er auch von 2002 bis 2008 Humanmedizin studiert hat. Danach begann er seine Facharztausbildung sowie ein PhD-Studium an der Universitätsklinik für Psychiatrie in Bern bei Prof. Dr. Werner Strik und Prof. Dr. Thomas Dierks und arbeitete dort auch als Oberarzt, bevor er 2015 mit einem Stipendium des Schweizer Nationalfonds für insgesamt vier Jahre ans Mount Sinai Hospital und ans Zucker Hillside Hospital nach New York ging.

Unter Supervision von Prof. Dr. Thomas Dierks lernte Herr Homan während seines PhD-Studiums die nichtinvasive Hirnstimulation als neues Behandlungsverfahren bei persistierenden Positivsymptomen der Schizophrenie kennen, und konnte dazu mehrere hochrangige Studien veröffentlichen. Diese Arbeiten wurden mit dem Hans-Heimann-Preis der Deutschen Gesellschaft für Psychiatrie, Psychotherapie und Nervenheilkunde (DGPPN) 2014 sowie dem Frutiger Preis 2014 ausgezeichnet. Sein PhD schloss er 2013 an der Universität Bern mit summa cum laude ab.

Begleitend absolvierte Herr Homan seine dreijährige psychotherapeutische Weiterbildung in psychodynamischer Psychotherapie am Sigmund-Freud-Institut in Bern, welche er 2011 erfolgreich abschloss. Im Jahre 2014 erlangte Herr Homan den Facharzttitel für Psychiatrie und Psychotherapie FMH. Seit Juli 2019 ist Herr Homan als Oberarzt an der KPPP der PUK Zürich tätig.



Persönlichkeit

Herr Homan hat in den Jahren seiner Tätigkeit als Arzt und Forscher fundierte Fachkenntnisse über das gesamte Spektrum der klinischen Psychiatrie und Psychotherapie erworben. Er zeichnet sich durch ein hohes Talent in der Führung von interdisziplinären Teams sowie in der spezialisierten psychotherapeutischen und pharmakologischen Behandlung von schwer kranken psychiatrischen Patienten aus. Herr Homan wird von den Patienten für seine Empathie und Kompetenz geschätzt, und ist auch ein allerseits geschätzter Kollege, der sich nebst seiner Fachkompetenz auch durch seine Hilfsbereitschaft und Sozialkompetenz und einen herausragenden Einsatz für die Lehre und Forschung auszeichnet. Ein wichtiges Anliegen ist für Herrn Homan die ärztliche Nachwuchsförderung, welche er durch seine Funktion als Arbeitsgruppenleiter an der PUK Zürich mit grossem Engagement unterstützt.

Wissenschaftliche Qualifikation

Herr Homan legt insgesamt 29 Originalpublikationen in erstklassigen wissenschaftlichen Fachzeitschriften vor, ausserdem drei Übersichtsarbeiten (Reviews), zwei Fallstudien (Case Reports) und zwei Editorials. Bei 15 der 29 Originalpublikationen war er Erstautor, bei vier davon Letztautor. Hervorzuheben ist, dass seine Arbeiten in hochrangigen Journalen publiziert worden sind, unter anderem in *Nature Neuroscience*, *JAMA Psychiatry*, *American Journal of Psychiatry*, *Biological Psychiatry* und *Neuropsychopharmacology*. Somit sind aus meiner Sicht die Anforderungen an die Publikationsfähigkeit für die Habilitation an der UZH ohne Zweifel erfüllt. Die wissenschaftliche Arbeit von Herrn Homan zeichnet sich einerseits durch sein Interesse in der Erforschung der neurobiologischen und psychologischen Mechanismen der Positivsymptomatik bei Schizophrenie aus; andererseits hat er sich mit der Frage der Möglichkeit einer personalisierten Medizin in der Psychiatrie beschäftigt und hierzu hochrangige Beiträge als Erstautor (*Nature Neuroscience*) und Letztautor (*JAMA Psychiatry*) vorgelegt.

Durch die in der hier vorgelegten kumulativen Habilitationsschrift beschriebenen Studienergebnisse konnte Herr Homan neuartige Erkenntnisse in Bezug auf die Möglichkeiten und Grenzen einer individualisierten Psychiatrie aufzeigen. Diese Erkenntnisse könnten im Verlauf der nächsten Jahre in klinischen Populationen eingesetzt und überprüft werden. Damit leistet Herr Homan einen wichtigen Beitrag zum Verständnis von neurobiologischen diagnostischen und prognostischen Biomarkern.

Insgesamt zeichnet sich die wissenschaftliche Tätigkeit von Herrn Homan durch Originalität und einen methodisch hohen Anspruch aus. Er besitzt ausgeprägtes Talent in der Anwendung von verschiedenen methodologischen Ansätzen, sowie höchste wissenschaftliche Kompetenz in der Planung und Leitung von wissenschaftlichen Projekten.

Die von Herrn Homan geleiteten Forschungsprojekte wurden durch verschiedene nationale und internationale Stiftungen finanziell gefördert. Zudem ist er Homan als Gutachter (Peer reviewer) für zahlreiche internationale Journale tätig, unter anderem *JAMA Psychiatry*, *Current Biology* und *Neuroimage*.

Didaktik und Lehre

Herr Homan engagiert sich seit dem Beginn seiner Karriere konstant und erfolgreich in der neurowissenschaftlichen und psychiatrischen Lehre, und hat gleich bei Stellenantritt an der Universität Zürich zahlreiche Vorlesungen und Seminare übernommen, weil ihm die Lehre besonders wichtig ist. Er vermittelt Medizinstudenten die biowissenschaftlichen Grundlagen in der psychiatrischen Forschung allgemein und zu Psychose und Positivsymptomatik im Besonderen. Im Rahmen der klinisch-psychiatrischen Lehre ist er Lehrdozent für den klinischen Kurs Psychiatrie, in welchem Medizinstudenten die Erhebung und Interpretation des psychopathologischen Befundes am psychiatrischen Patienten vermittelt werden. Zudem hält Herr Homan Vorlesungen zur Pathophysiologie und Klinik von psychiatrischen Erkrankungen. Herr Homan engagiert sich ausserdem in der Ausbildung von Medizinstudierenden und an der Weiterbildung von Assistenzärzten. Herr Homan hat ausserdem mehrere Kurse für Didaktik besucht und erfüllt damit auch alle Voraussetzungen für erfolgreiche Lehre.

Aus meiner Sicht erfüllt Herr Homan damit aufgrund seiner wissenschaftlichen und didaktischen Leistungen ganz klar die Voraussetzungen für die Venia Legendi an der MeF der UZH; er ist für die Forschung und Lehre hervorragend geeignet.

Entwicklungspotential

Herr Homan ist ein hervorragender Psychiater und klinischer Wissenschaftler mit beeindruckendem Leistungsausweis und einer gewinnenden, loyalen, kollegialen und leistungsorientierten Persönlichkeit. Er hat sich national und international stetig weiterentwickelt und bereits mehrere Stufen seiner klinischen und wissenschaftlichen Karriere absolviert. Seine Forschungsthemen und Projekte hat er sich über viele Jahre selbstständig erarbeitet und ist national und international gut vernetzt, was er mit zahlreichen Kooperationen im Sinne des Standortes PUK Zürich nützt. Herr Homan vertritt mit seinem translationalen und methodisch vielfältigen Schwerpunkt in der Erforschung Psychosen einen unverzichtbaren Bereich der klinischen Neurowissenschaften und der Psychiatrie.

Aus all den genannten Gründen kann ich aus voller Überzeugung bestätigen, dass Herr Homan aufgrund seiner wissenschaftlichen, klinischen und didaktischen Leistungen sowie seiner Persönlichkeit bestens geeignet ist und alle Voraussetzungen für die Venia Legendi an der MeF der UZH erfüllt.

Antrag

Als Fachvertreter unterstütze ich den Antrag von Herrn Homan zur Habilitation vollumfänglich und ohne Vorbehalt.

Für Rückfragen stehe ich gerne zur Verfügung und verbleibe mit freundlichen Grüßen


Prof. Dr. med. Erich Seifritz
Direktor KPPP



Philipp Homan, MD, PhD

Personal information

Address	University Hospital of Psychiatry Lenggstrasse 31 8032 Zurich
ORCID	0000-0001-9034-148X
Email	philipp.homan@bli.uzh.ch
Mobile	+41 79 379 60 97
Twitter	@philipphoman
Github	https://github.com/philipphoman
OSF	https://osf.io/cb8wp

Education

11/2015	Board certification (Facharzt). Psychiatry and Psychotherapy, Swiss Medical Association (FMH)
04/2013	PhD, Neuroscience (Dr. phil.). University of Bern, Switzerland. Advisor: Thomas Dierks, MD
07/2008	MD (Dr. med. univ.). Medical University of Vienna, Austria. Thesis defense: 6/4/2008. Advisor: Michael Hubenstorf, MD, PhD
10/1999 - 06/2002	Study of business and computer science. Technical University of Vienna, Austria

Employment history

07/2019 - present	Attending physician and group leader. University Hospital of Psychiatry, University of Zurich, Zurich, Switzerland.
08/2017 - present	Assistant Professor. Feinstein Institute for Medical Research and Zucker School of Medicine, New York, NY, USA.
08/2015 - 07/2017	Postdoctoral Research Fellow. Schiller Laboratory of Affective Neuroscience (Head: Daniela Schiller) at Friedman Brain Institute (Head: Eric Nestler), Icahn School of Medicine at Mount Sinai, New York, NY, USA. Funded by Swiss National Science Foundation (SNF 161077). Advisor: Daniela Schiller, PhD
07/2013 - 07/2015	Group leader. Non-invasive Brain Stimulation Unit, Translational Research Center at the University Hospital of Psychiatry, University of Bern, Switzerland. Advisor: Thomas Dierks, MD
09/2013 - 07/2015	Attending physician (Oberarzt). University Hospital of Psychiatry, University of Bern, Switzerland. Advisor: Werner Strik, MD
09/2008 - 08/2013	Resident. University Hospital of Psychiatry and Department of Endocrinology, Diabetology & Clinical Nutrition, Inselspital, University of Bern, Switzerland. Advisors: Werner Strik, MD and Peter Diem, MD

Supervision of junior researchers

- Stephanie Winkelbeiner, PhD, postdoc
- Marialuisa Cavelti, PhD, postdoc
- Samir Suker, MD, psychiatry resident
- Katharina Kunzelmann, PhD, graduate student
- Taylor Marzuk, MSc, research assistant
- Qi Lin, MSc, research assistant

- Jingchu Hu, PhD, graduate student

Teaching activities

- 01/2019** Neuroscience curriculum for Psychiatry residents. Zucker Hillside Hospital
- 06/2016** TMS training course for physicians and PhD-students. Graduate School of Health Sciene, University of Bern, Switzerland
- 06/2016** General psychopathology. Seminar, University of Bern, Switzerland
- 04/2015** TMS training course for physicians. University Hospital of Psychiatry Bern, Switzerland
- 10/2014 - 01/2015** General psychopathology. Seminar, University of Bern, Switzerland
- 02/2014 - 06/2014** General psychopathology. Seminar, University of Bern, Switzerland
- 03/2014** Training course on psychopathology. University Hospital of Psychiatry Bern, Switzerland
- 04/2014** Neuroimaging of higher brain functions. Lecture, University of Bern, Switzerland
- 05/2014** Training course on major depression. University Hospital of Psychiatry Bern, Switzerland
- 03/2013 - 06/2013** Problem Based Learning (PBL). Seminar, University of Bern, Switzerland

Scientific reviewing

- Adhoc reviewer** Current Biology; JAMA Psychiatry; Schizophrenia Bulletin; Bioinformatics; Psychoneuroendocrinology; eNeuro; Human Brain Mapping; NeuroImage; NeuroImage:Clinical; Neuropsychologia; Translational Psychiatry; Psychophysiology; Neuropsychobiology; Psychiatry Research; Neuroimaging; Journal of Neuroscience Methods; World Journal of Biological Psychiatry; Frontiers in Psychiatry; Cognitive, Affective, and Behavioral Neuroscience; Brain Imaging and Behavior; Scientific Reports; Clinical and Experimental Pharmacology and Physiology; Computational Psychiatry; Journal of Psychopharmacology; Neuroscience; PLOS One; Psychiatry Research; Progress in Neuro-Psychopharmacology & Biological Psychiatry; BMC Psychiatry
- Review Editor** Frontiers in Psychiatry (Neuropsychiatric Imaging and Stimulation), Neuropsychobiology

Organisation of conferences

- 06/2016** Organized a 2-day training course in transcranial magnetic stimulation for physicians and PhD-students together with Dr. Yosuke Morishima. Graduate School of Health Sciene, University of Bern, Switzerland
- 04/2015** Organized a 4-week training course in transcranial magnetic stimulation for physicians. University Hospital of Psychiatry Bern, Switzerland

Prizes, awards, fellowships

- 03/2019** Young investigator award from the World Federation of Societies of Biological Psychiatry (WFSBP)
- 04/2018** Travel award from the Schizophrenia International Research Society, Florence, Italy
- 05/2016** Poster award from the Friedman Brain Institute at the 8th Annual Neurosciene Retreat, New York Academy of Medicine, New York

- 06/2015** Advanced Postdoc.Mobility fellowship from the Swiss National Science Foundation, Switzerland (SNF 161077)
11/2014 Hans-Heimann-Preis from the Deutsche Gesellschaft für Psychiatrie, Psychotherapie und Nervenheilkunde (DGPPN), Germany
09/2014 Frutiger Award from the Foundation Adrian and Simone Frutiger, Switzerland
11/2011 Poster award from the DGPPN at the DGPPN Congress, Berlin, Germany

Oral presentations

Conference talks

- American College of Neuropsychopharmacology (ACNP)
- Schizophrenia International Research Society (SIRS)
- Winter Conference for Brain Research (WCBR)
- Society of Biological Psychiatry (SOBP)
- World Federation of Societies of Biological Psychiatry (WFSBP)
- German Federation of Psychiatry and Psychotherapy (DGPPN)
- European Conference of Schizophrenia Research (ECSR)

Invited talks

- Laureate Institute for Brain Research, Tulsa, Oklahoma
- National Institute of Mental Health (NIMH)
- IBM Watson, York Town Heights, New York
- University of Geneva
- Mount Sinai School of Medicine, New York
- Zucker Hillside Hospital, New York
- Medical University of Vienna
- University Hospital of Zurich
- University of Toronto



NOTIFICATION
on Conferment an Academic Degree

D I C T U M

Pursuant to § 87 para 1 i.c.w. § 124 para 1 University Act 2002 [Fed. Act I N°. 120/2002 i.i.p.f.] and Z 4 of annex 1 to the University Studies Act [Fed. Act I Nr. 48/1997 a.l.a.], the Curriculumsdirektorin for the diploma programme Medicine at the Medizinische Universität Wien herewith confers upon

Mr. Philipp Homan,
born 13 February 1980, registration number 9925118,
citizenship Austria,

the academic degree of
Doktor der gesamten Heilkunde (Dr.med.univ.)

G R O U N D S

Upon completion of the diploma programme Medicine following positive assessment of all curricular exams legally required for the diploma programme Medicine (notification bulletin of the Universität Wien, piece XXX, No. 308 dated 24 July 2002, i.i.p.f.), the pre-requisites for conferment of the academic degree of "Doktor der gesamten Heilkunde" have been fulfilled on 01 July 2008.
Hence the decision was in accordance with the ruling.

I N F O R M A T I O N O N T H E R I G H T T O A P P E A L

§ 25 para 1 Z 12 University Act 2002 provides the legal right to appeal against this notification to the Senate of the Medizinische Universität Wien. The appeal has to be filed within two weeks after delivery to the responsible Curriculumsdirektorin of the Medizinische Universität Wien (c/o Studien- und Prüfungsabteilung der Medizinischen Universität Wien) with reference to the notification and enclosure of a well-founded petition.

Vienna, this 24 July 2008

Die Curriculumsdirektorin
Univ.Prof. Dr.med.univ. Anita Rieder

UNIVERSITÄT BERN

The Graduate School for Health Sciences

hereby certifies that

Philipp Homan

from Austria, born on February 13, 1980

having submitted the thesis

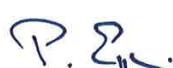
On the neurobiology of hallucinations

and having passed the thesis defence, is awarded the degree of

PhD in Health Sciences (Neurosciences)

with the mark **summa cum laude**

Bern, April 29, 2013



The Dean of the
Faculty of Medicine


The Dean of the
Faculty of Human Sciences
Universität Bern, the Rector

List of publications

Philipp Homan, MD, PhD*

1 Original investigations¹

1. **Homan P**, Argyelan M, Fales CL, DeRosse P, Szeszko PR, Robinson DG, Lencz T, Malhotra AK. Striatal volume and functional connectivity correlate with weight gain in early-phase psychosis. *Neuropsychopharmacology*, 2019. doi: 10.1038/s41386-019-0464-y. IF: 7.2.
2. Winkelbeiner S, Leucht S, Kane JM, **Homan P**. Evaluation of differences in individual treatment response in schizophrenia spectrum disorders. *JAMA Psychiatry*, 2019. doi: 10.1001/jamapsychiatry.2019.1530. IF: 15.9.
3. **Homan P**, Argyelan M, DeRosse P, Szeszko P, Gallego JA, Hanna L, Robinson D, Kane JM, Lencz T, Malhotra AK. Structural similarity networks predict clinical outcome in early-phase psychosis. *Neuropsychopharmacology*, 2019. <https://dx.doi.org/10.1038/s41386-019-0322-y>. IF: 7.2.
4. **Homan P**, Levy I, Feltham E, Gordon C, Hu J, Li J, Pietrzak RH, Southwick S, Krystal JH, Harpaz-Rotem I, Schiller D. Neural computations of threat in the aftermath of combat trauma. *Nature Neuroscience*, 2019. <https://dx.doi.org/10.1038/s41593-018-0315-x>. IF: 21.1.
5. Gaillard C, Guillod M, Ernst M, Torrisi S, Federspiel A, Schoebi D, Recabarren R, Ouyang X, Mueller-Pfeiffer C, Horsch A, **Homan P**, Wiest R, Hasler G, Martin-Soelch C. Striatal responsiveness to reward under threat-of-shock and working memory load: A preliminary study. *Brain and Behavior*. IF: 2.1.
6. Cavelti M, Winkelbeiner S, Federspiel A, Walther S, Stegmayer K, Giezendanner S, Laimboeck K, Strik W, Horn H, **Homan P**. Formal thought disorder in schizophrenia and white matter abnormalities: A tract-based spatial statistics analysis. *Psychiatry Research: Neuroimaging*, (279): 40–50, 2018. <https://dx.doi.org/10.1016/j.pscychresns.2018.05.011>. IF: 2.3.
7. Winkelbeiner S, Cavelti M, Federspiel A, Dierks T, Strik W, Horn H, **Homan P**. Decreased blood flow in the right insula and middle temporal gyrus predicts negative formal thought disorder in schizophrenia. *Schizophrenia Research*, 201: 432–434, 2018. <https://dx.doi.org/10.1016/j.schres.2018.06.009>. IF: 4.6.
8. Kunzelmann K, Grieder M, van Swam C, **Homan P**, Hubl D, Dierks T. Am I hallucinating or is my fusiform cortex activated? Functional activation differences in schizophrenia patients with and without hallucinations. *The European Journal of Psychiatry*, 33: 1–7, 2019. <https://dx.doi.org/10.1016/j.ejpsy.2018.06.002>. IF: 0.7.
9. Viviano JD, Buchanan RW, Calarco N, Gold JM, Foussias G, Bhagwat N, Stefanik L, Hawco C, DeRosse P, Argyelan M, Turner J, Chavez S, Kochunov P, Kingsley P, Zhou X, Malhotra AK, Voineskos AN, Carpenter W, Zaranski J, Arbach E, August S, Remington G, Dickie E, Kwan J, Plagiannakos C, Mason M, Boczułak M, Miranda D, **Homan P**, DeRosse P, Iacoboni M, Green M. Resting-state connectivity biomarkers of cognitive

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¹Equal contributions are marked with #.

performance and social function in individuals with schizophrenia spectrum disorder and healthy control subjects. *Biological Psychiatry*, 84(9): 665–674, 2018. <https://dx.doi.org/10.1016/j.biopsych.2018.03.013>. IF: 11.5.

10. Cavelti M, Kircher T, Nagels A, Strik W, Horn H, **Homan P**. Neuroimaging of formal thought disorder in schizophrenia: A systematic review. *Schizophrenia Research*, 199: 2–16, 2018. <https://dx.doi.org/10.1016/j.schres.2018.02.051>. IF: 4.6.
11. Hu J, Wang W, **Homan P**, Wang P, Zheng X, Schiller D. Reminder duration determines threat memory modification in humans. *Scientific Reports*, 8: 8848, 2018. <https://dx.doi.org/10.1038/s41598-018-27252-0>. IF: 4.0.
12. **Homan P**, Lin Q, Murrough JW, Soleimani L, Bach DR, Clem RL, Schiller D. Prazosin during threat discrimination boosts memory of the safe stimulus. *Learning & Memory*, 24(11): 597–601, 2017. <https://dx.doi.org/10.1101/lm.045898.117>. IF: 2.4.
13. **Homan P**, Ely BA, Yuan M, Brosch T, Ng J, Trope Y, Schiller D. Aversive smell associations shape social judgment. *Neurobiology of Learning and Memory*, 144: 86–95, 2017. <https://dx.doi.org/10.1016/j.nlm.2017.07.004>. IF: 3.0.
14. **Homan P**, Reddan MC, Brosch T, Koenigsberg HW, Schiller D. Aberrant link between empathy and social attribution style in borderline personality disorder. *Journal of Psychiatric Research*, 94: 163–171, 2017. <https://dx.doi.org/10.1016/j.jpsychires.2017.07.012>. IF: 3.9.
15. Cavelti M, **Homan P**, Vauth R. The impact of thought disorder on therapeutic alliance and personal recovery in schizophrenia and schizoaffective disorder: An exploratory study. *Psychiatry Research*, 239: 92–98, 2016. <https://dx.doi.org/10.1016/j.psychres.2016.02.070>. IF: 2.2.
16. Kindler J, Jann K, **Homan P**, Hauf M, Walther S, Strik W, Dierks T, Hubl D. Static and dynamic characteristics of cerebral blood flow during the resting state in schizophrenia. *Schizophrenia Bulletin*, 41: 163–70, 2015. <https://dx.doi.org/10.1093/schbul/sbt180>. IF: 7.3.
17. **Homan P**, Grob S, Milos G, Schnyder U, Eckert A, Lang U, Hasler G. The role of BDNF, leptin, and catecholamines in reward learning in bulimia nervosa. *International Journal of Neuropsychopharmacology*, 18(5), 2015. <https://dx.doi.org/10.1093/ijnp/pyu092>. IF: 4.2.
18. **Homan P**, Neumeister A, Nugent A, Charney D, Drevets WC, Hasler G. Serotonin versus catecholamine deficiency: behavioral and neural effects of experimental depletion in depression. *Translational Psychiatry*, 5: e532, 2015. <https://dx.doi.org/10.1038/tp.2015.25>. IF: 5.2.
19. **Homan P**#, Vermathen P#, Van Swam C, Federspiel A, Boesch C, Strik W, Dierks T, Hubl D, Kreis R. Magnetic resonance spectroscopy investigations of functionally defined language areas in schizophrenia patients with and without auditory hallucinations. *Neuroimage*, 94: 23–32, 2014. <https://dx.doi.org/10.1016/j.neuroimage.2014.03.009>. IF: 5.8.
20. **Homan P**, Drevets WC, Hasler G. The effects of catecholamine depletion on the neural response to fearful faces in remitted depression. *International Journal of Neuropsychopharmacology*, 17(9): 1419–28, 2014. <https://dx.doi.org/10.1017/s1461145714000339>. IF: 4.2.
21. **Homan P**, Drevets WC, Hasler G. Neural correlates of free t3 alteration after catecholamine depletion in subjects with remitted major depressive disorder and in controls. *Psychopharmacology (Berl)*, 231(2): 409–17, 2014. <https://dx.doi.org/10.1007/s00213-013-3250-2>. IF: 3.4.

22. Woodward T, Jung K, Hwang H, Yin J, Taylor L, Menon M, Peters E, Kuipers E, Waters F, Lecomte T, Sommer I, Daalman K, van Lutterveld R, Hubl D, Kindler J, **Homan P**, Badcock J, Chhabra S, Cella M, Keedy S, Allen P, Mechelli A, Preti A, Siddi S, Erickson D. Symptom dimensions of the psychotic symptom satiating scales in psychosis: A multi-site study. *Schizophrenia Bulletin*, 40 Suppl 4: S265–74, 2014. <https://dx.doi.org/10.1093/schbul/sbu014>. IF: 7.3.
23. **Homan P**, Drevets WC, Hasler G. Growth hormone response to catecholamine depletion in unmedicated remitted subjects with major depressive disorder and healthy controls. *Journal of Clinical Psychopharmacology*, 33: 621–626, 2013. <https://dx.doi.org/10.1097/JCP.0b013e31829a8284>. IF: 3.0.
24. **Homan P**, Grob S, Milos G, Schnyder U, Hasler G. Reduction in total plasma ghrelin levels following catecholamine depletion. Relation to bulimic and depressive symptoms. *Psychoneuroendocrinology*, 38: 1545–1552, 2013. <https://dx.doi.org/10.1016/j.psyneuen.2012.12.024>. IF: 4.0.
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Datum: 20.8.2019

Unterschrift:

17.05.2017/rweh

Seite 1/1



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Anrechnung von Weiterbildungen in der Hochschuldidaktik für die Habilitation

Sehr geehrter Herr Dr. Homan

Vielen Dank für die Information zum Nachweis einer Weiterbildung im Bereich der Hochschuldidaktik, die Sie uns zugestellt haben.

Folgende Weiterbildungen wurden von Ihnen besucht:

- Zucker Hillside Hospital Northwell Health, Weiterbildung «Effectively Presenting Neuroscience», 03/2018-06/2018 (8 Stunden)
Kursleitung: Dr. John Young und Dr. David Ross
- Universitäre Psychiatrische Dienste Bern (UPD), Weiterbildungskurs «Recovery praktisch – paritäre Einführung und Schulung», 23./29.08.2013, 7./15.11.2013 und 13./21.02.2014 (6 Tage)
Kursleitung: René Hadorn und Sandra Gelormini
- Universitäre Psychiatrische Dienste Bern (UPD), Mitarbeit an der Konzeption und dem Aufbau des Recovery Trialogs Bern, 2012-2013 (20 Stunden)

Die oben aufgeführten Weiterbildungen erfüllen die Anforderung der Didaktik Ausbildung für die Habilitation und können angerechnet werden.

Wir wünschen Ihnen weiterhin alles Gute für Ihre berufliche Laufbahn.

Freundliche Grüsse

Olga Zalesko B.A.
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Abstract

More than any other field in medicine, psychiatry has faced major theoretical and practical challenges – challenges which most likely reflect the complexity of both the brain and the environment. Individual differences between patients is one aspect of this complexity. Patients with the same diagnosis and with exposition to a similar environment may vary considerably in the way they process information on the algorithmic and the neural level. The detection of such differences may benefit from computational models that reduce the high dimensional data to theoretically meaningful parameters which may reflect individual correlates of clinical symptoms. Whether such differences translate directly into differences in response to treatment is an open question. Our own evaluation of individual differences in treatment response suggested that we tend to overestimate the heterogeneity in treatment effects, simply because a clear distinction of the relevant variance components is not possible from common randomized controlled trials. Elaborated study designs such as N-of-1 trials or advanced statistical modeling is needed to estimate the personal element of treatment response or side effects. Together, this suggests that a realistic evaluation of the need for precision medicine in psychiatry requires complex study designs together with computational modeling and advanced statistical techniques.

Habilitationsschrift

**Individual differences and
the scope of precision medicine in psychiatry**

Zur Erlangung der Venia Legendi der Universität Zürich

Verfasst von
Philipp Homan

Zürich, 20. August 2019

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1 Abstract

More than any other field in medicine, psychiatry has faced major theoretical and practical challenges – challenges which most likely reflect the complexity of both the brain and the environment. Individual differences between patients is one aspect of this complexity. Patients with the same diagnosis and with exposition to a similar environment may vary considerably in the way they process information on the algorithmic and the neural level. The detection of such differences may benefit from computational models that reduce the high dimensional data to theoretically meaningful parameters which may reflect individual correlates of clinical symptoms. Whether such differences translate directly into differences in response to treatment is an open question. Our own evaluation of individual differences in treatment response suggested that we tend to overestimate the heterogeneity in treatment effects, simply because a clear distinction of the relevant variance components is not possible from common randomized controlled trials. Elaborated study designs such as N-of-1 trials or advanced statistical modeling is needed to estimate the personal element of treatment response or side effects. Together, this suggests that a realistic evaluation of the need for precision medicine in psychiatry requires complex study designs together with computational modeling and advanced statistical techniques.

2 Introduction

2.1 Individual differences: the challenge of psychiatry

2.1.1 Views from inside the field

More than any other field in medicine, psychiatry is still facing major theoretical and practical challenges. These are acknowledged by leaders in the field. The theoretical challenges include pathogenesis and aetiology while the practical challenges include classification and diagnosis as well as prognosis and treatment (Stephan *et al.*, 2016a,b). What makes diagnoses a major problem is that they are still primarily based on subjective symptoms that lack objective biological correlates. The idea that mental disorders are brain disorders is already more than 150 years old (Griesinger, 1867) and is being questioned regularly (Borsboom, 2017; Fried *et al.*, 2017; Borsboom *et al.*, 2018); and indeed, patients' self reports are still the major source for diagnosing mental disorders. One reason why it has been difficult to confirm the hypothesis that mental disorders must have a neural basis might be that patients with the same diagnosis may have very different underlying neurobiology. In other words, a symptom-based diagnosis might not always map onto the same neurobiological disease mechanism. This means that the underlying biological mechanisms may vary between patients reporting the same symptoms, and a purely symptom-based diagnosis might blur biological differences and for the same reason might also lack the ability to guide treatment decisions. Thus, such diagnosis lacks biological validity and prognostic utility (Stephan *et al.*, 2016a; Insel, 2014; Schumann *et al.*, 2014; Krystal and State, 2014; Owen, 2014; Kapur *et al.*, 2012). It is therefore hardly surprising that developing new pharmacological treatments based on these diagnoses without a clear neural underpinning and novel treatment targets has largely been unsuccessful (Stephan *et al.*, 2016a; Sarter and Tricklebank, 2012).

More than any other field of medicine, psychiatry has also been subject to strong criticism from within and outside the field. One example from within the field was the recently published statement of the former director of the National Institute of Mental Health (NIMH), Thomas Insel. Given that one attempt to address the theoretical challenge of symptom-based diagnoses had been the transdiagnostic Research Domain Criteria introduced by NIMH under his leadership, a leadership that was meant to promote neurobiological research across diagnostic boundaries (Insel, 2013, 2014; Insel and Cuthbert, 2015), the lack of breakthroughs that have translated into clinical practice might have been the reason why Insel has recently voiced the following self-criticism: "I spent 13 years at NIMH really pushing on the neuroscience and genetics of mental disorders, and when I look back on that I realize that while I succeeded at getting lots of really cool papers published by cool scientists at fairly large costs – I think \$20 billion – I don't think we moved the needle in reducing suicide, reducing

hospitalizations, improving recovery for the tens of millions of people who have mental illness. I hold myself accountable for that (Rogers, 2017)."

2.1.2 Views from outside the field

In addition to comments on the challenges of psychiatry from within the field of psychiatry, there have been comments from outside the field as well. They often have similar objections against current practices in clinical psychiatry and psychiatric research. One important example was a widely discussed essay in the New York Review of Books by the former editor of the New England Journal of Medicine – one of the most prestigious scientific journals in the whole field of medicine. The essay included several arguments against the current state of affairs in psychiatry and deserves a literal quote since it recapitulates recurrent themes in the criticism of psychiatry. It argued that publication bias, industry sponsorship, and questionable withdrawal studies are the main reason why psychiatric drug efficacy should be contested: "The industry-sponsored studies usually cited to support psychoactive drugs – and they are the ones that are selectively published – tend to be short-term, designed to favor the drug, and show benefits so small that they are unlikely to outweigh the long-term harms. The problem with relapse studies ... is that they don't distinguish between a true relapse and withdrawal symptoms that result from the abrupt cessation of drugs (Angell, 2011c)." According to this view, efficacy of psychoactive drugs is overstated while the potential harm they cause is suppressed.

Similar criticism have been stated regularly in recent years (Angell, 2011a,b; Gøtzsche, 2014; Harrington, 2019; Korducki, 2019; Moncrieff and Kirsch, 2005; Frances and Widiger, 2012; Frances, 2012; Hengartner and Moncrieff, 2018; Moncrieff, 2018; Braillon *et al.*, 2019), and the subsequent exchanges between critiques and defenders of psychiatry are often redundant in nature (Angell, 2011c; Nutt *et al.*, 2014). Nevertheless, it might be useful to summarize the main objections, as in a book by Peter Gotzsche, the former director of the Nordic Cochrane Centre (Gøtzsche, 2015). The author concludes that psychiatric diagnoses are unreliable; biological explanations do not reduce but increase the stigma against mental illness; monoamine and dopamine hypotheses do not explain depression and psychosis; long term psychopharmacological treatment does not prevent recurrences of the illnesses; antidepressants may cause severe withdrawal symptoms; schizophrenia and other illnesses do not lead to brain damage; and psychoactive drugs do not prevent but cause brain damage. While these objections may sound exaggerated they have indeed been used in these or similar phrasing by critics of current psychiatric practice. Questioning not only commonly identified problems such as diagnosis and classification, they also raise doubt about biological correlates of mental illnesses such as depression (Homan *et al.*, 2015) and schizophrenia (Kapur and Seeman, 2001).

One could add another problem, not mentioned in this list, and not specific to psychiatry research but biomedical research in general: replication of scientific research findings. Not only in psychology do findings often not replicate (Open Science Collaboration, 2015), the same is true for major neurobiological findings in psychiatry as well (Drysdale *et al.*, 2016; Dinga *et al.*, 2018). In summary, respected professionals from within and outside the field have agreed that the major breakthroughs for patients based on a mainly neurobiological research agenda have not happened yet. One reason that they have not happened yet might be that the true appreciation of individual differences between patients was potentially more often claimed rather than actually considered. It might also be true that the consideration of individual differences is harder than initially believed. In the current work we try to summarize our own attempts to shed some light on such individual differences and to thereby gauge the scope for more precision in psychiatry.

2.1.3 Appreciating individual differences between patients

That patients differ from one another is by no means a controversial position to hold. On the contrary, the assumption is widely shared that much of what is done in medicine and psychiatry does not work, simply because it is based on average treatment effects; and that medicine should shift its focus on individual, not average, effects of treatment. A recent editorial in *Nature* summarized this accordingly: “Every day, millions of people are taking medications that will not help them. The top ten highest-grossing drugs in the United States help between 1 in 25 and 1 in 4 of the people who take them ... For some drugs, such as statins – routinely used to lower cholesterol – as few as 1 in 50 may benefit. There are even drugs that are harmful to certain ethnic groups because of the bias towards white Western participants in classical clinical trials (Schork, 2015).”

The problem is that while it might be true that different disease mechanisms may underly the same diagnoses, it is not clear whether this automatically translates into individual differences in how patients respond to the same treatment. Indeed, rather than disease mechanisms it is not unlikely that part of what has driven the attempt to personalize drug treatment is the sobering perspective that currently there are no new drugs for serious mental illness on the horizon. It follows that for the near future, psychiatry will likely have to live with the treatments that have already been established and have been shown to be as effective as treatments in general medicine (Leucht *et al.*, 2012). Although critical voices have often disagreed with the efficacy (Götzsche, 2014; Kirsch *et al.*, 2008; Moncrieff and Kirsch, 2005), tolerability (Davies *et al.*, 2019), and necessity (Wunderink *et al.*, 2013) of pharmacological treatment (especially with respect to antidepressants (Munkholm *et al.*, 2019)), clinical psychiatry will have to keep offering pharmacological treatment to patients with severe mental disorders in

the near future, and the drugs will likely be the same as they have been.

To address the rather sobering aspect of drug development one idea has been that the available treatments could at least be tailored to the individual (Schork, 2015). As we mentioned above, the rationale behind this idea is the assumption that there are individual differences between patients in terms of neurobiological disease mechanisms (Brodersen *et al.*, 2013; Deserno *et al.*, 2012; Demjaha *et al.*, 2012, 2014; Howes, 2010; Howes *et al.*, 2012; Stephan *et al.*, 2016a; Drysdale *et al.*, 2016) that require a corresponding adjustment of the pharmacological or somatic treatment in question. Importantly, such individual differences may be apparent already at the high risk state, before conversion to psychosis has occurred (Schmidt *et al.*, 2013; Schmidt and Borgwardt, 2013; Schmidt *et al.*, 2014; Koutsouleris *et al.*, 2014). For example, if psychosis patients who have been classified as treatment-resistant do indeed differ neurobiologically (in terms of a relatively undisturbed dopaminergic neurotransmission compared to controls) from patients that respond well to antipsychotic treatment (Vanes *et al.*, 2018), then antipsychotics that are mainly aimed at blocking the dopamine D2 receptor might not be the right treatment for patients who show this alteration even before starting any treatment.

Computational modeling has been proposed as one promising attempt to dissect individual differences in disease mechanisms (Adams *et al.*, 2016; Huys *et al.*, 2016; Stephan *et al.*, 2017b,a). In addition, a more data driven machine learning approach might complement theory driven computational models and improve classification of diseases and prediction of clinical outcome (Huys *et al.*, 2016). Thus, computational modeling and machine learning, sometimes summarized under the umbrella term 'computational psychiatry' (Huys *et al.*, 2016), have been proposed as part of the solution for the current challenges in psychiatry.

2.2 Individual differences in clinical symptoms

Differences between patients with the same diagnosis have often been highlighted (Fisher *et al.*, 2018) and are readily apparent to clinicians and researchers alike. For example, why is it that only some individuals who have been exposed to similar traumatic events later develop posttraumatic stress disorder? In a series of studies, we assessed different aspects of associative learning using computational and neuroimaging methods in healthy controls and in patients (Homan *et al.*, 2017a,b,c, 2018, 2019c). We showed that a specific learning component, prediction error weight, may explain individual differences in combat veterans. Indeed, highly symptomatic veterans assigned more weight to prediction errors, suggesting that they exaggerated their attention adjustment to unreliable stimuli. This finding was intriguing because it was based on a more fine-grained computational analysis that a conventional summary statistic approach might have easily missed (Daw, 2011; Wilson and Collins, 2019). It suggests that in

order to capture individual differences in patients' behavior one has to apply computational tools that exploit prior knowledge on learning behavior together with Bayesian statistics (Homan *et al.*, 2019c).

2.3 Individual differences in treatment response

Thus, individual differences in learning that map onto psychopathology are detectable with computational tools. Ultimately, these differences could inform treatment decisions if it can be shown that they also map onto differences in response to treatment. Although we have shown that these differences in treatment response require careful assumptions (Winkelbeiner *et al.*, 2019), previous work suggests that such differences (although detected here with more conventional imaging methods) may indeed correlate with treatment outcome. Given that brain stimulation has shown some promise in the treatment of persistent positive symptoms (Homan *et al.*, 2011; Brunelin *et al.*, 2012; Slotema *et al.*, 2010; Lefaucheur *et al.*, 2014), we investigated psychosis patients with persistent positive symptoms (Homan *et al.*, 2012b, 2013, 2014; Kindler *et al.*, 2015) and showed that individuals who would later respond well to a treatment with transcranial magnetic stimulation had a higher regional blood flow in the target area in the left temporal cortex (Homan *et al.*, 2012a). Notably, we also showed that patients stimulated at this area had a decrease of regional blood flow after treatment that was correlated with symptom improvement (Kindler *et al.*, 2013a,b).

Given that positive symptoms also include the understudied domain of thought disturbance, we aimed at characterizing this syndrome more comprehensively in terms of brain structure and function (Cavelti *et al.*, 2018a,b; Winkelbeiner *et al.*, 2018) because of the potential impact of thought disorder on real life outcome including recovery (Cavelti *et al.*, 2016). Moreover, we have also shown that thought disturbance is the syndrome of which we can predict the improvement during antipsychotic treatment using baseline imaging predictors (Homan *et al.*, 2019a). Importantly, we characterized individual treatment response using a comprehensive approach with mixed models which allowed us to separate random variation from actual treatment variation in each participant (Senn, 2016; Hecksteden *et al.*, 2018). In addition, we could estimate individual treatment response more efficiently by using all available data and more conservatively by applying shrinkage (Hedeker and Gibbons, 2006; Gelman *et al.*, 2013). Using this method, we showed that individual differences in structural brain similarity predicted treatment response in first episode psychosis (Homan *et al.*, 2019a), suggesting that these networks capture biologically and clinically meaningful correlates of psychosis.

2.4 Individual differences in side effects

Psychotic disorders are prevalent in about 3% of the population and add to both morbidity and mortality (Olfson *et al.*, 2015). They involve positive and negative symptoms as well as cognitive dysfunction (Howes and Murray, 2014). Antipsychotic drugs act on dopamine D2 receptors and are essential in the treatment of psychosis (McCutcheon *et al.*, 2019; Kapur and Seeman, 2001; Seeman and Lee, 1975; Seeman *et al.*, 1976). Clinical response, however, is variable and often poor. Indeed, 30-40% of patients with psychosis (Lindenmayer, 2000; Mortimer *et al.*, 2010) fail to respond to antipsychotics at recommended doses and duration – despite proper occupancy of dopamine D2 receptors (Wolkin and Barouche, 1989; Coppens *et al.*, 1991). It is thus essential that we improve outcome in those patients – particularly during their first illness episode, as relapses may delay response during the next episode (Takeuchi *et al.*, 2018). Importantly (and mirroring the heterogeneity of psychosis), there might be a different neurochemical deficit involved in cases of treatment-resistance. Alternatively, the dopaminergic dysfunction could be even higher in those cases, so that antipsychotic dopamine blockade is insufficient to improve symptoms. Importantly, clozapine has shown to be effective in such cases where other antipsychotics had failed (Kane *et al.*, 1988), highlighting that there does exist a treatment for such treatment-resistant psychoses. It is also known, however, that clozapine is under-utilized (Kane, 2012), which underscores the need of prospective biomarkers that may guide treatment (Homan *et al.*, 2019a). An important goal of current research is thus to prospectively test circuit-based markers of antipsychotic treatment response and to see if biomarker-informed treatment decisions may improve clinical outcome. This may allow to tailor treatments to subgroups of patients (Kapur *et al.*, 2012) and to identify novel treatment targets for the next generation of effective treatments (Insel, 2013).

Of similar importance, however, is the consideration of side effects of antipsychotic treatments. They add to the considerable burden of the illness and promote cardiovascular and metabolic diseases. The seminal framework by Davis *et al.* (1991) set the stage for considering striatal hyperdopaminergic neurotransmission as the key substrate of psychosis. Since then, numerous functional imaging studies have found corticostriatal abnormalities in psychosis. For example, an early PET study showed that positive symptoms were correlated with activity in the ventral striatum (Liddle and Wands, 1992). Studies using tasks during functional magnetic resonance imaging (fMRI) have found a correlation between reduced striatal activity and positive symptoms in medicated patients with psychosis (Juckel *et al.*, 2006; Menon *et al.*, 2011; Sorg *et al.*, 2013; Jensen *et al.*, 2008) and in first episode patients (Nielsen *et al.*, 2012a). In bipolar patients, the striatum has also shown decreased activation (Pavuluri *et al.*, 2011; Pompei *et al.*, 2011; Liu *et al.*, 2012; Yip *et al.*, 2015), although these studies also included non-

psychotic patients. More recent evidence comes from studies in chronic patients that examined striatal connectivity with limbic and prefrontal brain areas (Yoon *et al.*, 2012; Orliac *et al.*, 2013; Quidé *et al.*, 2013; Tu *et al.*, 2013; Khadka *et al.*, 2013; Mamah *et al.*, 2013), and our own group has expanded these examinations to patients with a first episode of psychosis (Sarpal *et al.*, 2015). Transcending mere striatal dysfunction, striatal dysconnectivity is thus a robust finding in psychosis. Notably, these findings are based on resting state fMRI, a measure that can be obtained relatively easily in a population such as first episode patients. Task-based fMRI, on the other hand, offers more experimental control over the measurement and the testing of specific mechanistic hypotheses with advanced computational modeling techniques (Homan *et al.*, 2019c). The trade-off is the challenge that task-based fMRI may impose on psychiatric patients. Nevertheless, a growing literature suggests that the striatum can indeed be studied with task-based fMRI in psychosis (Schlagenhauf *et al.*, 2009; Deserno *et al.*, 2012), and reinforcement learning is a reliable way to activate it.

Given the crucial role of the striatum in psychosis, it is reasonable to assume that effects of treatment should be reflected by striatal activity and connectivity. Support for this assumption comes from research in healthy controls and in patients. In controls, dopamine agonism increased whereas dopamine depletion decreased striatal connectivity with cortical targets (Cole *et al.*, 2013; Nagano-Saito *et al.*, 2008), supporting a correlation between dopamine receptor availability and frontostriatal connectivity (Ghahremani *et al.*, 2012). In psychosis patients, corticostratial activity and connectivity may reflect response to antipsychotic treatment. Indeed, striatal activation before and blood flow after antipsychotic treatment correlated with positive symptom improvement in schizophrenia (Lahti *et al.*, 2009; Nielsen *et al.*, 2012b). In addition, striatal activation increased after antipsychotic treatment in pediatric mania (Pavuluri *et al.*, 2012), supporting a cross-diagnostic effect. In first episode psychosis, our group found that baseline striatal connectivity distinguished responders from nonresponders (Sarpal *et al.*, 2016), a result that was replicated in an independent cohort of patients (Sarpal *et al.*, 2016). Striatal connectivity changes from baseline to follow-up also correlated with the improvement in positive symptoms in a subsample of those patients (Sarpal *et al.*, 2015) and in a similar study from a different group (Anticevic *et al.*, 2015). Together, these results suggest that striatal connectivity is a marker of treatment effects in psychosis.

Given that overall outcome is not only shaped by symptom improvement but also absence of severe side effects, we also studied one of the most common side effects in antipsychotic treatment, namely weight gain (Correll *et al.*, 2009). Focusing on the reward circuitry in the striatum, we showed that both striatal volume and frontostriatal connectivity predicted weight gain in first episode psychosis patients Homan *et al.* 2019b. This suggests that patients might not only benefit from the prediction of treatment response but also from the predic-

tion of severe side effects. Combining these two predictors may help calculating personalized risk-benefit profiles, where each patient's likelihood to respond is weighted by their likelihood to experience severe side effects.

3 Summary of included papers

3.1 Study 1: Homan et al. 2019a, *Neuropsychopharmacology*

In this study, we calculated structural similarity networks in each participant and hypothesized that the hubness, i.e., the number of edges connecting a node to the rest of the network, would be associated with clinical outcome. The study included 82 patients with early psychosis (defined as a cumulative life time exposure to antipsychotic treatment of less than 2 years) and 58 healthy controls. Medications were administered in a double-blind randomized manner, and patients were scanned at baseline prior to treatment with second-generation antipsychotics. Symptoms were assessed with the Brief Psychiatric Rating Scale at baseline and over the course of 12 weeks. Nodal degree of structural similarity networks was computed for each subject and entered as a predictor of individual treatment response into a partial least squares regression. We found that the first two partial least squares regression components explained 29% of the variance in treatment response after cross-validation. Nodes loading strongly on the first component were primarily located in the orbito- and prefrontal cortex, whereas nodes loading strongly on the second PLS component were primarily located in the superior temporal, precentral, and middle cingulate cortex. We interpreted these findings as suggesting link between brain network morphology and clinical outcome in early-phase psychosis.

3.2 Study 2: Homan et al. 2019b, *Neuropsychopharmacology*

This study was based on the clinical rationale that second-generation antipsychotic drugs are essential in the treatment of psychotic disorders while also being well-known for inducing substantial weight gain and obesity. Critically, weight gain may reduce life expectancy for up to 20-30 years in patients with psychotic disorders, and prognostic biomarkers are generally lacking. Even though other receptors are also implicated, the dorsal striatum, rich in dopamine D2 receptors, which are antagonized by antipsychotic medications, plays a key role in the human reward system and in appetite regulation, suggesting that altered dopamine activity in the striatal reward circuitry may be responsible for increased food craving and weight gain. We measured striatal volume and striatal resting-state

functional connectivity at baseline, and weight gain over the course of 12 weeks of antipsychotic treatment in 81 patients with early-phase psychosis. We also included a sample of 58 healthy controls. Weight measurements were completed at baseline, and then weekly for 4 weeks, and every 2 weeks until week 12. We used linear mixed models to compute individual weight gain trajectories. Striatal volume and whole-brain striatal connectivity were then calculated for each subject, and used to assess the relationship between striatal structure and function and individual weight gain in multiple regression models. Patients had similar baseline weights and body mass indices (BMI) compared with healthy controls. There was no evidence that prior drug exposure or duration of untreated psychosis correlated with baseline BMI. Higher left putamen volume and lower sensory motor connectivity correlated with the magnitude of weight gain in patients, and these effects multiplied when the structure-function interaction was considered in an additional exploratory analysis. Together, we concluded that the results provide evidence for a correlation of striatal structure and function with antipsychotic-induced weight gain. Lower striatal connectivity was associated with more weight gain, and this relationship was stronger for higher compared with lower left putamen volumes.

3.3 Study 3: Homan et al. 2019c, *Nature Neuroscience*

In this study we combined computational, morphological, and functional analyses to assess latent markers of associative threat learning with computational modeling combat veterans. We then estimated correlations of these markers with overt post-traumatic stress disorder. Using reversal learning, we found that symptomatic veterans showed greater physiological adjustment to cues that did not predict what they had expected, indicating greater sensitivity to prediction errors for negative outcomes. This exaggerated weighting of prediction errors shapes the dynamic learning rate (associability) and value of threat predictive cues. The degree to which the striatum tracked the associability partially mediated the positive correlation between prediction-error weights and PTSD symptoms, suggesting that both increased prediction-error weights and decreased striatal tracking of associability independently contribute to PTSD symptoms. Furthermore, decreased neural tracking of value in the amygdala, in addition to smaller amygdala volume, independently corresponded to higher PTSD symptom severity. We interpreted these results as providing evidence for distinct neurocomputational contributions to the symptoms of posttraumatic stress disorder.

3.4 Study 4: Winkelbeiner et al. 2019, *JAMA Psychiatry*

This study combined data from simulations and from a meta analysis of antipsychotic drug trials. We started out from the assumption that patients vary consid-

erably in their response to antipsychotic drugs in randomized controlled trials which is a widely shared belief. We assumed that a personal element of response should be reflected by a clinically relevant increase in overall variance in the treatment compared to the control group and analyzed 53 trials from the last 25 years resulting in 75 comparisons of antipsychotics with placebo. Surprisingly, we found that the average difference in Positive and Negative Symptoms Score variances was not large but slightly smaller under treatment compared to control. Thus, in this study, we found no evidence that antipsychotic drugs increased the outcome variance, suggesting no personal element of response to treatment but instead indicating that the variance was slightly lower in the treatment group than in the control group. We concluded that although the study cannot rule out that subsets of patients respond differently to treatment, it suggests that the average treatment effect is a more reasonable assumption for the individual patient than often assumed.

3.5 Study 5: Winkelbeiner et al. 2018, Schizophrenia Research

In this study, we analyzed individual differences in formal thought disorder, a major contributor to positive symptoms in psychosis and a cause of severe communication disturbances that may ultimately affect therapeutic reliance and clinical recovery. On the neural level, a range of associated brain areas have been found which might reflect the heterogeneity of formal thought disorder itself. Thus, we hypothesized that a differentiation into positive (such as unanticipated, bizarre, or inappropriately expressed speech) and negative (such as speech reduced in quantity, content, or fluency) thought disorder might help at characterizing the neural basis of thought disorder. Since this patient population is particularly difficult to recruit because of the communication difficulty, we included a relatively small sample of only twenty-four patients with psychosis. We assessed the severity of symptoms with the Thought, Language, and Communication, where the subscales Disorganization and Emptiness provided measures of positive and negative formal thought disorder. Using structural and perfusion imaging as readouts of the neurobiology, we found that negative thought disorder moderately correlated with decreased blood flow in the right insula and in the right middle temporal gyrus. We did not find evidence that positive thought disorder symptoms correlated with structural or perfusion measures. We interpreted these findings as providing preliminary evidence that abnormal blood perfusion might contribute to negative thought disorder and that studies investigating larger samples should elucidate whether positive and negative FTD indeed reflect distinct entities of thought disorder with different neurophysiological characteristics.

4 Conclusion

In summary, this work has shown that individual differences between patients constitute a major challenge for psychiatry. Patients with the same diagnosis and with exposition to a similar environment may vary considerably in the way they process information on the algorithmic and the neural level. The detection of such differences may benefit from computational models that reduce the high dimensional data to theoretically meaningful parameters. These parameters (such as the prediction error weight showed above) provide a representation of the data on a much lower dimension (Huys *et al.*, 2016) and may reflect individual differences that correlate with clinical symptoms. Whether such differences translate directly into differences in response to treatment is an open question. Our own evaluation of individual differences in antipsychotic treatment response suggested that we tend to overestimate the heterogeneity in treatment effects, simply because a clear distinction of the relevant variance components is not possible from classic randomized controlled trials. Elaborated study designs such as N-of-1 trials or advanced statistical modeling is needed to estimate the personal element of treatment response. Together, this indicates that a realistic evaluation of the need for precision medicine in psychiatry requires complex study designs together with computational modeling and advanced statistical techniques.

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5 Appendix: Discussed papers

5.1 Homan et al. 2019a, Neuropsychopharmacology



ARTICLE

Structural similarity networks predict clinical outcome in early-phase psychosis

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Despite recent advances, there is still a major need for prediction of treatment success in schizophrenia, a condition long considered a disorder of dysconnectivity in the brain. Graph theory provides a means to characterize the connectivity in both healthy and abnormal brains. We calculated structural similarity networks in each participant and hypothesized that the "hubness", i.e., the number of edges connecting a node to the rest of the network, would be associated with clinical outcome. This prospective controlled study took place at an academic research center and included 82 early-phase psychosis patients (23 females; mean age [SD] = 21.6 [5.5] years) and 58 healthy controls. Medications were administered in a double-blind randomized manner, and patients were scanned at baseline prior to treatment with second-generation antipsychotics. Symptoms were assessed with the Brief Psychiatric Rating Scale at baseline and over the course of 12 weeks. Nodal degree of structural similarity networks was computed for each subject and entered as a predictor of individual treatment response into a partial least squares (PLS) regression. The model fit was significant in a permutation test with 1000 permutations ($P = 0.006$), and the first two PLS regression components explained 29% (95% CI: 27; 30) of the variance in treatment response after cross-validation. Nodes loading strongly on the first PLS component were primarily located in the orbito- and prefrontal cortex, whereas nodes loading strongly on the second PLS component were primarily located in the superior temporal, precentral, and middle cingulate cortex. These data suggest a link between brain network morphology and clinical outcome in early-phase psychosis.

Neuropsychopharmacology (2019) 0:1–8; <https://doi.org/10.1038/s41386-019-0322-y>

INTRODUCTION

Finding predictors of response to antipsychotic drug treatment is of critical importance to improving outcomes for psychotic disorders [1, 2]. A priori identification of patients who are not likely to respond to a specific treatment strategy could reduce the number and length of ineffective treatment trials. Moreover, understanding the biological underpinnings of effective treatments may lead to the detection of malleable central nervous system targets for the development of new treatment strategies—a current imperative because of the long-standing dearth of novel antipsychotic treatments.

Although parallel group trials cannot determine treatment response for individual patients in a definite manner, we have previously applied rigorous a priori criteria to distinguish likely responders from non-responders [3, 4] and have shown that functional striatal connectivity was higher in non-responders compared with responders [5] and normalized with antipsychotic treatment [6].

The fact that we found that brain connectivity was related to treatment outcome is not surprising. Schizophrenia has long been considered a disorder involving dysconnectivity in the human brain [7, 8], and graph theory has provided means to characterize the connectivity in the healthy and abnormal human brain [9].

Briefly, graph theory describes brain networks abstractly as a set of nodes and edges, and quantifies their patterns of connectivity [7]. Although normal brain graphs have typical properties (i.e., they are more organized than random graphs), brain graphs of schizophrenia patients may have specific abnormalities.

For example, highly connected nodes in the brain that are also densely connected with one another, so-called rich clubs, are present in schizophrenia but less prominent compared with healthy controls [10]. Other structural imaging studies using graph theory found evidence for less information integration and more clustering of nodes across brain regions [10–12]. Similar findings have emerged in other structural studies [13, 14], but studies based on functional connectivity have not always converged with these structural findings [15–17], and only a minority of studies have demonstrated relevance of these graph metrics for clinical outcome.

Although two prior studies [18, 19] computed group-wise graph metrics in responders and non-responders, we here tested how individual network architecture relates to individual clinical outcome. Previous work has shown that this can be achieved by comparing, in each individual, the statistical similarity between brain regions [20–22] or by assessing their correlations across different imaging domains [23]. Although still speculative, previous studies suggest that statistical similarity networks might

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capture biologically meaningful correlates of development, aging, and brain disorders [24].

Statistical similarity between brain regions can be used to build similarity matrices across the whole brain for each individual from which a binary graph of nodes and edges can be constructed. This gives a similarity network (or connectome) for each participant, which can then serve as a predictor for individual treatment outcome. This was the approach we used in the current study (Fig. 1). Based on ample evidence that schizophrenia brain abnormalities are primarily located in highly connected nodes in the human connectome [25], we focused our analysis on the hubness (or nodal degree) graph metric, which can be computed from the similarity networks. We hypothesized that the hubness of cortical nodes would be associated with individual treatment outcome in two concatenated early psychosis cohorts. Treatment outcome of positive symptoms was computed using mixed models [26–30].

MATERIALS AND METHODS

Participants

We used two early-phase psychosis cohorts from two separate 12-week clinical trials on second-generation antipsychotics with a similar design and similar treatment effects (Fig. S1). Details have been published previously [31] and are summarized in Table 1, as well as in the Supplementary Information. Importantly, there were no significant differences between studies in duration of untreated psychosis ($t(62.48) = 0.72$, $P = 0.473$) and in the proportion of medication naïve participants ($\chi^2(1) = 2.66$, $P = 0.103$). Written informed consent was obtained from adult participants and the legal guardians of participants younger than 18 years. All participants under the age of 18 provided written informed assent. The study was approved by the Institutional Review Board (IRB) of Northwell Health, which served as the central IRB for all clinical sites. To replicate previously reported group differences in cortical thickness [32], we also included a sample of 58 healthy controls (Table 1). Healthy controls were recruited at the Zucker Hillside Hospital during the CIDAR trial.

Patients had a current DSM-IV-defined diagnosis of schizophrenia, schizophriform, schizoaffective disorder, or psychotic disorder not otherwise specified, and bipolar disorder with psychotic features and could have had up to 2 years of antipsychotic treatment. Many but not all subjects were first-episode patients. Note that excluding the patients with a diagnosis of bipolar disorder with psychotic features ($N = 3$) did not alter the results.

Symptom assessments using the anchored version of the Brief Psychiatric Rating Scale (BPRS-A) were done at baseline, weekly for 4 weeks, every 2 weeks until week 12. To obtain a measure of positive symptoms, we defined thought disturbance [3] as the sum of the following items: conceptual disorganization, grandiosity, hallucinatory behavior, and unusual thought content.

Structural magnetic resonance imaging and analysis
 Magnetic resonance imaging (MRI) exams were conducted on a 3-T scanner (GE Signa HDx). All participants were measured on the same scanner. We acquired anatomical scans in the coronal plane using an inversion-recovery prepared 3D fast spoiled gradient sequence (TR = 7.5 ms, TE = 3 ms, TI = 650 ms matrix = 256 × 256, FOV = 240 mm), which produced 216 contiguous images (slice thickness = 1 mm) through the whole brain. Image processing and segmentation were conducted with the FreeSurfer 5.1.0 recon-all pipeline and the Desikan-Killiany cortical atlas [33]. All image processing, parcellation, and quality control procedures were conducted while blinded to participants' demographic and clinical characteristics. Visual inspections for quality assurance were conducted and no manual interventions were necessary. Details

on the processing pipeline can be found in the Supplementary Information. We then tested for group differences in cortical thickness for each of the 68 regions as derived from the FreeSurfer recon-all pipeline and adjusted this analysis for age, sex, and intracranial volume.

Individual treatment response estimation

Rather than categorizing participants to responders and non-responders, which is statistically inefficient [29], we focused on treatment response as a continuous measure and used mixed models to make efficient use of the full sample and the repeated measures [26, 30, 34, 35].

Individual response was estimated using mixed models with restricted maximum likelihood and used as outcome measure in subsequent analyses. With mixed models, individual responses are estimated to be closer to the average treatment response, an effect that is well known as partial pooling or shrinkage [36]. The partial pooling effect for the two psychosis cohorts can be visualized by showing how individual treatment effects are pulled toward the average treatment effect (Fig. 2). Partial pooling makes the analysis less susceptible to individual outliers by attenuating the impact of participants with only few assessments (Fig. 2), which is of particular importance in estimating treatment response in relatively small samples. Although only 82 participants had MRI baseline data available, the full sample of 248 participants was used to obtain more precise estimates of the individual treatment response slopes. A more detailed description can be found in the Supplementary Information.

Graph theoretic analysis

Individual network graphs were computed following a new method that estimates statistical similarity across brain regions in each individual participant [20, 21]. The analysis flowchart is shown in Fig. 1. After cortical parcellation into 68 brain regions through the FreeSurfer recon-all pipeline, statistical similarity between all pair-wise brain regions was computed in each individual. First, probability density functions were estimated for the cortical thickness distribution in each region, using a Gaussian kernel and 512 sampling points. We followed the comprehensive characterization of structural similarity networks by Wang and colleagues [22] who also investigated the diminishing influence of increasing numbers of sampling points for the probability density function estimation. We then chose a more conservative resolution that the one used by Wang and colleagues [22] of 512 sampling points. This resulted in probability distributions for each of the 68 brain regions. Statistical similarity between each possible pair of distributions was then computed by calculating the Kullback-Leibler (KL) divergence between them [20–22]. The KL divergence measures the difference between two probability distributions (i.e., the loss of information when one distribution is used to approximate another). The KL divergence is thus defined as

$$D_{KL}(P||Q) = \sum_{i=1}^n P(i) \log \frac{P(i)}{Q(i)} \quad (1)$$

with P and Q being two probability distribution functions and n the number of sample points. Since $D_{KL}(P||Q)$ is not equal to $D_{KL}(Q||P)$, a symmetric variation of the KL divergence can be derived as follows:

$$D_{KL}(P, Q) = \sum_{i=1}^n \left(P(i) \log \frac{P(i)}{Q(i)} + Q(i) \log \frac{Q(i)}{P(i)} \right). \quad (2)$$

Finally, the following transformation was used to limit the measure to a range from 0 to 1:

$$KLS(P, Q) = e^{-D_{KL}(P, Q)}. \quad (3)$$

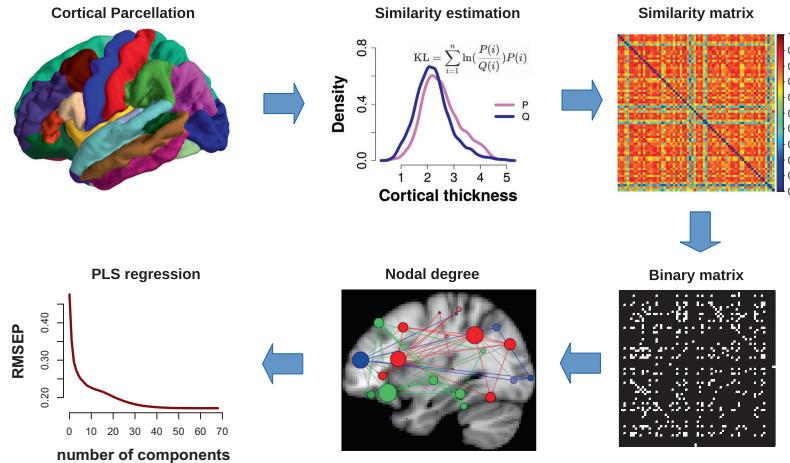


Fig. 1 Analysis flowchart. Computation of similarity networks in each participant by calculating the statistical similarity between brain regions. First, Freesurfer cortical parcellation was conducted for each individual participant. Cortical thickness was extracted for each vertex within each region and used to estimate the probability distribution function. The similarity between any pair of region was then estimated by calculating the KL divergence of their probability distributions, resulting in a 68×68 similarity matrix. The KL divergence computes the loss of information when one distribution is used to approximate another. The similarity matrix was then thresholded into a binary matrix to create a network graph. Graph-based degree (or hubness) for each node was then calculated for each individual participant. Nodal degrees were then entered as predictors into a partial least squares regression, using individual treatment response slopes as outcome measure. KL Kullback–Leibler divergence, PLS partial least squares regression, RMSEP root mean square error of prediction

Table 1. Sample characteristics						
Characteristic	PSY	Mean	SD	HC	Mean	SD
Males	59			25		
Females	23			33		
Age, years	82	21.6	5.5	58	28.1	11.9
Schizophrenia	57					
Schizoaffective disorder	15					
Psychotic disorder NOS	5					
Bipolar I disorder (with psychotic features)	3					
Prior medication exposure	75	8.4	29.4			
Medication naïve	28					
Medication 2 weeks or less	71					
Education, years	77	12.2	2.2	58	13.7	2.9
IQ	69	97.9	13.2	49	102.2	10
BPRS total	82	42.7	7.5	0		
BPRS TD	82	14.3	3.1	0		
DUP, weeks	77	110.9	176.2	0		

PSY patients with early psychosis, HC healthy controls, BPRS Brief Psychiatric Rating Scale, BPRS TD Brief Psychiatric Rating Scale Thinking Disturbance, DUP duration of untreated psychosis, NOS not otherwise specified, IQ intelligence quotient

We thus computed the KLS values for all possible pairs of brain regions in each individual participant, resulting in a 68×68 similarity matrix S_{ij} for each subject (Fig. 1). Individual similarity matrices were then binarized by employing a sparsity threshold τ (number of actual edges divided by the maximum possible number of edges in a network), which ensured the same number of nodes and edges for the networks across participants. This resulted in a binary adjacency matrix A_{ij} [22]:

$$A_{ij} = [a_{ij}] = \begin{cases} 1, & \text{if } s_{ij} > KLS_{th}; \\ 0, & \text{otherwise,} \end{cases} \quad (4)$$

with KLS_{th} being a subject-specific KLS threshold that ensured that all networks had the same number of nodes and edges across participants. Following previous work [23], we chose a sparsity threshold of $\tau = 0.1$ for our analysis but also repeated calculations for a range of thresholds (0.1–0.7, with intervals of 0.05). The binary matrices allowed us to construct graphs of nodes and edges.

The graph theoretic measure of primary interest was nodal degree or hubness. The degree, $k(i)$, of a node is the number of edges connecting the i -th region to the rest of the network:

$$k(i) = \sum_{j=1}^n A_{ij}, \quad (5)$$

where A_{ij} is the binary adjacency matrix, which was computed by thresholding the similarity matrix, S_{ij} .

29 Statistical analyses

We used multivariate partial least squares (PLS) regression to test the relationship between nodal degree and individual treatment

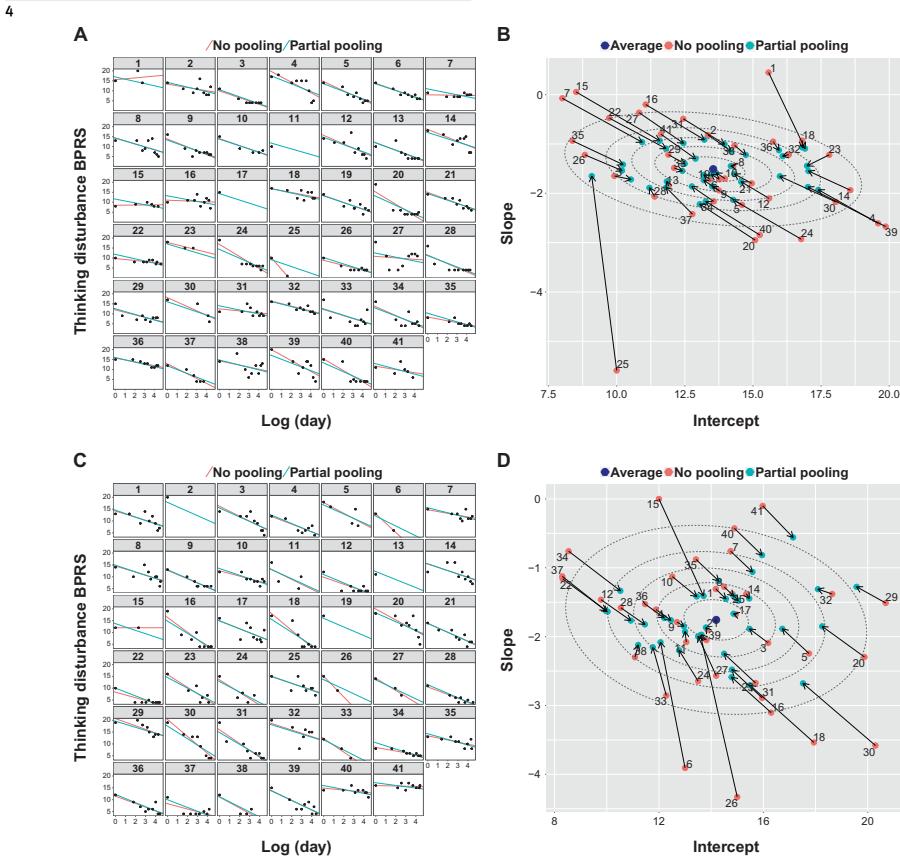


Fig. 2 Partial pooling to regularize individual response slopes. **a** Individual time courses for all participants from the first schizophrenia cohort. A log-linear relationship between time as measured in days from baseline and thinking disturbance symptoms was evident. Partial pooling regularized the individual slopes, i.e., the influence of outliers with only few assessments was attenuated. **b** The partial pooling effect is demonstrated by the individual responses being pulled toward the average treatment effect. As a consequence, outliers are less influential, as is particularly striking for those participants with few assessments. **c, d** The same is shown for the second schizophrenia cohort. Dotted ellipses indicate confidence regions for the average treatment effect (10, 30, 50, 70, 90%, respectively)

response. This method is particularly suited for a set of highly correlated predictors. In PLS, the optimal low-dimensional solution for a relationship between a set of correlated predictor variables and a response variable is computed. The 82×68 predictor variable matrix comprised estimates of degree (calculated at 10% connection density) for each of 68 nodes in each of 82 participants.

The (82×1) response vector comprised individual treatment response slopes. To account for unspecific inter-individual differences, the predictor matrix and the response vector were regressed on potential confounds, including study cohort (CIDAR, OMEGA3), baseline value of thinking disturbance, intracranial

volume, age, gender, and age \times gender interaction. Residuals of this regression were then used in the actual PLS analysis.

To rank each cerebral node according to its correlation with each PLS component, we used bootstrapping, i.e., drawing 1000 samples with replacement of the 82 individual participants, to compute the error on the PLS weights. A similar procedure has been used in recent graph theoretic work [23, 37, 38].

All analyses were conducted with R version 3.3.2 (2016-10-31) [39]. Mixed models were estimated using the lme4 library [40]. PLS regression were computed with the pls library [41], bootstrapping was performed with the boot library [42], and brain graph metrics were computed with the brainGraph [43] and igraph [44] libraries.

Python 2.7.15rc1 and pySurfer (0.8.0) were used for visualizing the imaging results.

RESULTS

Nodal degree predicts treatment response

The first two PLS regression components explained 29% (95% confidence interval (CI): 27, 30) of the variance in treatment response after cross-validation, with 1000 sets of five participants held out from the sample. This model fit was significant in a permutation test with 1000 permutations ($P = 0.006$). Notably, statistical significance ($P < 0.05$) was maintained when repeating the PLS regression across connection densities (0.1–0.7). The PLS components were positively correlated with treatment response slopes (first PLS component: $r = 0.66$, $P < 0.001$; second PLS component: $r = 0.4$, $P < 0.001$; Fig. 3). To assess their contribution, we ranked the 68 nodes of the individual networks according to their bootstrap standardized weight on each PLS component [23, 37, 38]. Most importantly, since the sign of the correlation between PLS components and treatment response slopes was positive (Fig. 3), nodes that correlated strongly with PLS scores had a negative relationship with treatment response. We found that they were primarily located in the orbito- and prefrontal cortices and posterior cingulate cortex for the first PLS component and in the superior temporal, precentral, and middle cingulate brain areas for the second PLS component (Fig. 3).

In summary, these findings suggest that individual differences in the configuration of structural similarity networks explain a significant proportion of the variance in treatment response.

We then also tested for group differences in nodal degree between psychosis patients and a cohort of healthy controls. Using permutation tests with 1000 permutations for each of the 68 nodes, we found a significant increase in nodal degree in the left orbitofrontal cortex ($P < 0.05$; Fig. S2).

Validation of results

To verify that our findings held up when using a different cortical parcellation scheme, we repeated the analysis using the Destrieux atlas [45], which comprises 148 nodes. We again found that the first two PLS components explained 30% (95% CI: 28, 31) of the variance in treatment response after cross-validation. In addition, excluding the patients with a diagnosis of bipolar disorder with psychotic features ($N = 3$) did not alter the results.

Group differences in cortical thickness

For completeness, we also assessed how the psychosis cohorts differed from the control cohort in terms of cortical thickness in all 68 cortical nodes. Confirming previous reports [32], cortical thickness was reduced in patients compared with controls most prominently in the left paracentral and parahippocampal gyrus and increased in the right rostral anterior cingulate cortex (Fig. S3). However, these alterations did not survive correction for multiple comparisons using false discovery rate ($q < 0.05$).

DISCUSSION

Here we showed that individual differences in structural similarity networks predicted treatment response in early-phase psychosis. The importance of this finding is twofold. First, we used the continuous scale to define treatment response, thereby increasing statistical sensitivity and avoiding power loss through dichotomization [29]. Second, and related, we computed networks for each participant, which allowed us to predict treatment response on the individual level.

We focused our analysis on a specific graph theoretic parameter, namely nodal degree or hubness. The rationale for this decision was that brain networks contain only a minority of highly connected nodes acting as hubs. Hubs are considered to be

functionally valuable by supporting information integration [25] but their value comes at a high biological cost due to increased metabolic demand and long-distance connections. Their prominent role suggests that schizophrenia-relevant brain abnormalities should be concentrated in hubs, a prediction that was indeed supported by a large body of meta-analytic evidence [25], where schizophrenia lesions were found most dominantly in frontal and temporal cortical hubs. In line with this notion, we found that nodal degree in orbito- and prefrontal areas contributed most strongly to the prediction of treatment response, with additional contributions from superior temporal regions. Importantly, nodes in the right orbitofrontal cortex have showed reduced degree compared with controls in functional networks in schizophrenia [15]. Our work extends this finding by showing that orbitofrontal nodes appear to impact clinical outcome in early-phase psychosis.

Although previous studies indicated the usefulness of brain morphology and machine learning in predicting response to treatment [46, 47], only one study has investigated the relationship between anatomical networks and clinical outcome [48]. That study used structural covariance of cortical folding to predict treatment response in first-episode psychosis, and found higher segregation, poorer integration, and vulnerable gyration covariance in non-responders. Specifically, non-responders showed reduced centrality of the left insula and anterior cingulate cortex. In addition, they were also more vulnerable to simulated lesions, i.e., covariance disintegrated after removal of high-degree hubs, supporting the relevance of nodal degree for treatment response [48]. A comparable study that used resting state functional connectivity found reduced global efficiency and increased clustering in patients with schizophrenia that normalized with response to antipsychotic treatment [19].

What is the biological meaning of structural similarity networks? It has been hypothesized that brain regions that grow together should display strong structural covariance across individuals [48, 49]. In line with this hypothesis, previous work has shown that structural networks of regions that grow together share similar global and nodal topological properties [50]. Thus, a likely interpretation is that structural similarity networks reflect "synchronized developmental change in distributed cortical regions" [50]. Accordingly, structural covariance networks show reorganization during normal development [24, 49, 51], aging [21, 52–54], and disease [12, 55, 56]. This suggests that structural similarity networks capture biologically meaningful correlates of development, aging, and brain disorders. Speculatively, then, these processes may impact distributed and treatment-relevant brain areas.

How can brain regions, similar in their thickness patterns and potentially growing synchronously, influence treatment outcome? Although still speculative, a potential mechanism is through the relationship with cognition. We have previously shown that higher scores in general cognition and reasoning capacity in particular were positive predictors of treatment response [4]. However, since that study did not include any brain imaging data, we could not characterize a potential neural correlate of this effect. Since brain network properties have been shown to be positively correlated with cognition [57], it is possible that they are the biological correlate underlying both cognition and improved treatment outcome.

An important difference between our study and previous attempts to characterize individual treatment response is that we did not dichotomize our sample into responders and non-responders. Although such dichotomization may be appealing and particularly relevant to clinicians, it is statistically inefficient to binarize a continuous measure [29, 58, 59]. The argument that binary decisions are what clinicians ultimately need to make, and therefore research should provide them with binary classifications, can easily be refuted. Indeed, if a binary decision must be made, it must be made at the point of actual clinical care, when all costs

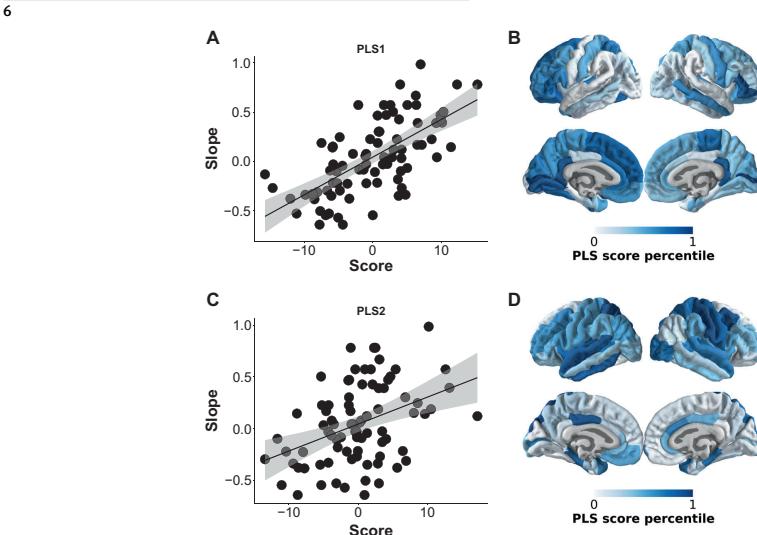


Fig. 3 Correlations of partial least squares (PLS) scores with individual treatment response and contribution of cortical nodes in the psychosis cohort ($N = 82$). Nodal degree for each of the 68 was entered into a PLS regression, with individual treatment response slopes as outcome measure. The first two PLS components explained a significant proportion of variance in treatment response. **a, b** The first PLS component correlated most strongly with nodal degree of orbito- and prefrontal cortices and posterior cingulate cortex. Note that more negative slopes meant better treatment response. **c, d** The second PLS component correlated most strongly with superior temporal, precentral, and middle cingulate brain areas

and potential benefits are known [59]. For example, the clinician may decide that a probable non-responder may still undergo treatment if the potential benefit outweighs the risk for that particular case. Furthermore, it is generally under-appreciated that it is often difficult to determine whether or not an individual patient responded to the treatment. The reason for this is that one does not know how the patient would have done under placebo. This is often overlooked and can limit attempts at classifying patients based on observed response. Thus, treatment response prediction based on a single-criterion classification in responders and non-responders should be treated with caution [75].

What is thus the potential clinical meaning and application of the current study's findings? With an approach such as this, the ultimate goal is to provide the clinician with an estimate of response probability for a given patient. As we have just shown, this is different from classifying the patient as a responder or non-responder. After taking all available information into account, it is the clinician (and not the researcher) who ultimately decides whether to treat even a likely non-responder. A study such as this can thus inform the clinician's decision process by providing individual predictions of treatment response.

The mixed model approach employed in this study is one way to address this problem; it separates random variation from actual treatment variation in each participant [29, 30]. In addition, individual treatment response is estimated more efficiently by utilizing the full data set and more conservatively by applying shrinkage [34, 36].

A few limitations merit comment. First, we did not include a placebo control group, which would have allowed us to compare the overall variability in response between the treatment and the

control groups. In principle, such a comparison would need to show that the variability in the treatment arm is higher than the one in the placebo arm, and that the difference is clinically relevant [60, 75]. However, placebo-controlled trials in early-phase psychosis have rarely been conducted. In addition, since the neurobiological underpinnings of graph metrics are still unknown, pathophysiological inferences must be made with caution. Next, we did not include functional connectivity [5] or other potentially predictive data in this study, which means that our findings may not be predictive when other predictors are included in the model. Finally, graph metrics may also vary depending on the parcellation scheme. However, in this study we repeated our analysis with an additional atlas of 148 nodes and found essentially the same results.

In conclusion, this study showed that advanced statistical modeling of treatment response and a relatively novel [20] computation of structural similarity networks established a potential link between brain network morphology and clinical outcome in early-phase psychosis.

FUNDING AND DISCLOSURE

This study was presented first at the European Conference of Schizophrenia Research 2017, Berlin, Germany. This work was supported by NIMH grant P50MH080173 to Dr. Malhotra, grant P30MH090590 to Dr. Kane, grant R01MH060004 to Dr. Robinson, grant R01MH076995 to Dr. Szeszko, and grant R21MH101746 to Drs. Robinson and Szeszko. Dr. Homan had full access to all the data in the study and takes responsibility for the integrity of the data and the accuracy of the data analysis. Dr. Robinson has

served as a consultant for Asubio, Otsuka, and Shire and has received grants from Bristol-Myers Squibb, Janssen, and Otsuka. Dr. Kane has served as a consultant for Alkermes, Eli Lilly, Forest, Forum, Genentech, Intracellular Therapies, Janssen, Johnson & Johnson, Lundbeck, Otsuka, Reviva, Roche, Sunovion, and Teva; he has received honoraria for lectures from Genentech, Janssen, Lundbeck, and Otsuka; and he is a shareholder in MedAvante and Vanguard Research Group. Dr. Lenzc has served as a consultant for Genomind. Dr. Malhotra has served as a consultant for Forum Pharmaceuticals and has served on a scientific advisory board for Genomind. The remaining authors declare no competing interests.

ACKNOWLEDGEMENTS

The authors acknowledge their patients, their patients' families, and their psychiatry research support staff. They thank Robert Gibbons, Ph.D., and Stephen Senn, Ph.D., for feedback regarding the statistical approaches used in this study.

DATA AND CODE AVAILABILITY

The data and code supporting the findings of this study are available online at <https://github.com/philiploman/ssn>. To ensure reproducibility we included a Makefile, which reflects the dependencies between different analysis modules and allows to run the analysis using the "make" command from the Unix command line.

ADDITIONAL INFORMATION

Supplementary Information accompanies this paper at (<https://doi.org/10.1038/s41386-019-0322-y>).

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Supplementary Information

First cohort: CIDAR trial

Settings

The study was conducted at eight area facilities in New York City and one facility each in San Antonio, TX and Calgary, Alberta, Canada. All sites were not-for-profit institutions (either academic centers, community facilities, or public hospitals) located in urban or suburban areas, serving diverse communities in terms of economic status and racial/ethnic composition. Imaging data was acquired only for participants from the site in New York City. Data were collected from December 2005 until April 2013.

Participants

Enrolled patients had a current DSM-IV-defined diagnosis of schizophrenia, schizophreriform, schizoaffective disorder, or psychotic disorder Not Otherwise Specified (NOS) and were between 15-40 years of age. At baseline, patients had 2 weeks or less of a lifetime exposure to antipsychotic medication treatment (at any dose) and current positive symptoms of at least 4 (moderate) on one or more of the Brief Psychiatric Rating Scale-Anchored version (BPRS-A) 7 items: conceptual disorganization, grandiosity, hallucinatory behavior, or unusual thought content. For women, a negative pregnancy test and agreement to use a medically accepted birth control method was required.

Exclusion criteria were (1) meeting DSM-IV criteria for current substance-induced psychotic disorder, psychotic disorder due to a general medical condition, delusional disorder, brief psychotic disorder, shared psychotic disorder, or mood disorder (major depression or bipolar) with psychotic features; (2) serious neurological or endocrine disorder or medical condition/treatment known to affect

the brain; (3) medical conditions requiring treatment with a medication with psychotropic effects; (4) medical contraindications to risperidone or aripiprazole treatment; (5) significant risk of suicidal or homicidal behavior; (6) any factor (eg, language limitations) that would preclude informed consent or participation in study procedures; (7) diagnosis of diabetes (defined as fasting plasma glucose >126 mg/dl) or the metabolic syndrome (defined as 3 or more of the following: high blood pressure (>130/85 mm Hg), truncal obesity (waist circumference >40 inches for men and >35 for women), elevated fasting glucose (>110 mg/dl), low, high-density lipoprotein (HDL) cholesterol (<40 mg/dl for men and <50 mg/dl for women), and elevated triglycerides (>150 mg/dl); and (8) requiring antidepressant or mood stabilizer treatment.

Written informed consent was obtained from adult participants and the legal guardians of participants younger than 18 years. All participants under the age of 18 provided written informed assent. The study was approved by the Institutional Review Board (IRB) of Northwell Health, which served as the central IRB for all clinical sites.

Treatment

The acute trial of aripiprazole vs risperidone treatment lasted 12 weeks. Participants were stratified by site, previous antipsychotic exposure (none vs any), and diagnosis (psychotic disorder NOS vs other eligible diagnoses) and were randomly assigned on a 1:1 basis to double-masked treatment with either aripiprazole (5-30 mg/d) or risperidone (1-6 mg/d). Inclusion criteria required all participants to have very limited prior antipsychotic exposure (less than 2 weeks, see above); any antipsychotics being taken at study entry were discontinued.

The initial daily dose was 1 study capsule (ie, 5 mg of aripiprazole or 1 mg of risperidone). Medication doses were advanced according to a titration schedule (level 2 at day 4, level 3 at week 1,

two level 2 capsules at week 4, a level 2 and level 3 capsule at week 6, and two level 3 capsules at week 8) until response criteria were achieved or dose-limiting side effects occurred. Study psychiatrists could advance or slow the titration schedule for clinical needs.

Assessments

Initial diagnostic eligibility was established with the Structured Clinical Interview for DSM-IV Axis I Disorders (SCID). These data were later reviewed in a consensus conference for final diagnostic assignment. Symptom assessments using the BRPS-A were done at baseline, weekly for 4 weeks, every 2 weeks until week 12. To obtain a measure of positive symptoms, we defined thought disturbance [3] as the sum of the following items: conceptual disorganization, grandiosity, hallucinatory behavior, and unusual thought content. Assessments were performed by a team of central raters that traveled to New York area sites in person or used secure teleconferencing elsewhere. The same rater performed assessments with each participant throughout that individual's study participation. Intraclass correlation coefficients for the BPRS items were generally high (> 0.9) [3]. At baseline, trained psychometricians administered the MCCB and adhered to procedures described in the test manual.

Structural magnetic resonance imaging and analysis

Magnetic resonance imaging exams were conducted on a 3-T scanner (GE Signa HDx). We acquired anatomical scans in the coronal plane using an inversion-recovery prepared 3D fast spoiled gradient (IR-FSPGR) sequence (TR = 7.5 ms, TE = 3 ms, TI = 650 ms matrix = 256 × 256, FOV = 240 mm) which produced 216 contiguous images (slice thickness = 1 mm) through the whole brain. Image processing and segmentation were conducted with the Freesurfer 5.1.0 recon-all pipeline and the Desikan-Killiany cortical atlas [31]. Of note, this pipeline includes two surface smoothing steps with 10 curvature

averaging iterations and 10 smoothing iterations. All image processing, parcellation, and quality control procedures were conducted while blinded to participants' demographic and clinical characteristics. Visual inspections for quality assurance were conducted and no manual interventions were necessary.

Second cohort: OMEGA3 trial

Participants

For this cohort, we additionally included participants with bipolar disorder with psychotic features and participants could have had up to 2 years of antipsychotic treatment. Many but not all subjects were first episode patients. Apart from that, we applied the same inclusion and exclusion criteria as in the CIDAR trial (see above). After complete description of the study, written informed consent was obtained from adult participants and legal guardians of participants younger than 18 years who provided written assent. The study was approved by the Institutional Review Board (IRB) of the Feinstein Institute for Medical Research.

Assessments

Initial diagnostic eligibility was established with the Structured Clinical Interview for DSM-IV Axis I Disorders (SCID). These data were later reviewed in a consensus conference for final diagnostic assignment. Assessments of BRPS-A were done at baseline, weekly for 4 weeks, every 2 weeks until week 12. Thought disturbance was defined as the sum of unusual thought content, hallucinations, and conceptual disorganization from the BPRS-A. Assessments were performed by trained clinical raters at the Zucker Hillside Hospital. The same rater performed assessments with each participant throughout that individual's study participation.

Structural magnetic resonance imaging and analysis

Magnetic resonance imaging and analysis was conducted similarly as in the CIDAR trial (see above).

Freesurfer processing details

Briefly, and following a recent thorough description of the procedure [61], this freesurfer processing includes motion correction and averaging [62] of two volumetric T1 weighted images. Non-brain tissue is then removed with a hybrid deformation method [63], followed by a Talairach transformation and segmentation of subcortical white matter and deep gray matter volumetric structures [64, 65]. intensity normalization, tessellation of the gray matter white matter boundary, and automated correction of topology [66, 67]. The next step is surface deformation, guided by intensity gradients for optimal placement of gray/white and gray/cerebrospinal fluid borders, using the location where the greatest shift in intensity defines the transition to the other tissue class [68, 69].

After completion of the cortical models, final processing steps include surface inflation [68], registration to a spherical atlas based on individual cortical folding patterns [70], and parcellation of the cerebral cortex into units with respect to gyral and sulcal structure [33, 71]. Importantly, cortical thickness is then measured as the Euclidean distance from the gray/white boundary to the gray/CSF boundary at each vertex on the tessellated surface [69]. Procedures for the measurement of cortical thickness have been validated against manual measurements [72] and have shown high test-retest reliability [73, 74].

Individual response estimation

Assuming linear improvement over time, individual treatment response can be modeled as

$$Y_{ij} = \beta_{0j} + \beta_{1j} X_{ij} + \epsilon_{ij}, \quad (6)$$

where Y is the outcome measure (i.e., the symptoms) at time i for patient j , β_{0j} is the intercept and β_{1j} is the slope for patient j , X_{ij} is a vector of the time predictor (measured in log-transformed days from baseline), while ϵ is the error term, which is assumed to be normally distributed with mean zero and some unknown variance,

$$\epsilon_{ij} \sim \mathcal{N}(0, \sigma^2). \quad (7)$$

This level-1 model thus indicates each patient's treatment response over time. It can be extended to a level-2 model by including sample averages for intercept and slope,

$$\beta_{0j} = \gamma_{00} + U_{0j} \quad (8)$$

$$\beta_{1j} = \gamma_{10} + U_{1j} \quad (9)$$

with

$$\begin{pmatrix} U_{0j} \\ U_{1j} \end{pmatrix} \sim \mathcal{N} \begin{pmatrix} 0, & \tau_{00}^2 & \tau_{01} \\ 0, & \tau_{01} & \tau_{10}^2 \end{pmatrix}, \quad (10)$$

where U_{0j} is the random intercept and U_{1j} is the random slope, that is, the individual deviances from the sample intercepts and slopes, respectively, both with zero mean and some unknown variances. Note that the random slope, U_{1j} , was the measure of interest in this study and used as primary outcome.

The covariance between the random intercept and the random slope is indicated with τ_{01} . This hierarchical unconditional growth model describes the average symptom trajectory while accounting for individual deviances from both the intercept and the slope.

Due to the well-known challenges in assessing this specific patient population, missed assessments and minor variations in rating intervals were to be expected. We dealt with them by calculating the exact days from baseline when the ratings were performed, and used days rather than weeks as the time variable in our analysis. To account for the log-linear decrease of symptoms over time (Fig. S1) we used the log-transformed number of days.

An important difference from previous strategies to characterize individual treatment response is that we did not distinguish, *a priori*, between patients who met or did not meet a defined response criterion.

Rather, our method allowed for the estimation of individual treatment response. While only 82 participants had MRI baseline data available, the full sample of 248 participants was utilized to obtain more precise estimates of the individual treatment response slopes. The rationale for this approach is that information from the full sample can be used to estimate individual slopes more conservatively because the average treatment effect of the full sample provides a reasonable prior assumption for each individual response slope [34]. Thus, similar to full Bayesian estimation, a prior enters the statistical calculation; however, here the prior is derived empirically from the data, hence this method bears similarity with Empirical Bayes. The result is that individual responses are estimated to be closer to the average treatment response, an effect that is well-known as partial pooling or shrinkage. [36]. The partial pooling effect for the two schizophrenia cohorts can be visualized by showing how individual treatment effects are pulled toward the average treatment effect (Fig. 2). Partial pooling makes the analysis less susceptible to individual outliers by attenuating the impact of participants with only few assessments (Fig. 2), which is of particular importance in estimating treatment response in relatively small samples.

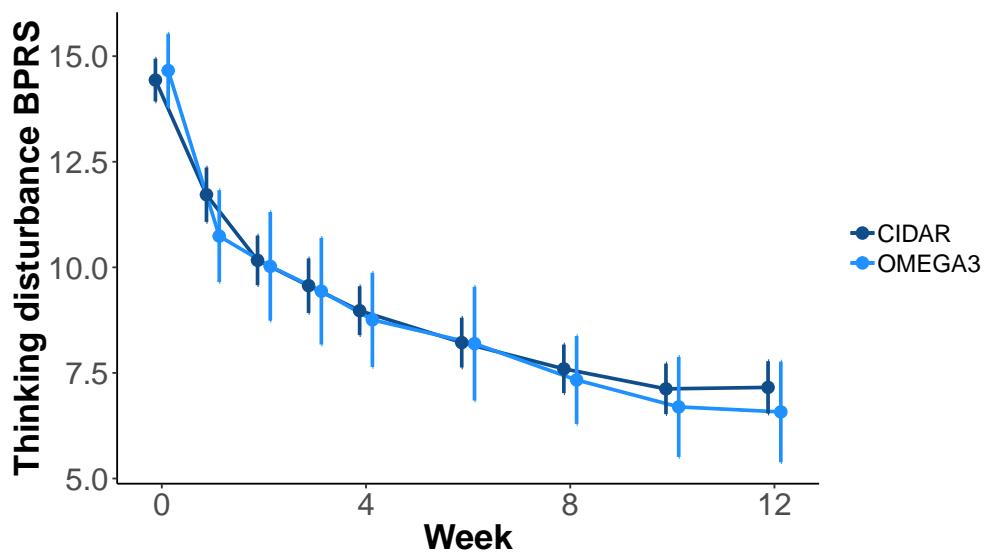
Thus, individual slopes were estimated using mixed models with Restricted Maximum Likelihood (REML) and used as outcome measure in subsequent analyses.

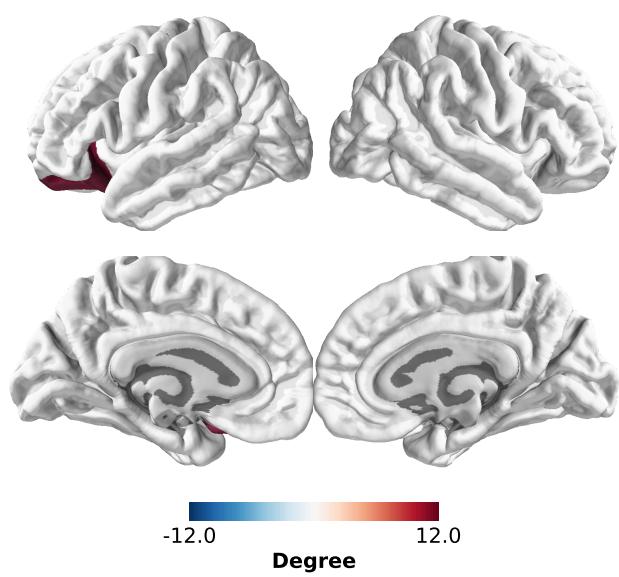
Figure legends

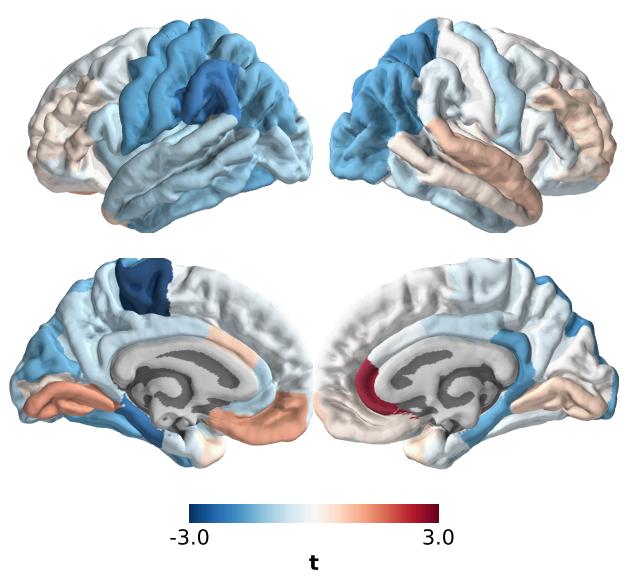
Figure S1: Both schizophrenia cohorts showed a similar average treatment effect. Means with 95% confidence intervals are shown. In both cohorts, symptoms decreased in a log-linear fashion with increasing weeks. To increase statistical power, both cohorts were thus concatenated for the subsequent analysis of treatment response prediction.

Figure S2: Significant group differences in nodal degree between patients with psychosis and healthy controls. Positive values indicate higher nodal degrees in patients compared to controls.

Figure S3: Group differences in cortical thickness. Cortical thickness was reduced in patients compared to controls most prominently in the left paracentral and left parahippocampal gyrus. Note that these reductions did not survive rigid correction for multiple comparisons using false discovery rate (FDR $q < 0.05$).







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ARTICLE

Striatal volume and functional connectivity correlate with weight gain in early-phase psychosis

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Second-generation antipsychotic drugs (SGAs) are essential in the treatment of psychotic disorders, but are well-known for inducing substantial weight gain and obesity. Critically, weight gain may reduce life expectancy for up to 20–30 years in patients with psychotic disorders, and prognostic biomarkers are generally lacking. Even though other receptors are also implicated, the dorsal striatum, rich in dopamine D2 receptors, which are antagonized by antipsychotic medications, plays a key role in the human reward system and in appetite regulation, suggesting that altered dopamine activity in the striatal reward circuitry may be responsible for increased food craving and weight gain. Here, we measured striatal volume and striatal resting-state functional connectivity at baseline, and weight gain over the course of 12 weeks of antipsychotic treatment in 81 patients with early-phase psychosis. We also included a sample of 58 healthy controls. Weight measurements were completed at baseline, and then weekly for 4 weeks, and every 2 weeks until week 12. We used linear mixed models to compute individual weight gain trajectories. Striatal volume and whole-brain striatal connectivity were then calculated for each subject, and used to assess the relationship between striatal structure and function and individual weight gain in multiple regression models. Patients had similar baseline weights and body mass indices (BMI) compared with healthy controls. There was no evidence that prior drug exposure or duration of untreated psychosis correlated with baseline BMI. Higher left putamen volume and lower sensory motor connectivity correlated with the magnitude of weight gain in patients, and these effects multiplied when the structure–function interaction was considered in an additional exploratory analysis. In conclusion, these results provide evidence for a correlation of striatal structure and function with antipsychotic-induced weight gain. Lower striatal connectivity was associated with more weight gain, and this relationship was stronger for higher compared with lower left putamen volumes.

Neuropsychopharmacology (2019) 0:1–7; <https://doi.org/10.1038/s41386-019-0464-y>

INTRODUCTION

Weight gain is a major side effect of treatment with antipsychotic drugs, but individual weight gain may vary considerably between patients treated with a given antipsychotic agent, especially in the first episode of illness [1].

Although all antipsychotic drugs induce weight gain, some appear to induce more than others [2, 3], and the reasons for the differences between drugs and between patients are unclear [4, 5]. Genetic variability [6–8] and lifestyle likely play a role, but structural and functional differences in the brain's dopaminergic reward system have not been fully understood. In addition to dopamine's function in food reward and in the control of food intake [9], dopamine is also a key factor in schizophrenia and in antipsychotic action, suggesting that one may expect baseline differences in striatal structure and function to account for the variability in antipsychotic-induced weight gain.

Even though other receptors are also strongly implicated in antipsychotic weight gain [10], the dorsal striatum, rich in dopamine D2 receptors, which are antagonized by antipsychotic medications, plays an important role in the human reward

system and in appetite regulation [11–14], suggesting that altered dopamine activity in the striatal reward circuitry may contribute to increased food craving and the resultant weight gain [15]. In line with this notion, previous work has shown that decreased baseline functional activity in the putamen predicted the amount of future antipsychotic weight gain [16], and olanzapine-induced activity in the dorsal striatum was associated with excessive eating behavior [17]. Supporting the hypothesis of altered reward processing in schizophrenia, these results suggest that decreased reward anticipation in the putamen before antipsychotic treatment may predispose to weight gain under antipsychotic treatment. However, we are not aware of studies in weight gain using risperidone and aripiprazole, two widely used second-generation antipsychotic drugs (SGAs) [18].

Furthermore, in addition to altered activation in fMRI tasks, variability in striatal structure and functional connectivity may also contribute to weight gain. Indeed, previous studies have shown higher striatal volumes in addictive behavior [19, 20], as well as weaker fronto-striatal connectivity in obesity [21]. Thus, we

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Received: 20 December 2018 Revised: 14 June 2019 Accepted: 8 July 2019
Published online: 17 July 2019

measured striatal volume and striatal resting-state functional connectivity in patients with early-phase psychosis at baseline, and weight gain over the course of 12 weeks of treatment with risperidone and aripiprazole, and hypothesized that higher striatal volume and lower cortico-striatal connectivity would be associated with more weight gain [21, 22], particularly in brain regions implicated in obesity [21, 23]. We also expected that striatal volume would be higher in patients compared with controls, as found in a previous meta-analysis [24].

MATERIALS AND METHODS

Participants

We used two early-phase psychosis cohorts from two separate 12-week clinical trials, with a similar design and similar treatment effects, resulting in a sample of 81 patients. We defined early-phase psychosis as having taken antipsychotic medications for a cumulative lifetime period of 2 years or less [25, 26]. Details have been published previously [18, 25–27] and are summarized in Table 1. Written informed consent was obtained from adult participants and the legal guardians of participants younger than 18 years. All participants under the age of 18 provided written informed assent. The study was approved by the Institutional Review Board (IRB) of Northwell Health. We also included a sample of 58 age-matched healthy controls (Table 1).

Table 1. Sample characteristics

Characteristic	PSY	Mean	SD	HC	Mean	SD
Males	58		25			
Females	23		33			
Age, years	81	21.5	5.5	58	28.1	11.9
Schizophrenia	57					
Schizophreniform disorder	14					
Psychotic disorder NOS	5					
Schizoaffective disorder	2					
Bipolar I disorder (with psychotic features)	3					
Prior medication exposure (days)	74	8.4	29.6			
Medication naïve	28					
Medication 2 weeks or less	70					
Education, years	76	12.2	2.2	58	13.7	2.9
Weight, lb	81	155.1	35.2	58	156.1	28.6
BMI	81	23.6	4.4	57	24.7	4.1
BPBS total	81	42.7	7.5			
DUP, weeks	76	111.9	177.1			
L caudate, mm ³	81	4061.7	552.4	58	3922.5	590.6
L hippocampus, mm ³	81	4032.7	383	58	4107.4	455.8
L putamen, mm ³	81	6381.8	768.1	58	6316.2	843.6 ^b
R caudate, mm ³	81	4223.4	526.3	58	4103.5	588
R hippocampus, mm ³	81	4086.9	403.9	58	4201	416.2 ^a
R putamen, mm ³	81	6187.1	766.9	58	6042.7	850.4

NOS not otherwise specified, PSY early-phase psychosis patients, HC healthy controls, BPBS Brief Psychiatric Rating Scale, BMI body mass index, DUP duration of untreated psychosis

^aSignificant difference between patients and controls ($P < 0.05$) after adjusting for age, sex, age by sex, body mass index, and total intracranial volume

^bSignificant predictor of weight gain in schizophrenia ($P < 0.05$)

Weight assessments and analysis

Weight measurements were completed at baseline, and then weekly for 4 weeks, and every 2 weeks until week 12. Our analysis included the following two steps: We first estimated individual levels of weight gain with linear mixed models, which allowed us to compute individual weight gain trajectories [28]. Specifically, we used weight as the dependent variable and time (measured in days from baseline) as a continuous predictor, and included a random intercept as well as a random slope for day. The random slopes account for the fact that individuals' trajectories were allowed to vary around the group mean, thereby capturing the phenomenon of primary interest in our study: individual deviances from the average weight gain trajectory. In a second step, we extracted those random slopes, and used them as dependent variable in a linear multivariable regression, with putamen volume as predictor of interest (see below). In other words, in the second step, we tested our primary hypothesis that it was the putamen volume that was correlated with individual weight gain.

Random slopes were estimated using restricted maximum likelihood and used as the dependent variables in our structural and functional imaging analyses (see below). All analyses were conducted in R version 3.6.0. Data and code of the current study are available online at <http://github.com/philiphoman/bmi>.

Structural imaging and analysis

Magnetic resonance imaging exams were conducted on a 3-T scanner (GE Signa HDx). We acquired anatomical scans in the coronal plane using an inversion-recovery prepared 3D fast spoiled gradient (IR-FSPGR) sequence (TR = 7.5 ms, TE = 3 ms, TI = 650 ms, matrix = 256 × 256, FOV = 240 mm), which produced 216 contiguous images (slice thickness = 1 mm) through the whole brain. After image processing and segmentation with FreeSurfer 5.1.0, we measured the volumes of putamen and caudate and also included the hippocampus as control region.

We then computed multivariable regressions for each region of interest, entering the individual weight gain slopes as the dependent variable and a predictor for subcortical volume. To adjust for unspecific confounders, we included additional variables in these models, namely age, sex, the age by sex interaction, baseline body mass index (BMI), duration of untreated psychosis (DUP), and total intracranial volume.

Since our primary hypothesis was based on the previous observation that the putamen is implicated in antipsychotic weight gain [16], we first tested for an association between the average putamen volume and individual weight gain. After demonstrating this association, we used the same model for the left and right putamen separately. In a second step, we also tested for associations between the caudate and the hippocampus with individual weight gain.

Functional imaging and analysis

We performed preprocessing following recommended procedures [29], after excluding patients with motion of more than two standard deviations above the average framewise displacement (FD) [27]. In total, four participants (one female) were excluded due to excessive head motion. Their demographic variables were inspected and were similar to the patients in the included samples. We processed neuroimaging data using SPM12 and Matlab2015b and applied slice time correction, motion correction, co-registration to the T1-weighted image, segmentation, and normalization. For the segmentation and normalization steps, we segmented the T1-weighted image into six tissue types (gray matter, white matter, cerebrospinal fluid (CSF), air, bone, and background), and used the tissue-probability information to register to the ICBM152 MNI template. We then applied a high-pass temporal filter of 0.005 Hz to all voxel time courses and to the motion time courses before nuisance correction. Nuisances were then regressed out of all voxels using CompCor; this

included the 12 motion parameters (absolute and differential motion) and the CompCor components explaining at least 50% of the variance for white matter and CSF voxels [30, 31]. To identify the white matter and CSF voxels that were entered into CompCor, each participant's white matter and CSF tissue-probability maps were thresholded at >99.8% probability to avoid partial voluming. We performed the analyses both with and without global signal regression. After nuisance regression, functional images were spatially smoothed (6-mm FWHM) and temporally filtered (bandpass 0.01–0.1 Hz). To create connectivity maps, the first time points were removed to allow for signal stabilization. Time courses were then extracted for the putamen seeds, which represented functionally distinct anatomical subregions of the striatum [32].

Since our structural analysis revealed a single region to be predictive of weight gain, i.e., the left putamen, we used this as a seed region in our functional connectivity analysis. Adopting the approach by Di Martino et al. [32], $4 \times 4 \times 4$ -mm spheres were defined in the subregions of the left putamen, including the dorsal rostral putamen ($x = -25, y = 8$, and $z = 6$), dorsal caudal putamen ($x = -28, y = 1$, and $z = 3$), and ventral rostral putamen ($x = -20, y = 12$, and $z = -3$). We computed correlation maps for each participant for all three of our seeds by extracting mean activity time courses from each seed region, and by calculating whole-brain voxelwise correlation maps with the extracted waveform as a reference. The resulting correlation maps were z-transformed.

Mean framewise displacement was also included to control for the residual effect of head motion. Given that use of the data scrubbing to eliminate motion-related artifact offers little advantage over group-level corrections and can correct the data incompletely [33], we accounted for head motion at the group level by including mean FD as a nuisance covariate [33, 34]. Group-level analyses were performed independently for each seed with SPM12. For each seed, all maps were entered into a general linear model with age, sex, FD, and baseline BMI as covariates, and the individual weight gain slope as the outcome measure. Significance was defined voxelwise at $P < 0.001$, with familywise error cluster correction at $P < 0.05$. We also included a region of interest analysis for brain areas implicated in obesity, including the dorsal anterior cingulate cortex (dACC; $x = 3, y = -12, z = 36$), right insula (rInsula; $x = 38, y = 18, z = -3$), right orbitofrontal cortex (rOFC; $x = 34, y = 30, z = -3$), and right superior frontal gyrus (rSFG; $x = 22, y = 25, z = 51$). For this analysis, we created

spherical masks with a radius of 4 mm and extracted the mean beta estimates for each region of interest.

Finally, we combined the structural and functional indices of the striatum and their relationship with individual weight gain in an exploratory analysis. We therefore added functional connectivity indices, as well as the functional connectivity by volume interaction to an extended regression model, with individual weight gain slopes as the dependent measure. This model allowed us to test whether striatal volume moderated the association between fronto-striatal connectivity and weight gain. We then performed a leave-one-out cross-validation to derive a measure of predictive performance of this full model, and calculated the percentage of variance explained after cross-validation.

RESULTS

Baseline differences

Patients and controls had similar weights and BMIs at baseline (that were also within the normal range; Table 1). There was no evidence that the duration of untreated psychosis correlated with BMI ($\beta = -0.01, t(62) = -0.14, P = 0.89$) or that prior exposure to antipsychotics (before entering the trial) was correlated with baseline BMI ($\beta = 0.19, t(68) = 1.59, P = 0.117$). Furthermore, non-drug-naïve patients did not have a higher baseline BMI ($M = 23.58, SD = 4.49$) compared with healthy controls ($M = 24.68, SD = 4.08$). The left hippocampal volume was significantly reduced in patients compared with controls (Table 1), but no significant case-control differences were observed for any of the striatal regions.

Striatal volume and weight gain

We found that the average putamen volume was positively correlated with individual weight gain ($\beta = 0.31, t(68) = 2.13, P = 0.036$). Note that this finding did not depend on the selection of covariates we included in our model. To demonstrate this, we also tested for a correlation between putamen volume and individual weight gain, after adjusting for intracranial volume ($\beta = 0.34, t(78) = 2.63, P = 0.01$; Fig. S1a). Although it is good practice to adjust such an analysis for at least intracranial volume, the correlation (while difficult to interpret) is present even without this adjustment ($\beta = 0.36, t(79) = 3.43, P < 0.001$; Fig. S1b).

To explore the contributions of the left and right putamen separately, we then repeated this analysis with the left and right putamen volumes. The left putamen volume correlated with the magnitude of weight gain (Fig. 1a) in patients ($\beta = 0.31$,

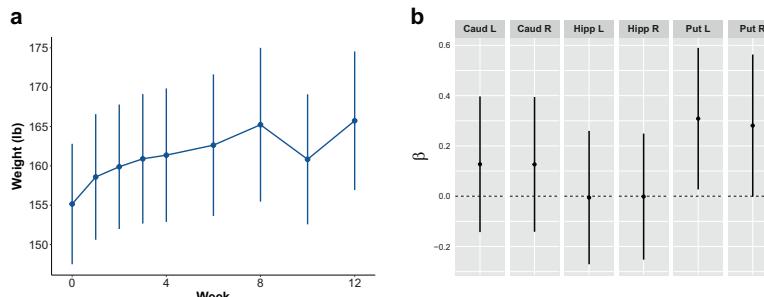


Fig. 1 Striatal volume correlates with weight gain in early-phase psychosis patients ($N = 81$). **a** Weight increased significantly over the course of the trial. Means with 95% confidence intervals are shown. **b** Weight gain in patients correlated with left putamen volume. Standardized beta coefficients with 95% confidence intervals are shown. All models were adjusted for age, sex, age by sex, duration of untreated psychosis, intracranial volume, and body mass index; none of which were significant predictors of weight gain. Error bars not touching the zero line indicate significant effects ($P < 0.05$). Caud, caudate; Hipp, hippocampus; Put, putamen; L, left, R, right

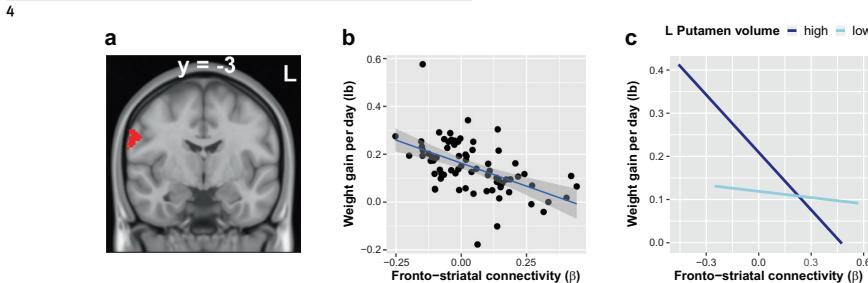


Fig. 2 Fronto-striatal connectivity and the interaction of striatal structure and function correlate with weight gain in early psychosis patients ($N = 75$). **a, b**, Lower functional connectivity between the left putamen and the lateral part of the right motor cortex correlated with weight gain. Following up on the left putamen volume finding, we tested for functional connectivity between the left putamen and the whole brain that was associated with weight gain. The left dorsal rostral putamen (red) was used as seed region, and the lateral part of the right sensory motor cortex (red) was the only connected region surviving a voxel level threshold of $P < 0.001$ and a cluster threshold of $P < 0.05$ (corrected for familywise error). **c**, Interaction of striatal structure and function correlated with weight gain in exploratory analysis. To illustrate the significant interaction between left putamen volume and connectivity, we plotted regression lines for weight gain and connectivity for the highest quartile (25%) of putamen volume and the lowest quartile (25%) of putamen volume

$t(68) = 2.18$, $P = 0.033$; Fig. 1b), with larger baseline volumes associated with greater weight gain during the subsequent trial.

Striatal connectivity and weight gain

Following up on the left putamen volume finding, we tested for functional resting-state connectivity [27] between the left putamen and the whole brain that was associated with weight gain in 75 patients. Notably, we found similar patterns of functional connectivity (Fig. S2) as previous work in striatal connectivity [32], including our own work on a subsample of the current cohort [35]. For the left dorsal rostral putamen, we found that decreased functional connectivity with the lateral part of the right sensory motor cortex pole also correlated with the amount of weight gain (Fig. 2a, b). Note that the statistical significance of this result did not depend on the inclusion or omission of the covariates we chose. Left putamen volume and fronto-striatal connectivity were not significantly correlated ($\beta = -0.05$, $t(73) = -0.39$, $P = 0.697$).

We did not find evidence that additional striatal connections with predefined regions that have been implicated in obesity (including anterior cingulate cortex, orbitofrontal cortex, and superior frontal gyrus) were associated with antipsychotic weight gain (Fig. S3).

Confirmation of results

Repeating the whole-brain analysis with global signal regressed out during preprocessing yielded essentially the same results (Fig. S4). Specifically, we found the same cluster in the right sensory motor cortex, where resting-state connectivity was negatively correlated with individual weight gain (Fig. S4b). Even though this connection did not survive cluster correction ($P = 0.065$, corrected for familywise error), it did survive correction for false discovery rate ($P = 0.036$).

Notably, the regression models were adjusted for age, sex, intracranial volume, baseline BMI, and duration of untreated psychosis; none of these variables showed a significant association with weight gain. In addition, to verify that the results were robust to the type of medication administered (risperidone vs. aripiprazole), we extended the models by including interactions of medication type and volume as well as medication type and connectivity, respectively, in the two regression models. There was no evidence for an effect of medication type on the weight gain and volume association ($\beta = 0.01$, $t(66) = 0.11$, $P = 0.916$) or the weight gain and connectivity association ($\beta = 0.07$, $t(71) = 0.46$,

$P = 0.648$). In addition, no interactions with volume or connectivity were found for prior exposure to antipsychotics and medication dose during the trial.

Striatal structure and function interaction

These results suggest that patients with higher striatal volume and more negative fronto-striatal connectivity gained more weight during the clinical trial. In an exploratory fashion, we then investigated a potential synergistic effect of striatal structure and function. Therefore, we added functional connectivity indices as well as the functional connectivity by volume interaction to an extended regression model. This model allowed us to test whether striatal volume moderated the association between fronto-striatal connectivity and weight gain. Indeed, we found evidence for an interaction of volume and connectivity ($\beta = -0.29$, $t(65) = -3.74$, $P < 0.001$; Fig. 2c), indicating that the negative relationship between fronto-striatal connectivity and weight gain was weaker with lower compared with higher striatal volumes. Note that this effect remained significant after excluding patients who had bipolar I disorder and a recent manic episode with psychotic features ($N = 3$), and after adjusting the model for type of medication, medication dose during the trial, and prior exposure to antipsychotic drugs. We then assessed how well this full model predicted future weight gain in unseen data. Because we did not have an independent data set available, we addressed this question with leave-one-out cross-validation, and found that the model explained 29% of the variance after cross-validation.

DISCUSSION

Here, we showed that striatal volume and fronto-striatal connectivity correlated with the degree of weight gain associated with antipsychotic treatment. Lower connectivity between the right sensory motor cortex and the left putamen was associated with more weight gain, and this relationship was stronger for higher compared with lower left putamen volume. In accordance with previous studies, we also found lower hippocampal volumes in patients compared with controls [36–38]. Yet, contrary to our expectation, we did not find that other brain areas implicated in obesity showed altered functional connectivity. In addition, we also did not find greater striatal volumes in patients compared with controls, possibly because our patients had only limited exposure to antipsychotic treatment.

We focused our study on the striatum, in line with this region's important role in reward processing and weight gain. Previous studies have shown that treatment with antipsychotics in healthy controls induced an increase in reward activation in the dorsal striatum that was correlated with excessive eating [17] and that attenuated reward anticipation normalizes under treatment with antipsychotics in patients with schizophrenia [39, 40]. However, although altered reward processing is likely to play a role [16], antipsychotic-induced weight gain remains a complex issue [4]. First, one could ask why patients did not gain weight before the antipsychotic treatment if baseline alterations in reward processing are the main cause for weight gain. Indeed, there is evidence suggesting that patients might show metabolic aberrations already before they start treatment, possibly due to unhealthy lifestyle, illness neurobiology, and genetic factors [41–44]. Furthermore, it is important to consider that some patients in the current study were not naïve to antipsychotic drugs but had been exposed to antipsychotic treatment prior to inclusion. Thus, weight gain may have occurred during this time frame. Nevertheless, we did not find evidence that baseline BMI correlated with days of prior drug exposure or that non-drug-naïve patients had a higher baseline BMI compared with controls, speaking against substantial weight gain before the trial. The current trial of 12 weeks, although short, thus appears long enough to induce substantial weight gain with considerable inter-subject variability, especially in subjects with minimal prior antipsychotic exposure [1]. Second, one might ask how antipsychotics and some of their counterparts, dopaminergic agonists, can both induce weight gain, as shown for Parkinson disease [45]. A possible explanation is that weight gain in Parkinson disease is triggered mainly by compulsive eating [45, 46] whereas a decreased reward experience may be the main cause for weight gain induced by antipsychotics. It is also noteworthy that increased dopamine transmission through sympathomimetics, such as amphetamine, typically results in suppressed appetite.

One mechanism that likely contributes to weight gain is thus via altered reward processing in the striatum. The striatum is part of the brain's reward circuitry, which has well-known dopaminergic components as well as neurotransmitters, including opioids, serotonin, and cannabinoids [9, 12, 17, 47, 48]. Accordingly, food consumption increased striatal dopamine in a previous study using positron emission tomography and this increase was positively correlated with meal pleasantness [12]. In addition, food cues elicited dorsal striatum dopamine increase and this increase correlated with hunger perception and food desire [49]. In obesity, dopamine has also been implicated in food reward and the control of eating behavior [50–52]. More specifically, functional magnetic resonance imaging studies in obesity have found altered activation in dopaminergic reward-related areas. Although these studies have described enhanced striatal activation in response to food-related cues [13, 53–55] the actual consumption of food elicited less activation in these areas [53, 54]. As previously suggested [17], these patterns are consistent with an imbalance between (increased) reward expectation and (decreased) reward experience, most likely through dopaminergic disruptions. Overeating could then be seen as a compensation to obtain the anticipated exaggerated reward [17].

Our study extends this model by considering the functional connectivity between the striatum and the sensory motor cortex, which has been implicated in food cue-related activity in obesity [13, 53, 54, 56, 57]. Our study, however, found that the strength of this connectivity was correlated with less weight gain. Furthermore, we found that the association with weight gain was moderated by left putamen volume, with higher volumes together with lower connectivity correlating with more weight gain. The moderating role of the putamen volume suggests a synergistic effect of striatal structure and function, with increased putamen volumes, possibly due to reduced endogenous dopamine

availability in the striatum [19, 20], multiplying the effect of lower cortico-striatal connectivity on weight gain [21, 22].

Some limitations merit comment. First, although the performance of the striatal structure and function model in predicting weight gain was promising (29% of the variance explained after cross validation), the sample size was small and we could not test the model performance in an independent data set. In addition, the functional connectivity measure that entered this model was not an independent measure but likely an overly optimistic estimate of the effect size of the fronto-striatal connectivity [58]. Nevertheless, if replicated in a larger sample, these data may help in the identification of patients at high risk for weight gain upon initiation of antipsychotic treatment. Furthermore, we used two different SGAs in this study, namely risperidone and aripiprazole. Although it is important to underscore that all current antipsychotics share affinity for the dopamine D2 receptors, they may still differ from a pharmacological point of view. For example, risperidone and aripiprazole share D2 receptor antagonism to induce an antipsychotic effect [59] but different receptor systems may be involved in side effects, such as weight gain. Apart from the striatum [16] weight gain has been associated with cortical 5-HT2A receptors in quetiapine monotherapy [60]. In addition, olanzapine exposure in healthy controls indicated negative effects on the peripheral metabolism. These findings suggest that antipsychotic-induced weight gain may involve additional central and peripheral aspects apart from the striatum. However, in the current study we did not find evidence that the type of medication interacted with the effects of striatal volume or fronto-striatal connectivity, suggesting similar effects of both drugs, which is also consistent with our previous finding for a larger trial [18] where no significant differences on weight gain were found between aripiprazole and risperidone. In addition, weight gain differences between aripiprazole and risperidone found in previous studies might reflect a difference in the sedative effect, with risperidone causing more sedation than aripiprazole [2]. Another limitation is that we used three putamen seeds, which we considered separately, thus not correcting for multiple comparisons across seeds. In addition, early-phase psychosis could have been defined differently, for instance, using the duration of untreated psychosis. However, consistent with our previous work, we used a criterion of cumulative exposure to antipsychotic treatment [18, 25, 26, 61]. Finally, future studies with repeated measurements of structural and functional imaging should investigate whether our findings reflect indeed a trait marker in patients prone to weight gain.

In conclusion, the current study showed that striatal structure and function correlated with antipsychotic-induced weight gain. Lower connectivity between the striatum and the sensory motor cortex was associated with more weight gain, and this relationship was stronger for higher compared with lower left putamen volumes in an exploratory analysis. This suggests that an imaging marker at baseline may identify patients who are prone to substantial weight gain during treatment.

FUNDING AND DISCLOSURE

Presented at the 57th Annual Meeting of the American College of Neuropsychopharmacology (ACNP). Supported by NIMH grant P50MH080173 to Dr. Malhotra, grant R01MH060004 to Dr. Robinson, grant R01MH076995 to Dr. Szczek and R21MH101746 to Drs. Robinson and Szczek. Dr Robinson has been a consultant to Costello Medical Consulting, Innovative Science Solutions, Janssen, Lundbeck, Otsuka, and US WorldMeds and has received research support from Otsuka. Dr Malhotra has served as a consultant for Forum Pharmaceuticals and has served 52 on a scientific advisory board for Genomind. Dr Lencz is a consultant for Genomind. The other authors report no financial relationships with commercial interests. The authors declare that they have no conflict of interest.

ACKNOWLEDGEMENTS

The authors thank Dr Lauren Hanna and Dr Juan Gallego for their careful clinical oversight of the study. They acknowledge their patients, their patients' families, and their psychiatry research support staff.

ADDITIONAL INFORMATION

Supplementary Information accompanies this paper at (<https://doi.org/10.1038/s41386-019-0464-y>).

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Supplementary Information

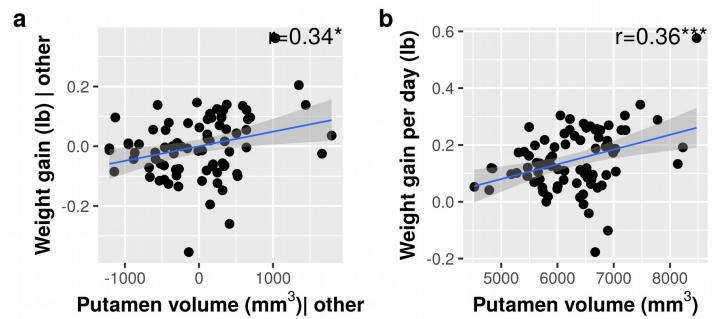


Figure S1: a, b. Positive correlation between putamen volume and individual weight gain. The correlation was present after adjusting for total intracranial volume (a) but also in a correlation without this adjustment (b). Linear regressions with 95% confidence bands are shown.

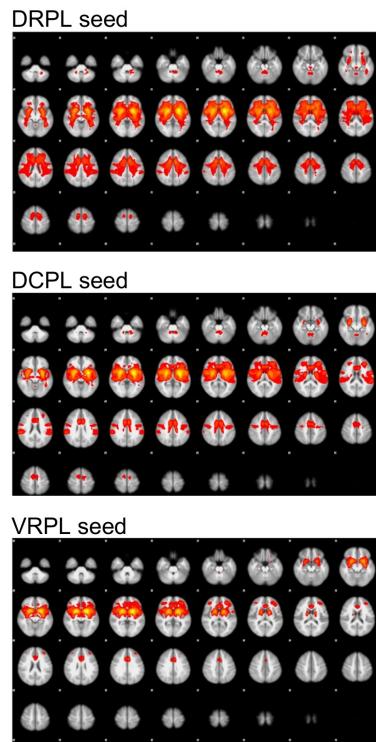


Figure S2: Striatal connectivity between seeds in the left putamen and the whole brain. We used the left dorsal rostral putamen (DRPL), the left dorsal caudal putamen (DCPL), and the left ventral rostral putamen (VRPL) as 4mm-seeds and computed voxel-wise wholebrain connectivity. Images are thresholded at $P < 0.0001$, uncorrected for visualization purposes.

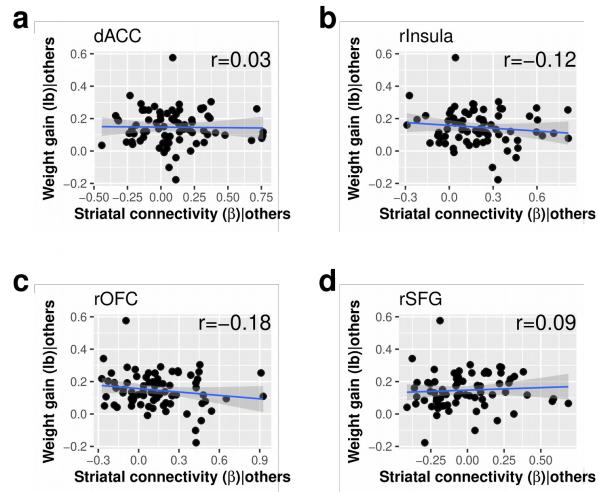


Figure S3: a-d. Striatal connectivity between left putamen and anatomically constrained brain regions implicated in obesity as predictors of weight gain. Partial correlations with 95% confidence bands are shown, after adjustment for age, sex, and BMI. Abbreviations: dACC, dorsal anterior cingulate cortex; rOFC, right orbitofrontal cortex; rSFG, right superior frontal gyrus.

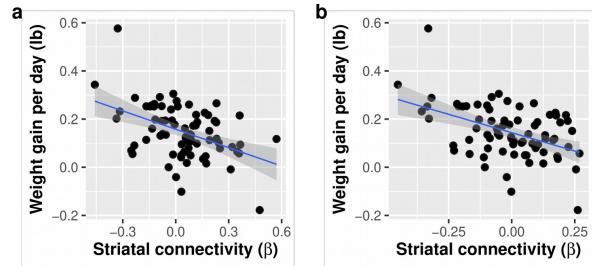


Figure S4: a, b. Similar fronto-striatal connectivity with and without global signal regressed out.
We extracted the beta weights for the connection between the left putamen and the lateral part of the right sensory motor cortex and saw comparable results. a., Without global signal regressed out during preprocessing. b., with global signal regression. Linear regressions with 95% confidence bands are shown.

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Neural computations of threat in the aftermath of combat trauma

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By combining computational, morphological, and functional analyses, this study relates latent markers of associative threat learning to overt post-traumatic stress disorder (PTSD) symptoms in combat veterans. Using reversal learning, we found that symptomatic veterans showed greater physiological adjustment to cues that did not predict what they had expected, indicating greater sensitivity to prediction errors for negative outcomes. This exaggerated weighting of prediction errors shapes the dynamic learning rate (associability) and value of threat predictive cues. The degree to which the striatum tracked the associability partially mediated the positive correlation between prediction-error weights and PTSD symptoms, suggesting that both increased prediction-error weights and decreased striatal tracking of associability independently contribute to PTSD symptoms. Furthermore, decreased neural tracking of value in the amygdala, in addition to smaller amygdala volume, independently corresponded to higher PTSD symptom severity. These results provide evidence for distinct neurocomputational contributions to PTSD symptoms.

On returning from combat, why do some military personnel develop symptoms of PTSD and others do not? PTSD symptoms may develop after exposure to a traumatic event and are characterized by symptoms of re-experiencing, avoidance of trauma reminders, negative alterations in cognitions and mood, and alterations in arousal and reactivity^{1–3}. A prominent learning theory suggests that PTSD symptoms largely reflect maladaptive associative learning during and after a traumatic event⁴. Associative learning of threat⁵ is the process by which benign stimuli such as people, locations, and objects (that is, conditioned stimuli) acquire threatening properties through pairing with an aversive outcome, and have the capacity to trigger and maintain defensive responses well after the aversive event is decoupled from the conditioned stimuli. Although abnormal threat conditioning features prominently in theoretical accounts of PTSD, the manner in which learning becomes dysfunctional is less clear⁵.

Accumulating evidence suggests a variety of impaired learning processes in PTSD, including overgeneralization, heightened contextual anxiety, diminished inhibition in response to safety cues, and failure to retain extinction learning⁶. These findings link PTSD to basic learning processes, but they do not disambiguate specific aspects of learning that may contribute to the disorder, such as the learning rate or the computation of aversive value. It is possible that PTSD-related abnormalities are influenced by learning parameters that we cannot directly observe, but are able to infer from observable behavior. Computational indices, which estimate such latent learning parameters, may be able to detect such differences.

Theories of associative learning, such as the Pearce–Hall learning mechanism⁷, envision that learning cue-outcome associations

involves tracking of several quantities: prediction errors for reinforcement, which occur when the outcome is more or less than expected (that is, surprising), and associability, reflecting the extent to which each cue has been previously accompanied by surprise. The value assigned to cues in the environment is revised in each encounter based on the prediction error. Associability dynamically guides value learning by accelerating it to cues whose predictions are poor (large prediction errors), and decelerating it when predictions become reliable. Here, we used a hybrid version of the Pearce–Hall learning model to estimate the computations performed during associative threat learning^{7–9} and how the behavioral and neural tracking of these computations relate to PTSD symptom severity.

To cover the full spectrum of symptomatology, we recruited 54 combat-exposed veterans with a wide range of PTSD symptoms based on the gold-standard structured clinical interview for PTSD, the Clinician-Administered PTSD Scale (CAPS; Table 1 and Supplementary Fig. 1). Twenty-four participants had a diagnosis of PTSD and 30 participants were combat veteran controls without PTSD diagnosis. We used the threat reversal paradigm—where flexible updating of threat responses is required—together with computational modeling, to uncover latent learning parameters that are relevant for the symptomatology.

We expected that the observed threat learning behavior would be similar across different levels of symptoms (reflecting the unspecific and subtle aberrations found in threat response conditioning in PTSD in general^{10,11}) but that the underlying neural computations might reveal disease-related differences.

The amygdala is a locus of associative learning in the brain^{7,12}, and previous work has linked PTSD symptoms with abnormal

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Table 1 | Sample characteristics

Characteristic	N	Mean	s.d.
Males	49		
Females	5		
Age	54	32.8	8
Education	51	3.7	1.2
ASI	52	20.7	13.3
BDI	53	16	13.3
CAPS	54	39.2	32
CES	51	16.8	6.5
STAIS	49	40.8	13.7
Medicated	18		
Comorbidities:			
MDD	17		
Past alcohol abuse	7		
Panic disorder	5		
Past cannabis abuse	4		
Generalized anxiety	3		
Social phobia	3		
Anxiety disorder NOS	2		
Adjustment disorder	1		
Anxiety disorder GMC	1		
Dysthymic disorder	1		
Other DSM-IV Axis I disorder	1		
Past cocaine abuse	1		
Past opiates abuse	1		
Specific phobia	1		

Education was a categorical variable, defined as: 1, 8th grade or less; 2, some high school; 3, high school graduate or General Education Diploma; 4, some college; 5, college graduate; 6, advanced graduate degree. Abbreviations: ASI, Anxiety Sensitivity Index; BDI, Beck Depression Inventory; CES, combat exposure score; STAIS, State Anxiety subscale of the Spielberger State-Trait Anxiety Inventory; MDD, major depressive disorder; NOS, not otherwise specified; GMC, due to general medical condition; s.d., standard deviation; DSM, Diagnostic and Statistical Manual of Mental Disorders.

amygdala structure¹³, as well as heightened amygdala reactivity to stimuli laden with emotionally negative content^{14,15}. Our goal here was thus to examine whether the structural and functional implementations of specific learning computations in the amygdala relate to PTSD symptoms, and whether the threat learning-related function and volume of the amygdala contribute to PTSD symptoms in a complementary manner.

The experiment began with an acquisition phase, in which two visual stimuli (mildly angry faces) were presented consecutively in a pseudorandomized order. One of the stimuli was paired with a mild electric shock on one-third of the trials (face A), while the other was never paired with the shock (face B). The acquisition phase was immediately followed by a reversal phase, in which the contingencies were flipped such that the formerly neutral stimulus (face B) was now paired with a shock and face A became the neutral stimulus (Fig. 1a). Skin conductance response (SCR) served as the index of conditioned defensive responses.

Results

Irrespective of symptoms, veterans show successful reversal learning. Combat-exposed veterans ($N=54$ participants) successfully acquired and reversed threat conditioning, as assessed by the differential SCR (face A versus face B) in the two phases of the task (Fig. 1b). To test for a potential relationship between threat reversal

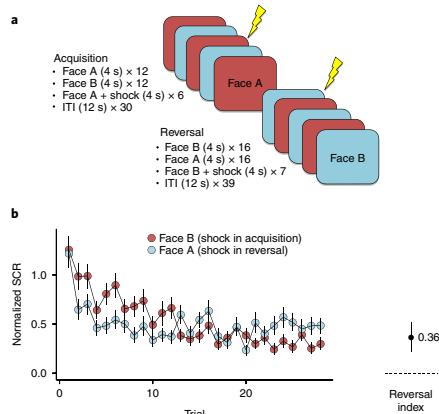


Fig. 1 | Experimental overview. **a**, Experimental design. The experiment consisted of 69 trials and a reinforcement rate of 33%. Stimuli were presented for 4 s in two pseudorandomized orders, followed by an intertrial interval of 12 s. During acquisition, face A was paired with a shock in about one-third of the trials and face B was not paired. Reversal started after 30 trials without previous instructions or warnings. During reversal, face B was now paired with a shock in about one-third of the trials, whereas face A was not paired anymore (ITI, intertrial interval). **b**, Time course of threat reversal learning. Mean normalized SCRs with standard errors ($N=54$ participants). Participants showed successful threat reversal, indicated by a significant interaction of stage by stimulus; that is, a reversal index (subtracting stimulus discrimination (face A–face B) in reversal from stimulus discrimination in acquisition) with 95% confidence intervals that is significantly different from zero in a one-sample t test, two-sided ($t(53)=4.75$, $P<0.001$).

and PTSD symptoms, we used a linear regression with threat reversal index as predictor and CAPS scores as the outcome. Reversal index was calculated by subtracting stimulus discrimination in reversal (that is, face A minus face B) from stimulus discrimination in acquisition (Fig. 1b). Controlling for irrelevant variables (age and gender), the regression revealed no significant relationship between symptoms and reversal learning ($\beta=0.02$, $t(50)=0.13$, two-tailed, $P=0.894$). We also did not find evidence that PTSD symptoms were related to stimulus discrimination during threat acquisition only ($\beta=0.03$, $t(52)=0.22$, two-tailed, $P=0.827$) or during the reversal phase only ($\beta=0.02$, $t(52)=0.12$, two-tailed, $P=0.901$). Additional ways of categorizing veterans as highly and mildly affected did not reveal any significant results (see Methods, ‘Sample characteristics’). These results motivate the use of a computational approach that could potentially reveal latent learning differences across individuals exposed to combat trauma.

Pearce–Hall hybrid model best describes conditioned threat responses. To estimate parameter weights for the specific computations performed during associative threat learning^{16,17} and how they relate to PTSD symptom severity, we used a hybrid Rescorla–Wagner and Pearce–Hall model, which we have previously employed with the same task in healthy participants^{18,19}. The computational model was informed by the Pearce–Hall learning

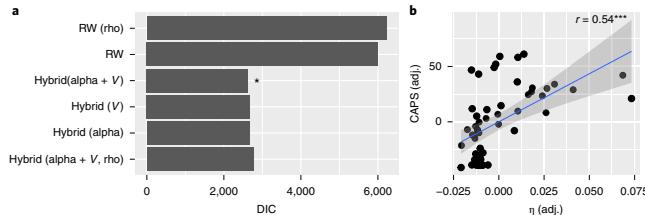


Fig. 2 | Computational model comparison and relationship to PTSD symptoms. **a**, All three versions of the hybrid model informed by the Pearce–Hall learning mechanism outperformed the simpler Rescorla–Wagner (RW) model. In addition, the hybrid model with associability and value outperformed the models with either value only or associability only and was thus the winning model (indicated with an asterisk). An extension of the Rescorla–Wagner or hybrid model with a scaling parameter ρ for the reversal stage, reflecting the potential change of learning during the reversal stage, did not perform better than the hybrid model with alpha, (associability), and V (value), which we thus kept as the winning model. **b**, Prediction-error weight η predicts symptoms as assessed with the CAPS. Using the best-fit model parameters, we found that a higher prediction-error weight η (which captures the learning rate for associability) predicted more CAPS symptoms. A partial correlation is shown after adjustments for age and gender and a Pearson correlation coefficient with a two-tailed significance test. Error shadings correspond to standard errors. adj., adjusted for all other parameters in the model; *** $P < 0.001$.

mechanism for associability-gated learning⁶. Like the classic Rescorla–Wagner model¹⁶, the hybrid model updates the value of each cue on each presentation of that cue, based on the discrepancy between the expected and obtained outcome, or the prediction error. The hybrid model, however, replaces the constant learning rate of the Rescorla–Wagner model by a dynamic associability parameter⁶. Associability reflects the attention that a cue receives on the basis of how accurately it has predicted outcome in the past. Unreliable cues receive more attention (higher associability) as they are likely to be unreliable in the future; and since they are unreliable, they should be updated preferentially as new information becomes available¹⁷ (see Supplementary Material for details and for simulated parameter recovery as well as model fits and Supplementary Figs. 2 and 3).

First, to verify the suitability of the model, we conducted model comparison between several versions of reinforcement learning models. Using hierarchical Bayesian modeling we fitted three different versions of this hybrid model to the SCR data; all three outperformed the simpler Rescorla–Wagner model (deviance information criterion, DIC: 6003.91). In addition, the hybrid model with associability (α) and an additional predictor for value (V) updating (DIC: 2630.37) outperformed the models with either value alone (DIC: 2678.75) or associability alone (DIC: 2661.6) and was thus the winning model (Fig. 2a). There was no evidence that an additional scaling parameter for the reversal stage (reflecting a different prediction-error weight for the reversal stage) improved the model fit (DIC: 2772.27). Notably, similar results were obtained when using maximum likelihood estimation (MLE) as in a previous study (ref.); see Supplementary Material for details and Supplementary Fig. 4). These findings indicate that the recorded SCRs during reversal learning reflect value expectations modulated by cue-specific attention. Next, we used this winning hybrid ($\alpha + V$) model to examine whether learning parameters that describe behavior and neural activity relate to PTSD symptom severity.

Symptomatic veterans assign higher weights to prediction errors. To understand how the model computations relate to overt PTSD symptoms, we used the best-fit model parameters. In the winning hybrid model, the prediction-error weight η , which can be seen as a learning rate for associability, is a quantity estimated for each participant from the SCR. The prediction-error weight quantifies how much weight is assigned to wrong predictions when updating trial-by-trial associability. It is possible that more symptomatic combat

veterans would be more sensitive to prediction errors, and will assign higher weights to them. Indeed, we found that higher prediction-error weight was associated with higher CAPS symptoms ($\beta = 0.55$, $t(50) = 4.57$, two-tailed, $P < 0.001$; Fig. 2b; note that this association held up when using a non-parametric rank correlation test that is more robust to outliers, see Supplementary Material). This finding suggests that highly symptomatic combat veterans were more influenced by prediction errors, weighing them more strongly as they adjusted trial-by-trial attention to cues.

Symptomatic veterans show altered amygdala value computation. During the reversal task, the value assigned to each cue is continuously updated on the basis of associability-gated prediction error. Mathematically, a value in a current trial reflects the value in the previous trial plus prediction error multiplied by associability. Associability in each trial is updated by the weighted prediction error in the previous trial (see Supplementary Material for details). As reported above, the prediction error weight was positively associated with PTSD symptoms. As the weighted prediction error shapes value, we next examined whether the neural tracking of value related to PTSD symptoms.

We focused our neural investigation on the amygdala, given its role in associative learning¹², value encoding^{18–21}, and evidence linking PTSD symptoms with heightened amygdala reactivity to emotionally negative stimuli^{4,13}. Given that amygdala morphology has also been linked with stress-related psychopathology^{13,22}, we examined whether amygdala neural computations and morphology are different manifestations of the same source problem (that is, redundant) or whether they incrementally explain variance in PTSD symptoms.

To address this, we calculated linear regression models including functional (value encoding based on the winning hybrid model) and structural indices for amygdala as predictors of the PTSD symptoms (for a similar analysis using the classic Rescorla–Wagner model, see Supplementary Figs. 5–7). To account for unspecific intersubject variability, these models were adjusted for age, gender, head movement and total intracranial volume (see also Supplementary Material). We found a structure-function relationship with CAPS in the right amygdala (Fig. 3a), where both volume ($\beta = -0.52$, $t(47) = -2.02$, two-tailed, $P = 0.01$; Fig. 3b) and neural activity ($\beta = -0.29$, $t(47) = -2.02$, two-tailed, $P = 0.049$; Fig. 3c) independently predicted the total CAPS score. In the left amygdala, the effect of value-dependent activity remained significant

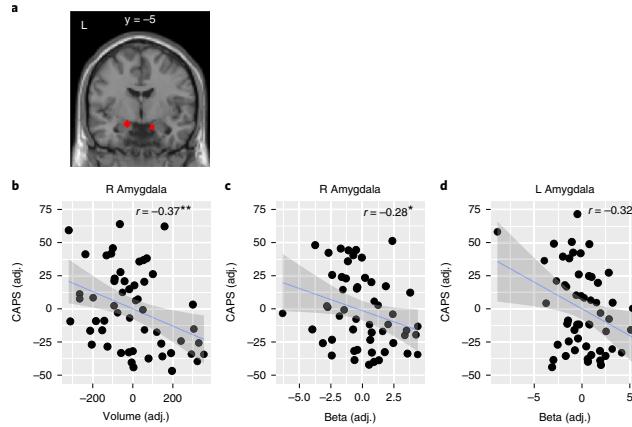


Fig. 3 | Amygdala structure and value computation contribute to PTSD symptoms using a hybrid computational model of associability and value encoding. **a**, Region of interest (ROI) used in the computational imaging analysis. The amygdala (red) was defined functionally, using the contrast of conditioned stimuli (both CS+ and CS-) versus baseline. **b-d**, Amygdala volume and value-dependent neural activity independently contribute to PTSD symptoms. Partial correlations are shown ($N=54$ participants) with Pearson correlation coefficients and two-tailed significance tests. Right amygdala volume and activity as well as left amygdala activity correlated negatively with PTSD symptoms as measured with CAPS. Thus, lower value tracking in the amygdala and smaller amygdala volume correspond to higher symptom severity. Regressions were adjusted for age, gender, head movement, and total intracranial volume. Error shadings correspond to standard errors. adj., adjusted; ** $P < 0.01$; * $P < 0.05$.

when including amygdala volume in the same model ($\beta=-0.34$, $t(47)=-2.34$, two-tailed, $P=0.024$; Fig. 3d), but no independent effect for volume emerged ($\beta=-0.25$, $t(47)=-1.27$, two-tailed, $P=0.211$).

We verified that the findings were comparable when restricting the study sample to the male participants and when using non-parametric rank correlations (see Supplementary Material). We also confirmed that individual differences in right amygdala volumes did not affect the effect of neural activity on CAPS symptoms (see Supplementary Material and Supplementary Fig. 8).

To further characterize the relationship between structure and function we added the interaction term to the model and found that there was no evidence for a synergistic effect between these independent variables (right amygdala: $\beta=-0.95$, $t(46)=-0.88$, two-tailed, $P=0.385$; left amygdala: $\beta=-0.91$, $t(46)=-0.78$, two-tailed, $P=0.437$). However, the correlation between structure and function (adjusting for total head volume) was significant and negative (right amygdala: $\beta=-0.39$, $t(49)=-2.11$, two-tailed, $P=0.04$; left amygdala: $\beta=-0.39$, $t(49)=-2.13$, two-tailed, $P=0.038$). A possible explanation is a compensatory recruitment of amygdala neurons in veterans with smaller amygdala volumes, probably due to a stress-related gray matter reduction²³.

To fully characterize the brain-behavior relationship with respect to symptoms, we tested whether individual differences in prediction-error weights were associated with differences in amygdala volume. We entered the prediction-error weight (η) as an outcome measure into a linear regression and used amygdala volume as predictor, including additional regressors for age, gender and total intracranial volume. We did not find evidence that right amygdala volume ($\beta=-0.22$, $t(49)=-1.14$, two-tailed, $P=0.26$) or left amygdala volume ($\beta=-0.19$, $t(49)=-1$, two-tailed, $P=0.321$) were associated with prediction-error weight.

In addition to value computation, the winning hybrid model also captures prediction error and associability, both of which are associated with amygdala neural activity^{8,9,17}. Since they are not strongly correlated in the hybrid model^{2,3}, they can be assessed separately (see also Supplementary Material and Supplementary Fig. 9). We therefore computed a second general linear model (GLM) with trial-by-trial regressors for associability, shock occurrence, and prediction error, all of which were parametric modulators of cue offset, as this is the time point when prediction error and associability are computed. We expected that tracking of associability in the amygdala¹⁷, reflecting the proposed attention-gating role of this brain region, would be attenuated by PTSD symptoms. However, we did not find evidence for a relationship between amygdala neural activity and PTSD symptoms for either associability (left: $\beta=-0.14$, $t(47)=-1.01$, two-tailed, $P=0.316$; right: $\beta=-0.06$, $t(47)=-0.43$, two-tailed, $P=0.667$) or prediction error (left: $\beta=-0.03$, $t(47)=-0.2$, two-tailed, $P=0.839$; right: $\beta=0.04$, $t(47)=0.28$, two-tailed, $P=0.781$), suggesting that amygdala value encoding contributes to the symptoms of PTSD, whereas associability and prediction error were less influential.

All together, these findings show that lower neural tracking of value in the amygdala, in addition to smaller amygdala volumes, corresponded to higher PTSD symptom severity.

Additional brain regions tracking threat computations. The striatum, the hippocampus and the dorsal anterior cingulate cortex (dACC) have also been implicated in the computations related to threat learning^{24,25}. We extended our analysis to these brain regions and tested whether neural tracking of value, associability and prediction error in these regions correlated with PTSD symptoms. Using a linear mixed model with brain region and CAPS as factors and neural value computations as an outcome, we found a main

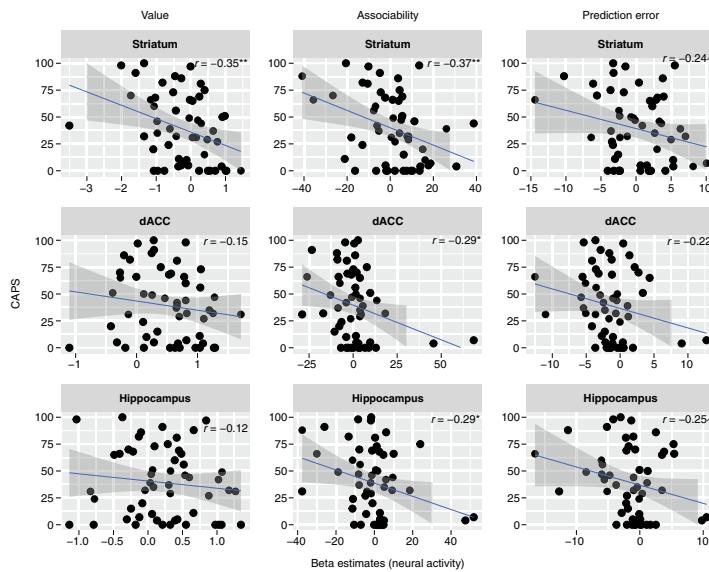


Fig. 4 | Neural computations of value, associability and prediction error and their relationship to CAPS symptoms for different ROIs. We found negative correlations for value encoding as well as associability that were attenuated for prediction error ($N=54$ participants). Pearson correlation coefficients are shown with two-tailed significance tests. Error shadings correspond to standard errors. ** $P<0.01$; * $P<0.05$; ~ $P<0.1$.

effect of CAPS ($F(1, 52)=5.49, P=0.023$) as well as an interaction of brain region and CAPS ($F(2, 104)=3.14, P=0.047$), driven by significant negative correlations between value tracking in the striatum (Fig. 4). These results suggest that, similar to amygdala, lower value tracking in the striatum (but not hippocampus or dACC) relates to higher symptom severity.

To test for a relationship between PTSD symptoms and neural tracking of associability and prediction error in these regions, all of which have been implicated in prediction error^{20,21} and associability^{25,38,39} encoding, we computed a linear mixed model with brain region, learning component and CAPS as factors, and neural activity as the dependent variable. We found an interaction of learning component and CAPS ($F(1, 208)=20.43, P<0.001$), driven by negative correlations between neural tracking of associability and CAPS that were attenuated for prediction error in all three regions (Fig. 4). We confirmed that these findings for value, associability and prediction-error computation were robust to the gender imbalance, clinical heterogeneity and medication status (see Supplementary Material). We also verified that the correlations were present when testing for non-parametric rank correlations. These results indicate that the lower tracking of associability (and less so of prediction error) in the striatum, hippocampus and dACC relate to higher symptom severity.

Finally, to investigate a dissociation of associability and prediction error in amygdala and striatum as reported in a previous study⁴, we tested for an interaction of region (amygdala, striatum) and learning component (associability, prediction error). To improve

comparability between the current and the previous study, we ran this analysis only in veterans without a diagnosis of PTSD, and found no evidence for dissociation ($F(3, 203)=0.58, P=0.629$). We also did not find evidence that the amygdala tracked associability in the current study in veterans without PTSD.

The absence of a dissociation that was found in a previous study⁴ merits an explanation. First, it is noteworthy that the current study does replicate the computational results of the previous study⁴, namely the superiority of the hybrid model over the Rescorla-Wagner model. On the neural level, the previous study found an interaction of region (amygdala, striatum) and learning component (associability, prediction error) that had a medium to large effect size (Cohen's $d=0.66$, 95% confidence interval (CI): 0.12; 1.17, $t(16)=2.71$, one-sample t -test, two-tailed, $P=0.02$). In the current study, we found that this interaction was not significant (Cohen's $d=-0.13$, 95% CI: -0.49; 0.23, $t(29)=-0.7$, one-sample t test, two-tailed, $P=0.49$). Rather, the striatum, but not amygdala, tracked associability in addition to tracking prediction error.

Several factors could explain this result. First and foremost, the current study's population was exposed to combat trauma, therefore meeting criterion A (exposure to a traumatic event) in the clinical assessment of PTSD symptoms and, in addition, was exposed to chronic stress associated with a deployment to combat zone. One may speculate that this traumatic stress (which has been shown to affect amygdala functioning^{13,22,30}) may be the root cause for a shift in tracking from the amygdala to the striatum as part of brain plasticity. Second, the current sample differed significantly from the

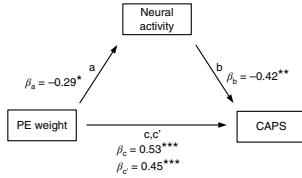


Fig. 5 | Associability-related neural activity in the right striatum partially mediates the relationship between prediction-error weights and CAPS. Standardized regression coefficients are shown ($N=54$ participants) and their statistical significance tested with one-sample t tests, two-tailed. Both prediction-error weights and striatal neural tracking of associability independently predicted PTSD symptoms as measured with CAPS when included as predictors in the same model. PE, prediction error; *** $P<0.001$; ** $P<0.01$; * $P<0.05$.

previous sample in terms of gender ratio (M/F=49/5 in the current study versus 9/8 in Li and colleagues; $P=0.001$) and age range (20–52 in the current study versus 18–31 in Li and colleagues'). Alternatively, given that we ran this analysis only in veterans without a diagnosis of PTSD ($N=30$), a lack of statistical power might have contributed to the non-replication.

Brain-behavior relationship. Prediction-error weights shape the computations of value and associability. The neural underpinnings of higher prediction-error weights, observed in the behavior of individuals with more PTSD symptoms, may therefore relate to computations of value in the amygdala and the striatum, as well as to computations of associability in the striatum, dACC and hippocampus. While PTSD symptoms correlated positively with prediction-error weights, however, they correlated negatively with the neural tracking of value and associability. To better understand these inverse relationships we conducted a mediation analysis. This analysis revealed that the correlation between prediction-error weight and CAPS was partially mediated by the tracking of associability (but not prediction error) in the right striatum, as shown in the four steps of a mediation analysis (Fig. 5). We found that: (1) prediction-error weight positively correlated with CAPS ($\beta=0.54$, $t(52)=4.62$, two-tailed, $P<0.001$); (2) prediction-error weight negatively correlated with neural activity ($\beta=-0.29$, $t(52)=-2.22$, two-tailed, $P=0.03$); (3) neural activity negatively correlated with CAPS ($\beta=-0.43$, $t(52)=-3.43$, two-tailed, $P=0.001$); and (4) prediction-error weight (eta) and neural activity independently predicted CAPS ($\text{eta}: \beta=0.45$, $t(51)=3.89$, two-tailed, $P<0.001$; neural activity: $\beta=-0.3$, two-tailed, $t(51)=-2.55$, $P=0.014$). Finally, we tested whether the difference between paths c and c' was significantly different from zero. To maximize statistical power, we used non-parametric bootstrapping with 5,000 draws to derive an empirical null distribution. We found that the difference between the effect with the mediator present and the effect without it (paths c and c' ; Fig. 5) was indeed significant ($\beta=0.09$, 95% CI: 0.02; 0.2; $P=0.01$). This result indicates that our mediation model supports a significant partial mediation.

This suggests that—at least for the right striatum and associability—both higher prediction-error weights and decreased neural tracking of associability are independently related to higher CAPS symptoms. Speculatively then, the higher weight assigned to prediction errors might be a compensatory adjustment for the decreased neural tracking of associability. We did not find evidence that the neural tracking in any other region fully or partially mediated the relationship between prediction-error weights and CAPS.

Together, these findings indicate that the effect of higher prediction-error weights in individuals with higher CAPS scores was complemented by decreased striatal activity during associability computation.

Discussion

The current study found that even highly affected combat veterans were able to perform reversal learning when the SCRs were analyzed using conventional summary statistics. A more fine-grained computational analysis, however, revealed that subtle differences in latent learning components are at play: symptomatic veterans assigned more weight to prediction errors. An intuitive way of interpreting this result is in terms of attention. Highly affected individuals were more sensitive when their predictions about outcomes were wrong, and they exaggerated their adjustment to the cues that did not predict what they had expected. This behavior may be associated with the increased aversion to ambiguous losses, which was recently observed in PTSD in the context of economic decision-making. Future research will need to determine the exact relationships between decision making under uncertainty, reinforcement learning, and post-trauma symptomatology^{1,2,3}.

On the neural level, we found that the neural computations that were shaped by these altered prediction-error weights contributed to the symptoms of PTSD: aversive value encoding in the amygdala and striatum, and associability computations in the striatum, dACC, and hippocampus. Our study further indicates that the right amygdala computations contribute to the symptomatology above and beyond the effects of smaller amygdala volumes¹³, suggesting additive effects of right amygdala volume and function. A model-based functional magnetic resonance imaging (fMRI) analysis such as the one used in this study can therefore not only indicate where in the brain a certain task-related activity emerges, but also which computations are probably performed.

The implication of these findings for PTSD becomes clear when the absence of behavioral differences (as indexed by reversal learning) is considered: as is well known from the behavioral (and, to a lesser extent, from the fMRI) literature, no consistent and clinically relevant differences have emerged in threat conditioning paradigms³⁴, which is surprising given the proposed central role of threat conditioning in the pathophysiology of PTSD'. A possible explanation is that behavioral measures, for example, SCR, are noisy and can indeed be interpreted as noisy realizations of deterministic learning models³⁵. This suggests that the differences that are relevant for the disease may in fact be reflected by the latent parameters of the generative model rather than the noisy behavioral data.

Although all veterans were combat-exposed, only some of them developed symptoms strong enough to warrant a classical (DSM based) PTSD diagnosis. While our results do not allow us to draw causal inferences, our data do support the notion that veterans may develop more severe PTSD symptoms in response to altered neural computation of value and associability in several brain regions. Interestingly, the enhanced sensitivity to prediction errors was partially mediated by the striatal associability computations, suggesting that both increased prediction-error weight and decreased striatal tracking of associability independently contribute to PTSD symptoms. It is possible that the enhanced sensitivity to prediction errors might be the by product of the decreased neural associability tracking.

All in all, these results suggest that exploiting the combined power of computational, morphological, and functional analyses enable us to relate latent markers of learning and morphological indices to overt symptoms, as specific targets for investigating trauma-related psychopathology and its potential treatment.

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Any methods, additional references, Nature Research reporting summaries, source data, statements of data availability and associated accession codes are available at <https://doi.org/10.1038/s41593-018-0315-x>.

Received: 10 July 2018; Accepted: 4 December 2018;
Published online: 21 January 2019

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Acknowledgements

The main source of funding for this work was provided by NIMH 105535 R01 grant awarded to I. Harpz-Rotem and D. Schiller (MPI) and funding provided by the Clinical Neurosciences Division of the National Center for PTSD. Additional support was provided by Klingenstein-Sims Fellowship Award in the Neurosciences to D. Schiller, The Brain and Behavior Research Foundation 23260 to I. Harpz-Rotem, Chinese NSF grant 31421003 to J. Li, and the Swiss National Science Foundation grant SNF 161077 to P. Homan. The analytic work was supported in part through the computational resources and staff expertise provided by Scientific Computing at the Icahn School of Medicine at Mount Sinai.

Author contributions

I.L., I.H.R., and D.S. designed the study. E.F., C.G., I.L., and I.H.R. collected the data. J.H. scored the data. P.H. analyzed the data. I.L., J.L., I.H.R., and D.S. contributed to data analysis. J.H.K., R.H.P., and S.S. contributed to the interpretation of the results. P.H., I.L., I.H.R., and D.S. wrote the first draft of the manuscript. All authors contributed to the final version of the manuscript.

Competing interests

The authors report no competing interests.

Additional information

Supplementary information is available for this paper at <https://doi.org/10.1038/s41593-018-0315-x>.

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Methods

Sample characteristics. General description and excluded participants. A total of 77 participants took part in the experiment. Due to problems with the SCR equipment or measurement problems during the functional scan, we did not obtain complete skin conductance and/or functional imaging data of 23 participants. These participants were similar compared to the included participants (see Supplementary Table 1). This was confirmed by comparing age, CAPS, BDI, STAI-S, ASI, CES and education between excluded and included participants in a linear mixed model with the within subject factor metric (with the aforementioned variables as levels) and the between subject factor sample (levels: included, excluded) as well as a random intercept. Importantly, the effects of sample ($F(1, 75.05) = 0.05, P = 0.832$) and the metric by sample interaction ($F(6, 434.7) = 0.13, P = 0.993$) were both not significant.

Thus, the full analysis was conducted on 54 combat veterans (see Table 1 for complete demographic and psychopathology details). The sample partially overlaps ($N=30$) with the sample in a previous report¹. The main reason to consider the partially overlapping structural data in the current study is that it increased the predictive validity of the right amygdala neural computations effect. In addition, since volume had already been shown to be predictive of CAPS symptoms¹, the current study aimed to explicitly test whether the effect of neural computation goes beyond this effect of volume.

The study was approved by the Yale University Human Investigating Committee and the Human Subjects Subcommittee of the VA Connecticut Healthcare System and compliance with all relevant ethical regulations was ensured throughout the study. All participants gave informed consent and were paid for their participation. Sample size was determined based on the assumption of a medium to large ($\eta^2 = 0.4$) brain-behavior relationship between PTSD symptoms and blood-oxygenation-level-dependent (BOLD) activation. The necessary sample size was thus calculated as $N = 46$ with 80% power and $N = 61$ with 90% power.

Addressing the gender imbalance in the study sample. Since there was a considerable gender imbalance in our study sample (49 of the 54 participants were male), we verified that all of the main results of the current study hold up when restricting the study sample to only male participants. Specifically, the effect of higher prediction-error weight predicting more CAPS symptoms remained significant ($\beta = 0.51, t(46) = 4.03$, two-tailed, $P < 0.001$). In addition, the effect of value computation for the right amygdala changed only minimally ($\beta = -0.27, t(43) = -1.78$, two-tailed, $P = 0.083$) and remained significant for the left amygdala ($\beta = -0.36, t(43) = -2.38$, two-tailed, $P = 0.022$); and the volume effect for the right amygdala remained significant ($\beta = -0.5, t(43) = -2.54$, two-tailed, $P = 0.015$).

In addition, the interaction of region and CAPS remained significant for value computation ($F(2, 94) = 3.12, P = 0.049$), and the interaction of learning component by CAPS remained significant for associability and prediction-error computation ($F(1, 188) = 21.94, P < 0.001$).

Thus, the heterogeneity introduced by gender seems to be negligible in this study, which is why we decided to keep the female participants in the sample to maximize statistical power of the otherwise relatively small study and precision of the estimated effects.

Addressing the clinical heterogeneity of the study sample. We recruited veterans with a wide range of psychopathology, from completely healthy to pronounced PTSD, which can be seen from the distributions of PTSD (CAPS), depression (BDI), and anxiety symptoms (STAI-S; ASI; Supplementary Fig. 1). Nevertheless, the reversal learning index did not differ significantly between combat veterans with and without PTSD ($t(48.46) = 0.17$, two-tailed, $P = 0.868$), between combatants with high versus low PTSD (median split; $t(36.15) = -0.51$, two-tailed, $P = 0.611$), or between combat veterans with CAPS values at the extreme lower (CAPS ≤ 20 ; $N = 19$) or extreme higher end (CAPS ≥ 65 ; $N = 10$; $t(10.73) = 0.48$, two-tailed, $P = 0.638$).

To confirm that our main results were not significantly influenced by the clinical heterogeneity in our sample, we adjusted our models testing for neural computations of value, associability and prediction error for comorbidities and medication status. For value computation, the region by CAPS interaction remained significant when the model was adjusted for the number of comorbidities and medication status ($F(3, 153) = 3.51, P = 0.017$). In addition, the effect was also robust to the adjustment for depression (BDI; $F(3, 153) = 3.73, P = 0.013$), state anxiety (STAI-S; $F(3, 141) = 3.57, P = 0.016$) and anxiety sensitivity (ASI; $F(3, 150) = 3, P = 0.032$).

Similarly, for associability and prediction-error computation, the learning component by CAPS interaction remained significant when the model was held up when using non-parametric rank correlation tests. Specifically, we confirmed this for the correlation between prediction-error weight and CAPS ($\rho = 0.52$, two-tailed, $P < 0.001$), for the correlation between left amygdala neural value tracking and CAPS ($\rho = -0.3$, two-tailed, $P = 0.03$), the correlation between right amygdala volume and CAPS ($\rho = -0.37$, two-tailed, $P = 0.005$), the correlation

between right amygdala neural value tracking and CAPS ($\rho = -0.27$, two-tailed, $P = 0.046$), the correlation between striatum neural value tracking and CAPS ($\rho = -0.35$, two-tailed, $P = 0.009$), the correlation between striatum neural associability tracking and CAPS ($\rho = -0.37$, two-tailed, $P = 0.006$), the correlation between dACC neural associability tracking and CAPS ($\rho = -0.29$, two-tailed, $P = 0.031$), and the correlation between hippocampus neural associability tracking and CAPS ($\rho = -0.29$, two-tailed, $P = 0.033$).

Together, these results suggest that the findings of this study were robust to the clinical heterogeneity of the study sample, and that the correlations we report were robust to outliers.

Study design. The study consisted of a threat reversal learning experiment during fMRI on a single day. Threat learning was measured with SCR; structural magnetic resonance images were acquired in the same MRI session, immediately before the task. Participants were randomly assigned to one of two trial orders (see below). Due to the study design, data collection and analysis were not performed blind to the conditions of the experiments.

Screening procedures. Psychopathology was assessed using the Structural Clinical Interview for DSM-IV, the gold-standard CAPS for PTSD diagnosis. Exclusion criteria were mental retardation, psychosis, bipolar disorder, substance dependency (life time), drug abuse in the past year, alcohol abuse in the past 60 days, neurological disorders, learning disabilities, attention deficit hyperactivity disorder, use of antipsychotic, hypnotic or sedative medications and less than 30 days' stable dose of antidepressants. Participants currently below PTSD clinical cutoff (that is, presence of at least one criterion B symptom, at least three criterion C symptoms, at least two criterion D symptoms, as well as criteria A, E and F met) with a history of PTSD diagnosis were also excluded (remitting PTSD). We additionally measured the combat exposure score (CES), depression with the BDI, anxiety sensitivity with the ASI and state anxiety with the STAI-S. Participants underwent breathalyzer and urine tests before the experiment to further validate substance use beyond the Structured Clinical Interview for DSM-IV.

Experimental task. We used the same task as in a previous study on threat reversal in healthy participants; that is, a threat discrimination and reversal task, with delay conditioning and partial reinforcement of about 33% (Fig. 1a). Participants were told that they would see visual images on a screen while receiving shocks. The level of the shocks was determined by participants before the experiment. Participants inside the MRI were instructed to pay attention to the screen and try to figure out the relationship between the stimuli and the shocks. Importantly, we did not mention the two stages or the reversal of contingencies. The conditioned stimuli were two mildly angry male faces from the Ekman series.

Stimuli and apparatus. The unconditioned stimulus was a mild electric shock to the foot (200 ms duration, 50 pulses s⁻¹). The stimuli were presented for 4 s, with a 12 s intertrial interval in which a fixation point was presented (Fig. 1a). During acquisition, one face (face A) was paired with the unconditioned stimulus on one-third of the trials, while the other (face B) was never paired with the unconditioned stimulus. During reversal, these contingencies switched, and face B was now paired with the unconditioned stimulus on approximately one-third of the trials and face A was not paired with the unconditioned stimulus. The order of the different trial types was pseudorandomized (no consecutive reinforced trials and no more than two consecutive trials of each kind), and the designation of faces into 'face A' and 'face B' was counterbalanced across participants. During acquisition, there were 12 presentations of each of the faces, intermixed with an additional six presentations of face A that co-terminated with the unconditioned stimulus. Reversal immediately followed acquisition, and the transition between the stages was unsignaled. This stage consisted of 16 presentations of each of the faces, intermixed with seven additional presentations of face B that co-terminated with the unconditioned stimulus. We considered the first trial in which face B co-terminated with the unconditioned stimulus as the beginning of the reversal stage (Fig. 1a).

Physiological data acquisition and analysis. Mild shocks were delivered through a stimulating bar electrode attached to the participant's right ankle. A BIOPAC stimulator charged by a stabilized current was used, with cable leads that were magnetically shielded and grounded through a radio frequency filter. The participants were asked to set the level of the shock themselves using a work-up procedure before scanning. In this procedure, a participant was first given a very mild shock (20 V, 200 ms, 50 pulses/s), which was gradually increased to a level the participant indicated as uncomfortable, but not painful (with a maximum level of 70 V). Skin conductance was assessed with shielded Ag-AgCl electrodes, filled with standard NaCl electrolyte gel and attached to the middle phalanges of the second and third fingers of the left hand. The electrode cables were grounded through an radio frequency filter panel. The skin conductance signal was amplified and recorded with a BIOPAC Systems skin conductance module connected to a computer.

Data were continuously recorded at a rate of 200 samples per second. An off-line analysis of the analog skin conductance waveforms was conducted with

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AcqKnowledge software (BIOPAC Systems). The level of SCR was assessed for each trial as the base-to-peak amplitude difference in skin conductance of the largest deflection (in microsiemens; μs) in the 0.5–4.5-s latency window after stimulus onset. The minimal response criterion was 0.02 μs . Responses below this criterion were encoded as zero. The raw skin conductance scores were square-root transformed to normalize the distributions, and scaled according to each participant's average response to the unconditioned stimulus.

Statistical analysis. We averaged the learning effects (face A minus face B) across trials by stage (acquisition, reversal) for each participant and calculated a threat reversal index by subtracting the learning effect of reversal from the learning effect of acquisition. To assess whether participants showed successful threat reversal, we tested whether the reversal index was significantly different from zero with a one-sample t -test. The threshold for this analysis was set at $P < 0.05$, two-tailed. The relationship between latent learning parameters (see below) and PTSD symptomatology was estimated with a linear regression model. Data distribution was assumed to be normal but this was not formally tested. However, individual data points are shown in scatter plots throughout the manuscript.

Computational modeling. Following classic computational learning theory¹, we assumed a deterministic learning model and a probabilistic observation model to describe the generation of our data. The deterministic learning model describes the dynamics of how internal variables gate learning, while the observation model describes how the internal variables are realized in observed data.

Pearce–Hall learning model. Unlike the Rescorla–Wagner model (see below) that treats the learning rate as constant, the Pearce–Hall model for associability-gated learning substitutes associability for the constant learning rate. Thus, such a model incorporates prediction-error-driven value updating into an associability model resulting in the hybrid model:

$$\begin{aligned}\delta_n &= r_n - V_n(x_n) \\ V_{n+1}(x_n) &= V_n(x_n) + \kappa\alpha_n(x_n)\delta_n \\ \alpha_{n+1}(x_n) &= \eta[\delta_n] + (1-\eta)\alpha_n(x_n).\end{aligned}\quad (1)$$

Here, x_n is the conditioned stimulus on trial n (conditioned stimulus: CS+ or CS−) and r_n as the unconditioned stimulus delivered (1 for unconditioned stimulus, 0 for no unconditioned stimulus). The punishment prediction error δ_n measures the difference between the expected and predicted shock on trial n . The associability α for the value update is a variable. The value for the conditioned stimulus not observed on trial n remains unchanged. Since associability of trial n depends on absolute prediction errors from past but not current trials, associability $\alpha_n(x_n)$ and prediction error δ_n are relatively uncorrelated.

To derive the best fits for this model, we assumed that $V_0 = 0.5$, reflecting the assumption that getting a shock or not was equally likely for the first trial. We compared the fit of different versions of the hybrid model to the SCR data by optimizing the free parameters of each model. We assumed the likelihood of each trial's SCR S_n to be an independent and identically distributed Gaussian distribution around a mean determined by value, associability or the combination of both value and associability as predicted by the model on that trial (plus a constant term):

$$\begin{aligned}S_n &\sim \text{Normal}(\beta_0 + \beta_1 V_n(x_n), \sigma) \\ S_n &\sim \text{Normal}(\beta_0 + \beta_1 \alpha_n(x_n), \sigma) \\ S_n &\sim \text{Normal}(\beta_0 + \beta_1 V_n(x_n) + \beta_2 \alpha_n(x_n), \sigma).\end{aligned}\quad (2)$$

As can be seen, these correspond to linear regressions of value or associability, or the combination of both, to the SCR. We tested all three possible combinations (equations 1–3; Hybrid (V); Hybrid (α); Hybrid ($\alpha + V$)), all in separate fits of all free parameters.

Using Hierarchical Bayesian modeling, we first verified that we could recover simulated parameters of initial association (α_0), κ and the associability learning rate, η (Supplementary Fig. 2). We also ruled out that an extended model with an additional scaling parameter that captured a change of the prediction-error weight for the reversal stage would fit the data better (Fig. 2a).

Rescorla–Wagner learning model. Although we found that a hybrid model of associability and value computation outperformed a simpler Rescorla–Wagner model, we were also interested in how a basic Rescorla–Wagner model could explain value computation in the amygdala. The Rescorla–Wagner model is the standard model of error-driven predictive learning. It assumes that the expected value (V) for each trial is updated according to the learning rate and the prediction error:

$$\begin{aligned}V_{n+1}(x_n) &= V_n(x_n) + \alpha\delta_n \\ \delta_n &= r_n - V_n(x_n).\end{aligned}\quad (3)$$

Here, x_n is the conditioned stimulus on trial n (face A or face B) and r_n as the unconditioned stimulus delivered (1 for unconditioned stimulus, 0 for no

unconditioned stimulus). The punishment prediction error δ_n measures the difference between the expected and predicted shock on trial n . The learning rate α for the value update is a constant free parameter. The expected value for the conditioned stimulus absent on trial n remains unchanged. To derive the best fits from the Rescorla–Wagner model, we assumed that $V_0 = 0.5$, reflecting the assumption that getting a shock or not was equally likely for the first trial. We considered that model-based value computation would be correlated with amygdala activity, and that this correlation would be more negative for individuals with higher levels of PTSD symptoms. After verifying that simulated parameters could be recovered with the hierarchical Bayesian approach used in this study (Supplementary Fig. 5) and that the model indeed fitted the recorded SCRs (Supplementary Fig. 6), we calculated linear regression models including functional (value encoding) and structural indices for amygdala as predictors of the PTSD symptoms. To account for unspecific intersubject variability, these models were adjusted for learning rate, age, gender, head movement and total intracranial volume. For the right amygdala (Supplementary Fig. 7a), we found that volume predicted CAPS symptoms ($\beta = 0.49$, $t(46) = -2.59$, two-tailed, $P = 0.013$ and Supplementary Fig. 7b) while neural activity as a predictor did not reach statistical significance ($\beta = -0.28$, $t(46) = -2$, two-tailed, $P = 0.052$ and Supplementary Fig. 7c). In the left amygdala, the effect of value-dependent activity remained significant when including amygdala volume in the same model ($\beta = -0.35$, $t(46) = -2.42$, two-tailed, $P = 0.02$ and Supplementary Fig. 7d), but no independent effect for volume emerged ($\beta = -0.2$, $t(46) = -1.04$, two-tailed, $P = 0.303$).

We also confirmed that the results were robust to the specific ROI definition of the amygdala and that individual differences in right amygdala volumes did not affect the effect of neural activity on CAPS symptoms. We repeated our analysis of right amygdala value computation using the winning hybrid ($\alpha + V$) model. We used the individual amygdala segmentations as computed by FreeSurfer as masks for the ROI analysis of the right amygdala in SPM. After running the recon-all pipeline in FreeSurfer, we converted and binarized the subcortical segmentation of each individual to NIfTI format. We then applied the individual normalization parameters calculated by SPM during the SPM preprocessing pipeline to warp the FreeSurfer segmentation to the Montreal Neurological Institute space. A figure (Supplementary Fig. 8) shows two illustrative participants with individual amygdala masks (estimated in FreeSurfer and indicated in red) projected on to their T1-weighted brain anatomy in SPM.

Given that we found an effect of volume for the right amygdala, we thus extracted the mean beta estimates of these individual right amygdala masks and entered the estimates in a multivariable regression model, using CAPS as the dependent measure and the beta estimates together with amygdala volume as predictors, adjusting for age, gender and intracranial volume. We found a similar effect for the neural activity compared to the original findings in the right amygdala ($\beta = -0.3$, $t(48) = -2.07$, two-tailed, $P = 0.044$), suggesting that the BOLD effects were correctly estimated.

To further characterize the relationship between structure and function we added the interaction term to the model and found that there was no evidence for a synergistic effect between these independent variables (right amygdala: $\beta = -0.7$, $t(45) = -0.71$, two-tailed, $P = 0.482$; left amygdala: $\beta = -0.02$, $t(45) = -0.02$, two-tailed, $P = 0.988$). Moreover, the correlation between structure and function was not significant (right amygdala: $\beta(52) = 0.05$, two-tailed, $P = 0.742$; left amygdala: $\beta(52) = 0.04$, two-tailed, $P = 0.797$). Further, a mediation analysis with amygdala volume as a mediator of the association between value activity and CAPS symptoms did not show evidence for full or partial mediation; instead, inclusion of amygdala volume did in fact improve the predictive validity of neural activity. A possible explanation is a compensatory recruitment of amygdala neurons in veterans with smaller amygdala volumes, probably due to a stress-related gray matter reduction¹³.

We also tested a potential difference in learning rates between acquisition and reversal and additionally tested an extended version of the Rescorla–Wagner model. We added an additional scaling parameter ρ , which captures the change in the learning rate during the reversal stage. For acquisition, we thus used the classical Rescorla–Wagner model:

$$\begin{aligned}V_{n+1}(x_n) &= V_n(x_n) + \alpha\delta_n \\ \delta_n &= r_n - V_n(x_n),\end{aligned}\quad (4)$$

and the extended model for reversal:

$$\begin{aligned}V_{n+1}(x_n) &= V_n(x_n) + \rho\alpha\delta_n \\ \delta_n &= r_n - V_n(x_n),\end{aligned}\quad (5)$$

where ρ is the scaling parameter. We performed a model comparison between the two models, computing the DIC that captures the goodness of fit of a Bayesian hierarchical model with lower values meaning better fits¹⁴. Notably, we found that the simpler model provides a better fit to the data (extended model: DIC = 6223.11; simpler model: DIC = 6003.91).

Finally, we investigated whether fitting V_0 , the initial value, as an additional free parameter would improve the model fit and found that the resulting DIC was higher than the one from the original simpler model; we thus kept the simpler model with V_0 fixed at 0.5.

Hierarchical Bayesian model fitting. *Bayesian modeling versus MLE.* We used Hierarchical Bayesian analysis (HBA) to obtain estimates of the free parameters in our computational models. The advantage of HBA compared to MLE is that individual differences are accounted for but information across individuals is pooled so that individual estimates are pulled toward the population mean (an effect sometimes referred to as shrinkage¹).

In MLE, on the other hand, point estimates are obtained that maximize the likelihood of the data for each individual separately¹. Individual ML estimates, however, may suffer from noise and may be unreliable when faced with an insufficient amount of data¹. In addition, there is no guarantee that non-linear optimization algorithms commonly used (such as fmincon in MATLAB) will provide a set of parameter values that uniquely maximize the log-likelihood; indeed, premature stops of the algorithm are common, which provide a local (and thus sub-optimal) instead of a global maximum of the likelihood function¹. Group-level analysis of MLE, which treats a group as a single subject and estimates a single set of parameters for a whole group of individuals, is commonly used to generate fMRI regressors for model-based analyses as it is supposed to generate more reliable estimates needed for fMRI¹, but it inevitably ignores individual differences and does not allow for group comparisons.

Hybrid model comparison using MLE. Nevertheless, to replicate the model comparison reported in Li and colleagues more directly¹, we also performed a model comparison of the hybrid models using MLE as in the aforementioned study, and ranked all three hybrid models as well as the Rescorla–Wagner model according to their Bayesian Information Criterion (BIC). Consistent with the results in Li and colleagues¹, the model with the lowest BIC was the hybrid ($\alpha + V$) model, outperforming the other hybrid models as well as the Rescorla–Wagner model (Supplementary Fig. 4a). More specifically, direct comparisons using likelihood ratio tests revealed that the Hybrid (V) model outperformed the Rescorla–Wagner analysis model ($\chi^2 = 408.11$, d.f. = 108, $P < 0.001$), and the Hybrid ($\alpha + V$) outperformed the Rescorla–Wagner model ($\chi^2 = 877.61$, d.f. = 162, $P < 0.001$), the Hybrid (V) model ($\chi^2 = 469.5$, d.f. = 54, $P < 0.001$) and the Hybrid (α) model ($\chi^2 = 348.54$, d.f. = 54, $P < 0.001$).

We did not find evidence that the individual MLE model fits interacted with the PTSD symptomatology; the correlation between model parameters and symptoms was essentially flat for each of the four models (Supplementary Fig. 4b).

Details on the Bayesian modeling procedure. To perform HBA, we used the probabilistic programming language Stan v2.15.1 (Stan Development Team, 2014), which makes use of Markov chain Monte Carlo (MCMC) sampling algorithms termed Hamiltonian Monte Carlo. Hamiltonian Monte Carlo provides an efficient sampling algorithm even for multilevel models and highly correlated parameters³.

For the Rescorla–Wagner model, individual parameters were assumed to be drawn from group-level normal distributions. Normal and half-Cauchy distributions were used for the priors of the group-level means and standard deviations, respectively^{39,40}. We used weakly informative priors³ to minimize the influence of those priors on the posterior distributions with our relatively small sample size. As the learning rate α is bounded between 0 and 1, we used the inverse probit transformation (the cumulative distribution function of a unit normal distribution) to convert unconstrained values into this range. The mathematical relationship between the probability density function and the cumulative density function of the unit normal distribution guarantees that this transformation that the converted prior will be uniformly distributed between 0 and 1. Stan provides a fast approximation of the inverse probit transformation (the `Phi_approx` function) to achieve this. The learning rate was thus declared as follows:

$$\begin{aligned}\mu_{\alpha'} &\sim \text{Normal}(0, 1) \\ \sigma_{\alpha'} &\sim \text{half-Cauchy}(0, 5) \\ \alpha' &\sim \text{Normal}(\mu_{\alpha'}, \sigma_{\alpha'}) \\ \alpha &= \text{Probit}^{-1}(\alpha')\end{aligned}$$

where $\mu_{\alpha'}$ and $\sigma_{\alpha'}$ are hyper-parameters that dictate the distribution of α' and sequentially α .

A total of 2,000 samples were drawn after 1,000 burn-in samples for each of four chains (resulting in a total of 8,000 samples). To assess the convergence of the chains for each parameter, we used the Gelman–Rubin test⁴¹ that calculates an \bar{R} statistic, with \bar{R} values close to 1.00 indicating that the MCMC chains have converged to the target distributions.

Notably, the \bar{R} values obtained for all model parameters were 1.00, and visual inspection of MCMC chains confirmed the mixing of MCMC samples. In addition, effective sample sizes (ESS) of model parameters, which are associated with autocorrelation and mixing of MCMC chains (with a smaller ESS indicating higher autocorrelation), were typically greater than 1,000 (out of 8,000 total samples). The minimum ESS of hyper-parameters was 592. Visual inspection of the parameters with smaller ESS confirmed their convergence to the target distributions.

For the hybrid models, priors of individual parameters were again assumed to be drawn from group-level normal distributions, but normal and half-normal

distributions were used for the priors of the group-level means and standard deviations, respectively.

Parameter recovery tests. To further verify the plausibility of our model, we used simulated data to test whether simulated parameters could be recovered (recovery tests). We generated true parameter values, simulated synthetic behavioral data based on the parameters and recovered their parameter values using the HBA described in the previous section. Results confirmed that the model was successful at recovering the simulated parameters (Supplementary Figs. 2 and 5).

Statistical analysis. For the Rescorla–Wagner model, we used the individual posterior means of the learning rate to calculate the trial-wise expected value for each participant. These values were used as parametric modulators in the model-based fMRI analysis. For the hybrid model, trial-wise expected value, associability and prediction error were used as parametric modulators in the model-based fMRI analysis (see below).

Structural magnetic resonance imaging and analysis. A Siemens Trio TIM 3T and 12-channel receiver array head coil were used for data acquisition. High-resolution T1-weighted anatomical images ($1 \times 1 \times 1 \text{ mm}^3$) were acquired with an MPAGE pulse sequence (voxel size $1 \times 1 \times 1 \text{ mm}^3$; repetition time = 2.5 s; echo time = 2.77 ms; flip angle = 7°; 256×256 matrix, 176 sagittal slices of 1 mm).

Blinded to the clinical status, image processing and segmentation were conducted using the automated FreeSurfer recon-all pipeline (<http://surfer.nmr.mgh.harvard.edu>). FreeSurfer transforms brains from native space to standard space to perform subcortical segmentation, and then transforms them back to native space to extract individual amygdala volumes. We thus used those extracted measures of amygdala volume for each participant and restricted the analysis to this a priori ROI. Amygdala volume measures were then used as predictors in multivariable linear regressions (see below).

Notably, excessive head motion was found to be associated with reduced estimates of gray matter thickness and volume compared to age- and gender-matched samples and consequently with inflated effect sizes⁴². Following a recent suggestion that participants' head movement during functional imaging sequence may provide a proxy for head movement during the structural sequence (where no head movement is recorded), we calculated the total head movement in mm during fMRI for each participant and included this scalar as a covariate in the statistical analysis⁴². Importantly, this covariate was used as a proxy measure for head movement during the anatomical session. The movement during the fMRI session was regressed out in the fMRI design matrix. Note that the exclusion of this additional regressor in the multivariable regression did not alter the main results of our analysis.

fMRI and analysis. Functional images were acquired using a single-shot gradient echo planar imaging sequence (repetition time = 2,000 ms; echo time = 25 ms; field of view = 192 cm; flip angle = 75°; bandwidth = 4,340 Hz per pixel; echo spacing = 0.29 ms). Forty contiguous oblique axial slices ($3 \times 3 \times 3 \text{ mm}^3$ voxels) parallel to the anterior–commissure–posterior–commissure line were obtained.

Analyses of the imaging data were conducted using SPM 12 (<http://filion.ac.uk/spm12>). After discarding the first eight volumes, native-space images were realigned, slice-time corrected and co-registered to each subject's structural scan. Structural image preprocessing included segmentation, bias correction and spatial normalization; these normalization parameters were also used to normalize the functional images. Finally, functional images were smoothed with a Gaussian kernel (4 mm full-width at half-maximum).

Model-based fMRI analysis. We conducted a computational analysis using the hybrid Pearce–Hall learning model, with the fMRI regressors derived from the fits to the SCR data. Cue onset and offset were modeled as two discrete events, and each expected value (V) regressor was included as a parametric modulator of the stimulus onset event. In addition, the occurrence of a shock (0 for trials with no shock, 1 for trials with a shock) and prediction error were modeled as parametric modulators of cue offset.

Six regressors modeling affine head-motion parameters were also included in the GLM. All events were convolved with a canonical gamma-variate hemodynamic response function. The contrast of interest was the correlation of expected value V , corresponding to the expectation of a shock on each trial, with the BOLD response in the brain. We thus computed images of this contrast for each participant and used the contrast images as input for the ROI-based analyses. Our ROI-based analysis focused on the amygdala. We defined the amygdala ROIs functionally, using an independent contrast of conditioned stimuli (both face A and face B) versus baseline and a relatively loose contrast of $P < 0.001$. For each ROI, we extracted the mean beta estimates obtained from the GLM for the correlation of expected value V with the BOLD response. The beta estimates were then entered as predictors in multivariable linear regressions.

To assess the independent contributions of structural and functional indices on PTSD symptoms, we then calculated multivariable linear regressions including both the structural and functional indices as predictors, and the symptoms as measured with the CAPS as outcome measures. For each ROI, the structural index was the volume, the functional indices were the extracted mean beta estimates

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obtained from the computational GLM for the correlation of expected value V with the BOLD response. These models were adjusted for learning rate, age, gender, head movement and total intracranial volume to adjust for unspecific intersubject variability.

In addition to the amygdala, we extended our analysis to the dACC, the striatum and hippocampus. The dACC was defined with an independent functional contrast (face A versus face B in acquisition, corresponding to conditioned stimulus: CS+ versus CS-); since Li and colleagues¹ found the strongest activation in the caudate nucleus, we used a contrast of shock occurrence versus baseline in a sample of healthy control participants who underwent the same task to ensure reliable activation^{10,11}. Like the functional contrast for the dACC, this contrast for the striatum was also independent of the computational contrasts. The hippocampal ROI was defined anatomically using the WFU Pickatlas¹². We then used two separate GLMs to examine neural activity related to value encoding (GLM 1) as well as associability and prediction error (GLM 2). Replicating the design of a previous study in reversal learning and value encoding for GLM 1, we included value encoding as parametric modulator of stimulus onset and included shock occurrence and prediction error as additional regressors of stimulus offset. For GLM 2, we followed the design of the previous study by Li and colleagues¹ and included associability, shock occurrence and prediction error as parametric modulators of stimulus offset.

We then extracted the beta estimates of the ROIs and computed separate linear mixed models for value computation as well as associability and prediction error, respectively. Neural activity was used as the dependent variable in these models, and predictors for region and CAPS (model 1, predicting value computation) as well as learning component (model 2; predicting associability and prediction error) were entered as predictors. In addition, we included a random intercept and a random slope for region to account for the within-subject correlations.

We also performed a computational analysis using the simpler Rescorla–Wagner learning model, with the fMRI regressors derived from the fit to the SCR data. Cue onset and offset were modeled as two discrete events, and each expected value (V) regressor was included as a parametric modulator of the stimulus onset event. In addition, the occurrence of a shock (0 for trials with no shock, 1 for trials with a shock) and prediction error were modeled as parametric modulators of cue offset, but are not considered in this study due to algebraic collinearity with the V regressor. This means that the parametric regressor of interest was expected value, which modulated cue onset, while the regressors for shock outcome and prediction error were included in the design matrix (modulating cue offset) but are not considered in this study.

While this setup is in line with previous studies using the same reversal paradigm together with computational modeling^{1,2}, we manually confirmed that the amount of collinearity in the fMRI design matrix was acceptable. We calculated the variance inflation factor (VIF) for the value regressor. The VIF reflects how much the variance of the estimated regression coefficient is increased by the correlation among the model regressors. Its square root quantifies how larger the standard error is compared with what it would be if the regressor were uncorrelated with the model regressors. While it is common practice to consider a VIF > 10 as problematic, it is important to note that even in the presence of collinearity the regression coefficients will be unbiased. As the term VIF suggests, what is affected is the variance of the estimates, resulting in increased noise and reduced statistical power. However, we verified that the VIF of the value regressor was below five for each participant, with a mean VIF across participants of 1.5

(s.d. 0.13) suggesting that collinearity was not an issue for this design. Supporting this conclusion, the effects for amygdala value computation on CAPS symptoms remained significant when including the amygdala activation during shocks in the same model (left: $\beta = -0.34$, $t(46) = -2.32$, two-tailed, $P = 0.025$; right: $\beta = -0.31$, $t(46) = -2.14$, two-tailed, $P = 0.038$).

Reporting Summary: Further information on research design is available in the Nature Research Reporting Summary linked to this article.

Code availability

The code used for the analyses is available online at: <http://osf.io/rxsw2/>.

Data availability

Data used to support the conclusions of this study is available online at: <http://osf.io/rxsw2/>.

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Data collection

AcqKnowledge software (version 4, BIOPAC Systems)

Data analysis

Stan 2.15.1, R version 3.3.2, Matlab Version 8.6.0.267246 (R2015b), SPM 12

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The data that support the findings of this study as well as the code used for the analyses are available from the corresponding authors upon reasonable request.

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Life sciences study design

All studies must disclose on these points even when the disclosure is negative.

Sample size	Sample size was determined based on the assumption of a medium to large ($r = 0.4$) brain-behavior relationship between PTSD symptoms and BOLD activation. The necessary sample size was thus calculated as N=46 with 80% power and N=61 with 90% power.
Data exclusions	A total of 77 participants took part in the experiment. Due to problems with the SCR equipment or measurement problems during the functional scan, we did not obtain complete skin conductance and/or functional imaging data of 23 participants.
Replication	We did not replicate our findings in an independent sample. To ensure reproducibility of our findings we structured the analyses with a 'Makefile' which documents the dependencies between different analysis modules and allows to run the analyses using the 'make' command at the Unix command line. All the code used is organized in a git repository and is available from the authors upon request.
Randomization	In this study, combat veterans with varying degrees of psychopathology were recruited. Thus, due to the study design, group-membership (combats with PTSD versus combats without PTSD) was not randomized but predetermined. We did, however, randomize participants to two experimental orders in the reversal learning experiment.
Blinding	Since participants did not receive a treatment but were tested in an experiment, blinding was not applicable in this study.

Reporting for specific materials, systems and methods

Materials & experimental systems

n/a	Involved in the study
<input checked="" type="checkbox"/>	<input type="checkbox"/> Unique biological materials
<input checked="" type="checkbox"/>	<input type="checkbox"/> Antibodies
<input checked="" type="checkbox"/>	<input type="checkbox"/> Eukaryotic cell lines
<input checked="" type="checkbox"/>	<input type="checkbox"/> Palaeontology
<input checked="" type="checkbox"/>	<input type="checkbox"/> Animals and other organisms
<input type="checkbox"/>	<input checked="" type="checkbox"/> Human research participants

Methods

n/a	Involved in the study
<input checked="" type="checkbox"/>	<input type="checkbox"/> ChIP-seq
<input checked="" type="checkbox"/>	<input type="checkbox"/> Flow cytometry
<input type="checkbox"/>	<input checked="" type="checkbox"/> MRI-based neuroimaging

Human research participants

Policy information about [studies involving human research participants](#)

Population characteristics 54 participants, mean age: 32.81 (7.97), 49 males, 5 females; mean CAPS score 39.22 (32.01).

Recruitment Participants were recruited from the community by posting research flyers at several VA hospital in the region, VA community-based clinics, Veterans Centers, universities bulletin boards, and at universities veterans' organization offices.

Magnetic resonance imaging

Experimental design

Design type	Task; event-related
Design specifications	69 trials, 4 s each, intertrial interval 12s.
Behavioral performance measures	The behavioral performance was measured with skin conductance response. Skin conductance was assessed with shielded Ag-AgCl electrodes, filled with standard NaCl electrolyte gel, and attached to the middle phalanges of the second and third fingers of the left hand. The electrode cables were grounded through an RF filter panel. The skin conductance signal was amplified and recorded with a BIOPAC Systems skin conductance module connected to a

computer. Data were continuously recorded at a rate of 200 samples per second. An off-line analysis of the analog skin conductance waveforms was conducted with AcqKnowledge software (BIOPAC Systems). The level of skin conductance response was assessed for each trial as the base-to-peak amplitude difference in skin conductance of the largest deflection (in microsiemens) in the 0.5–4.5 s latency window after stimulus onset. The minimal response criterion was 0.02 microsiemens. Responses below this criterion were encoded as zero. The raw skin conductance scores were square root transformed to normalize the distributions, and scaled according to each participant's average response to the US. We averaged the learning effects (face A minus face B) across trials by stage (acquisition, reversal) for each participant and calculated a threat reversal index by subtracting the learning effect of reversal from the learning effect of acquisition. To assess whether participants showed successful threat reversal, we tested whether the reversal index was significantly different from zero with a one sample t-test.

Acquisition

Imaging type(s)	Structural and functional.
Field strength	3 Tesla.
Sequence & imaging parameters	Structural: MPRAGE pulse sequence (voxel size 1x1x1 mm; repetition time =2.5 s; echo time =2.77 ms; flip 363 angle =7°; 256×256 matrix, 176 sagittal slices of 1 mm). Functional: single-shot gradient echo EPI sequence (TR =2000 ms; 380 TE =25 ms; FOV =192 cm; flip angle =75°; bandwidth =4340 Hz/px; echo spacing =0.29 ms). Forty contiguous oblique-axial slices (3×3×3 mm voxels) parallel to the AC-PC line.
Area of acquisition	Whole brain scan.
Diffusion MRI	<input type="checkbox"/> Used <input checked="" type="checkbox"/> Not used

Preprocessing

Preprocessing software	Analysis of the imaging data were conducted using SPM 12 (http://fil.ion.ac.uk/spm12). After discarding the first eight volumes, native-space images were realigned, slice-time corrected, and coregistered to each subject's structural scan. Structural image preprocessing included segmentation, bias correction, and spatial normalization; these normalization parameters were also used to normalize the functional images. Finally, functional images were smoothed with a Gaussian kernel (4 mm FWHM).
Normalization	Structural image preprocessing included spatial normalization, these normalization parameters were also used to normalize the functional images.
Normalization template	MNI.
Noise and artifact removal	Motion parameters.
Volume censoring	No volumes were censored.

Statistical modeling & inference

Model type and settings	We performed a computational analysis using the Rescorla-Wagner learning model, with the fMRI regressors derived from the fits to the skin conductance data. Cue onset and offset were modeled as two discrete events, and each computational regressor was included as a parametric modulator of the stimulus onset or offset event. In addition, the occurrence of a shock (0 for trials with no shock, 1 for trials with a shock) was modeled as parametric modulators of cue offset. Six regressors modeling affine head-motion parameters were also included in the GLM. All events were convolved with a canonical gamma-variate hemodynamic response function (HRF). The contrast of interest was the correlation of expected value V, corresponding to the expectation of a shock on each trial, with the BOLD response in the brain. We thus computed images of this contrast for each participant and used the contrast images as input for ROI-based analysis.
Effect(s) tested	For each ROI, we extracted the mean beta estimates obtained from the GLM for the correlation of expected value V with the BOLD response for each participant. These beta estimates were then entered as predictors in multivariable linear regressions, with the psychopathology symptoms as outcome measure.
Specify type of analysis:	<input type="checkbox"/> Whole brain <input checked="" type="checkbox"/> ROI-based <input type="checkbox"/> Both
Anatomical location(s)	Amygdala, dACC, and striatum were defined functionally; the hippocampus was defined using the WFU Pickatlas.
Statistic type for inference (See Eklund et al. 2016)	Mean beta estimates extracted from the ROI.
Correction	ROI-based analysis.

Models & analysis

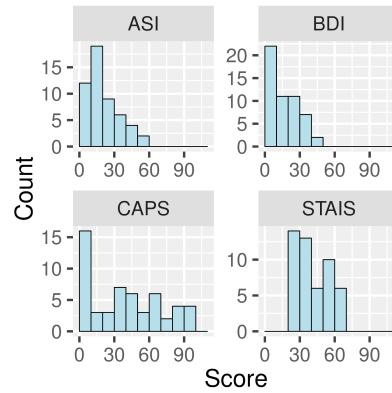
n/a	Involved in the study
<input checked="" type="checkbox"/>	□ Functional and/or effective connectivity
<input checked="" type="checkbox"/>	□ Graph analysis
<input checked="" type="checkbox"/>	□ Multivariate modeling or predictive analysis

In the format provided by the authors and unedited.

Neural computations of threat in the aftermath of combat trauma

Philipp Homan¹, Ifat Levy^{1,2}, Eric Feltham^{3,4}, Charles Gordon^{3,4}, Jingchu Hu¹, Jian Li^{1,5}, Robert H. Pietrzak^{3,4}, Steven Southwick^{3,4}, John H. Krystal^{3,4}, Ilan Harpaz-Rotem^{1,6,7*} and Daniela Schiller^{1,6,7*}

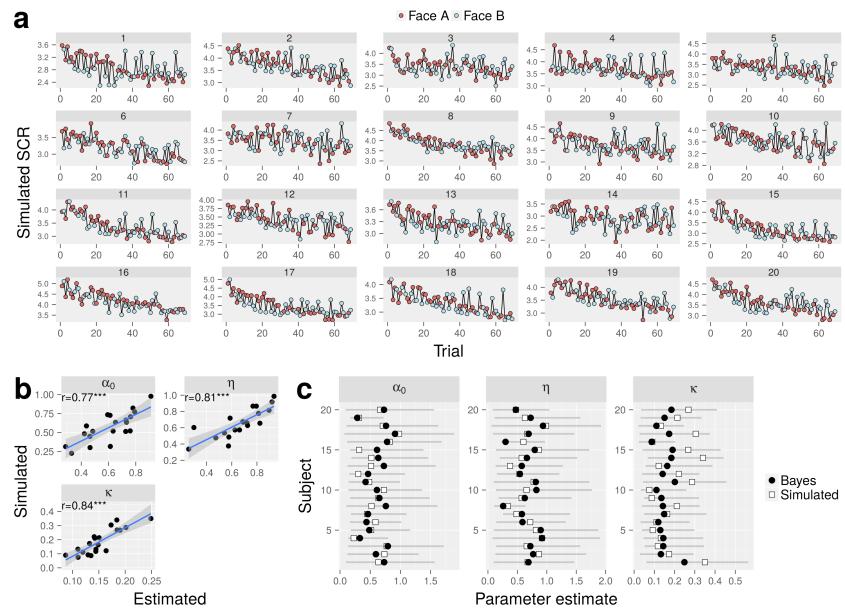
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Supplementary Figure 1

Distributions of psychopathology in the study sample show the wide range of symptoms in participants, from completely healthy to highly affected by PTSD.

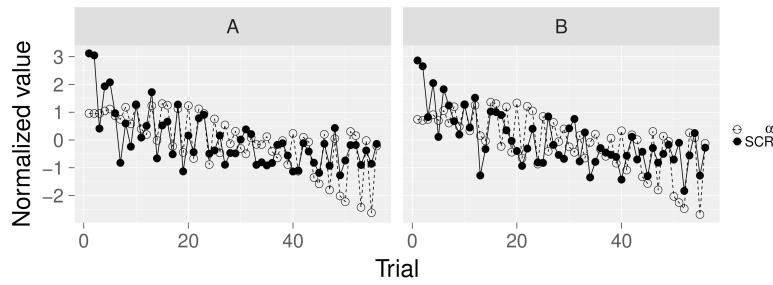
ASI, Anxiety Sensitivity Index; BDI, Beck Depression Inventory; CAPS, Clinician-Administered PTSD Scale; CES, combat exposure score; STAIS, State Anxiety subscale of the Spielberger State-Trait Anxiety Inventory.



Supplementary Figure 2

Hierarchical Bayesian model recovers simulated learning parameters.

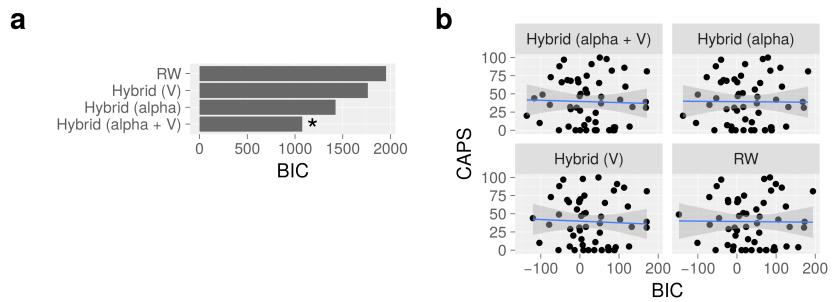
To validate our Hierarchical Bayesian hybrid model before testing it in the clinical dataset, we simulated skin conductance data in 20 participants. We estimated the initial associability α_0 , K , the normalization factor, and η , the prediction error weight. **a. Simulated data in 20 subjects.** **b. Correlations between estimated and simulated values.** The Bayesian model successfully recovered the simulated values. Pearson correlation coefficients with two-tailed significance tests are shown. Error shadings correspond to standard errors. **c. Estimated parameters with 90% credible intervals and simulated parameters.** All simulated parameters were within the 90% credible intervals of the estimated parameters, indicating successful parameter recovery. $^{***}, P < 0.001$.



Supplementary Figure 3

Average skin conductance response across subjects and the best-fit associability trace for all trials without a shock.

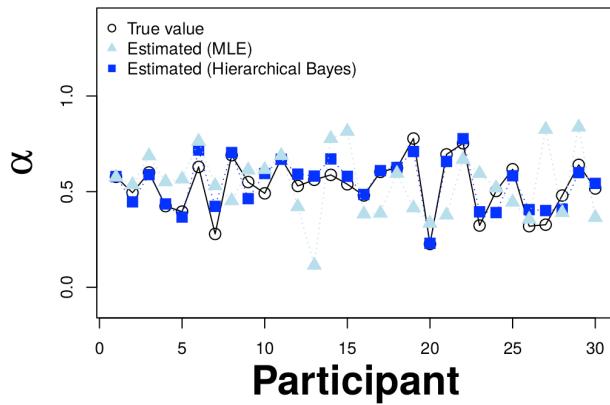
As there were two experimental orders, time courses are displayed for each order separately ($N = 54$ participants). All traces were z-transformed and mean-centered for displaying purpose.



Supplementary Figure 4

Model comparison using maximum likelihood estimation (MLE) and the relationship of model fits to PTSD symptoms.

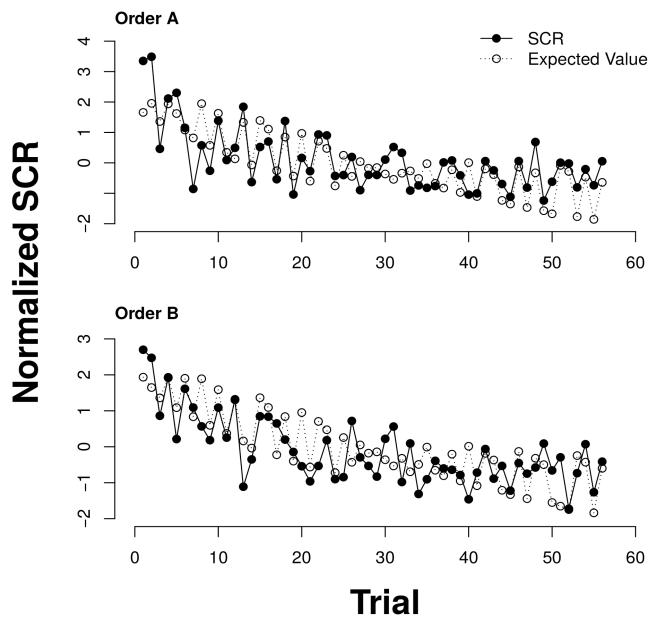
a. MLE analysis is consistent with the Bayesian analysis and a previous study. In line with a previous study,⁸ the hybrid model with the lowest Bayesian Information Criterion (BIC) was the hybrid ($\alpha + V$) model, outperforming the other hybrid models as well as the Rescorla-Wagner (RW) model. The asterisk indicates the winning model. **b. No evidence that model fits interacted with the symptom levels of PTSD.** The correlation between model fits and symptoms was essentially flat for each of the 4 models. Error shadings correspond to standard errors. PTSD, posttraumatic stress disorder. CAPS, Clinician-Administered PTSD Scale; RW, Rescorla-Wagner model.



Supplementary Figure 5

Hierarchical Bayesian model recovers simulated learning parameters of a Rescorla–Wagner model.

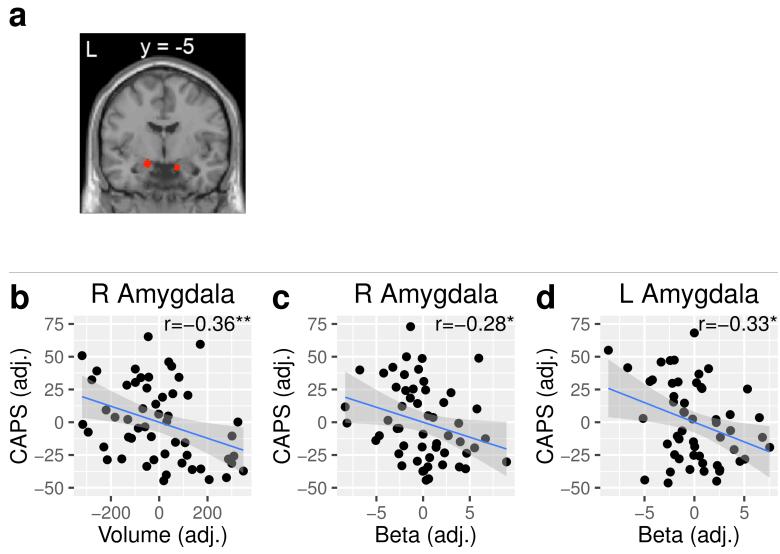
To validate our Hierarchical Bayesian model before testing it in the clinical dataset, we simulated skin conductance data in 30 participants. We estimated the learning rate α (the free parameter in the Rescorla–Wagner model) using our Hierarchical Bayesian model coded in Stan and also compared it to a Maximum Likelihood Estimation (MLE) using the non-linear optimizer fmincon in MATLAB. The Bayesian model successfully recovered the simulated values.



Supplementary Figure 6

Average skin conductance response across subjects and the best-fit expected value trace for all trials without a shock using a Rescorla-Wagner model.

As there were two experimental orders, time courses are displayed for each order separately ($N = 54$ participants). All traces were z-transformed and mean-centered for displaying purpose.

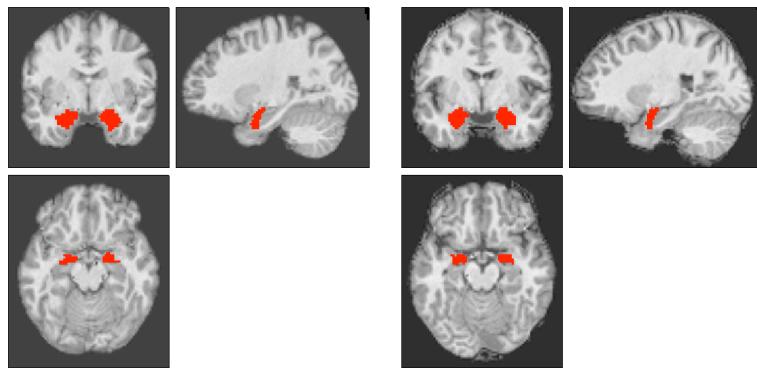


Supplementary Figure 7

Amygdala structure and value computation contribute to PTSD symptoms using a Rescorla-Wagner model.

a. Regions of interest used in the computational imaging analysis. The amygdala (red) was defined functionally, using the contrast of conditioned stimulus vs. baseline. **b-d. Amygdala volume and value-dependent neural activity independently contribute to PTSD symptoms.** Partial correlations are shown ($N = 54$ participants) with Pearson correlation coefficients and two-tailed significance tests. Right amygdala volume and activity as well as left amygdala activity correlated negatively with the PTSD symptoms as measured with the CAPS. Regressions were adjusted for learning rate, age, gender, head movement, and total intracranial volume.

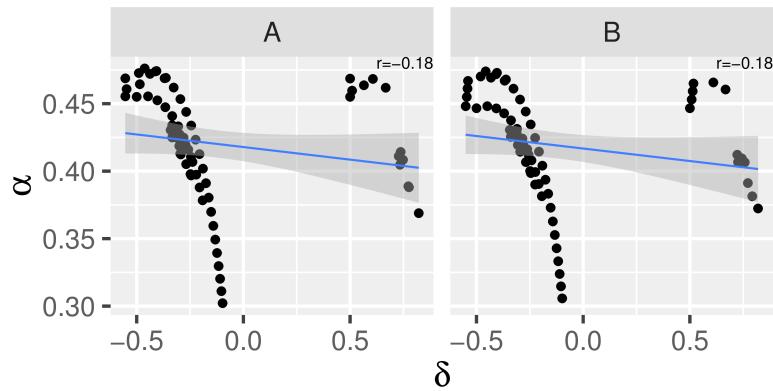
Error shadings correspond to standard errors. CAPS, Clinician-Administered PTSD Scale; adj., adjusted; **, $P < 0.01$; *, $P < 0.05$.



Supplementary Figure 8

Individual amygdala regions of interest for two illustrative participants.

To ensure accurate appearance in the published version, please use the Symbol font for all symbols and Greek letters. Brain slices are shown at $x = 24$, $y = -4$, $z = -16$ in the Montreal Neurological Institute (MNI) coordinate system, using the neurological convention (i.e., left is left).



Supplementary Figure 9

Regression of prediction error (δ) and associability (α) shows that the two learning components were not significantly correlated.

Data for both experimental orders (A and B) is shown ($N = 69$ trials) together with Pearson correlation coefficients and two-tailed significance tests. Error shadings correspond to standard errors.

Supplementary Tables and Figures

Characteristic	N	Mean	SD
Males	23		
Females	0		
VCC	11		
VPTSD	12		
Age	23	33.61	8.02
Education	23	3.57	1.12
ASI	21	19.67	11.74
BDI	23	15.83	11.51
CAPS	23	36.35	31.31
CES	23	16.96	5.92
STAIS	22	40.73	11.97

Table S1: Characteristics of excluded participants due to missing skin conductance and/or functional imaging data. Abbreviations: ASI, Anxiety Sensitivity Index; BDI, Beck Depression Inventory; CAPS, Clinician-Administered PTSD Scale; CES, combat exposure score; STAIS, subscale of the Spielberger State-Trait Anxiety Inventory; SD, Standard deviation; VCC, veteran combat controls; VPTSD, veterans with posttraumatic stress disorder.

5.4 Winkelbeiner et al. 2019, JAMA Psychiatry

Evaluation of Differences in Individual Treatment Response in Schizophrenia Spectrum Disorders A Meta-analysis

Stephanie Winkelbeiner, PhD; Stefan Leucht, MD; John M. Kane, MD; Philipp Homan, MD, PhD

+ Supplemental content

IMPORTANCE An assumption among clinicians and researchers is that patients with schizophrenia vary considerably in their response to antipsychotic drugs in randomized clinical trials (RCTs).

OBJECTIVE To evaluate the overall variation in individual treatment response from random variation by comparing the variability between treatment and control groups.

DATA SOURCES Cochrane Schizophrenia, MEDLINE/PubMed, Embase, PsycINFO, Cochrane CENTRAL, BIOSIS Previews, ClinicalTrials.gov, and World Health Organization International Clinical Trials Registry Platform from January 1, 1955, to December 31, 2016.

STUDY SELECTION Double-blind, placebo-controlled, RCTs of adults with a diagnosis of schizophrenia spectrum disorders and prescription for licensed antipsychotic drugs.

DATA EXTRACTION AND SYNTHESIS Means and SDs of the Positive and Negative Syndrome Scale pretreatment and posttreatment outcome difference scores were extracted. Data quality and validity were ensured by following the PRISMA guidelines.

MAIN OUTCOMES AND MEASURES The outcome measure was the overall variability ratio of treatment to control in a meta-analysis across RCTs. Individual variability ratios were weighted by the inverse-variance method and entered into a random-effects model. A personal element of response was hypothesized to be reflected by a substantial overall increase in variability in the treatment group compared with the control group.

RESULTS An RCT was simulated, comprising 30 patients with schizophrenia randomized to either the treatment or the control group. The different components of variation in RCTs were illustrated with simulated data. In addition, we assessed the variability ratio in 52 RCTs involving 15 360 patients with a schizophrenia or schizoaffective diagnosis. The variability was slightly lower in the treatment compared with the control group (variability ratio = 0.97; 95% CI, 0.95–0.99; $P = .01$).

CONCLUSIONS AND RELEVANCE In this study, no evidence was found in RCTs that antipsychotic drugs increased the outcome variance, suggesting no personal element of response to treatment but instead indicating that the variance was slightly lower in the treatment group than in the control group; although the study cannot rule out that subsets of patients respond differently to treatment, it suggests that the average treatment effect is a reasonable assumption for the individual patient.

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Personalized medicine is based on a widely held assumption that patients differ substantially in their response to treatments. The goal of personalized medicine is to find the right treatment for the right patient. Psychiatry is no exception. An assumption among clinicians and researchers alike is that the response to antipsychotic drugs by patients with psychosis differs considerably between individuals.¹

We report that this assumption may be unfounded. Although variation in the observed treatment responses obviously exists, it is crucial to distinguish between observed and true treatment response: observed response consists of true response plus regression to the mean, some placebo effects, and random terms such as (but not restricted to) measurement error. First, we exemplify why confusing observed with true treatment response is so common, and we use simulated data to show how variation that is purely random and unrelated to permanent differences in treatment response may suggest the need for personalized treatment. Next, we review the evidence of the differences in treatment response by conducting a meta-analysis of the variation in antipsychotic treatment trials. Although this issue is brought up by statisticians regularly,² it deserves more attention from a general psychiatric audience.

Where does the assumption of individual differences in treatment response come from? In general, antipsychotic drugs are assessed in randomized clinical trials (RCTs), the criterion standard for identifying the efficacy of a treatment. In RCTs, patients are assessed at baseline (eg, with the Positive and Negative Syndrome Scale [PANSS]) and randomized to either a treatment or a control group. What RCTs can ultimately provide is an answer to whether a treatment works in general. This average treatment effect is derived from the direct comparison of the response between the treatment and the control groups, which is imperative in an RCT.³ Understandably, this answer may leave clinicians unsatisfied; after all, they are treating individual, and not typical, patients. From a clinical perspective, patients vary considerably in their response to antipsychotic drugs, and the general response may seem almost like an uninformed guess for the individual patient. Furthermore, clinicians seem to prefer categories such as normal or abnormal and responders or nonresponders to inform diagnostic and therapeutic decisions. A consequence is that many investigators now try to personalize medicine by aiming to tailor treatments to individual patients. They agree that response to treatment varies from patient to patient.

However, estimating individual response to treatment, known as the treatment-by-patient interaction, is more complex than often appreciated and depends on laborious study designs, such as repeated crossover trials.² However, as we illustrate with simulated data, such study designs are needed to distinguish individual response to treatment from other components of variation that are unrelated to permanent differences in treatment response.⁴⁻⁷

By design, RCTs cannot estimate the treatment-by-patient interaction, the index of individual response. Although RCTs do not tell anything about individual response, they might indicate something about the presence of individual response. As recognized early by Fisher,⁸ an increase in variance in the treatment group compared with the control group could indicate the presence of variation in response to treatment.² The strength of this increase

Key Points

Question Is there evidence from randomized clinical trials that patients respond differently to antipsychotic drugs?

Findings In this meta-analysis of 52 randomized clinical trials involving 15 360 patients with a schizophrenia or schizoaffective diagnosis, the outcome variability in the antipsychotic drug treatment group was not higher but slightly lower than that in the placebo control group.

Meaning This study cannot rule out that individual differences in drug response might still exist, but it does question the assumption of a personal element of response to antipsychotic treatment.

would then quantify the size of the personal element of response and provide evidence for the presence of a treatment-by-patient interaction.⁹ A method has been developed to compare variances between groups across studies¹⁰ and has been adopted by a meta-analysis package.¹¹ In psychiatry, this method has been applied to compare variances in brain structure¹² and inflammatory parameters in psychosis.¹³ This method compares the variance of treatment and control by computing their ratio: a ratio of 1 means equal variances, a ratio greater than 1 means more variability in the treatment group, and a ratio smaller than 1 means less variability in the treatment group compared with the control group.^{10,12,13}

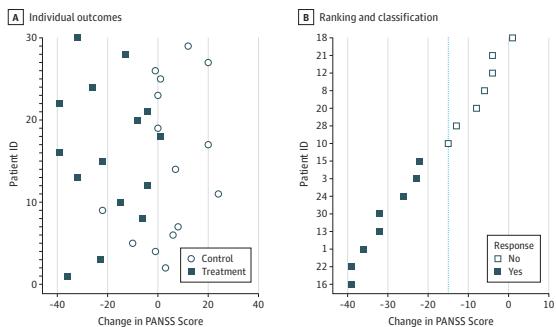
This study is organized in 2 parts. The first part illustrates the different components of variation in RCTs with simulated data, showing the importance of recognizing the treatment-by-patient interaction (which reflects individual treatment response) as the component of interest. The second part shows the results of a meta-analysis, which tested for the presence of treatment-by-patient interaction in empirical data from antipsychotic drug RCTs. We compared the overall variability in the treatment group with the overall variability in the control group, using data from a recently published meta-analysis,¹⁴ summarizing 24 years of placebo-controlled, antipsychotic RCTs in schizophrenia. We hypothesized that compared with control, the often-highlighted heterogeneity in patients with schizophrenia would be reflected by a clinically relevant increase in overall variance of treatment, outcome, which is compatible with a personal element of response that deviates from the estimated average treatment effects.

Methods

Trial Simulation

To illustrate the different components of variation in RCTs, we simulated data from patients with schizophrenia who were randomized to either the antipsychotic treatment or control group and assessed with the PANSS and a positive effect of treatment (Cohen $d = 1.32$; $t_{2,75} = 3.46$; $P = .002$). First, we added a single crossover condition with either a constant or a varying treatment effect, and then we added a double crossover to this simulated trial. With these additions, we show how the variability between and within patients has to be distinguished from the treatment-by-patient interaction, the component reflecting the individual differences in treatment response.

Figure 1. Observed Response Suggests Heterogeneity in Treatment Response



A. Thirty patients with schizophrenia were randomized to either the antipsychotic treatment or the control group. Despite the trial being simulated, with a main effect of treatment (Cohen $d = 1.32$; $t_{22.5} = 3.46$; $P = .002$), it may be tempting to infer from the Positive and Negative Syndrome Scale (PANSS) pretreatment and posttreatment outcome difference scores that some patients in the treatment group responded better than others. This observed pretreatment and posttreatment outcome difference can be misleading given

that individual differences might be merely some unexplained components of variance. **B.** The treatment group patients are ranked according to the observed pretreatment and posttreatment outcome differences and classified as responders or nonresponders based on an arbitrary threshold (dashed light blue line). Although the ranking is not necessary for the classification, it increases the perception of individual differences in response to treatment.

Meta-analysis

To ensure data quality and validity, this meta-analysis was conducted in accordance with the PRISMA guidelines. We searched Cochrane Schizophrenia, MEDLINE/PubMed, Embase, PsycINFO, Cochrane CENTRAL, BIOSIS Previews, ClinicalTrials.gov, and World Health Organization International Clinical Trials Registry Platform from January 1, 1955, to December 31, 2016.

Using the meta-analysis of Leucht et al¹⁵ as a basis, we included published and unpublished double-blind, placebo-controlled RCTs of at least 3 weeks' duration. These studies investigated adults with a diagnosis of schizophrenia spectrum disorders and prescription for licensed antipsychotic medications, except clozapine. Studies were excluded if they investigated relapse prevention, patients with predominant negative symptoms, patients with major concomitant somatic or psychiatric illness, or intramuscular formulations of antipsychotic treatment, or if they were Chinese research. We included only studies that reported the necessary information (mean, SD, and sample size) of the PANSS pretreatment and posttreatment outcome difference scores.

In studies that combined comparisons of multiple antipsychotic drugs with placebo, we calculated an aggregated SD across all comparisons, leaving only 1 SD per study. We extracted the PANSS means and SDs of the pretreatment and posttreatment outcome difference scores as well as the sample sizes for the treatment and the control groups. Further information on the search strategy is published elsewhere.¹⁵

Statistical Analysis

The SDs of the pretreatment and posttreatment outcome difference scores in the treatment and control groups consist of

the same variance components, including the within-patient variation. The treatment group, however, may also include the additional treatment-by-patient interaction, which could indicate the presence of individual response differences. Thus, in the case of a variable treatment effect, an increase of the variance in the treatment group, compared with the control group, should be observable. To assess this variation, we calculated for each comparison between antipsychotic and placebo drugs the relative variability of treatment and control as the log variability ratio ($\log VR$)¹⁶ with

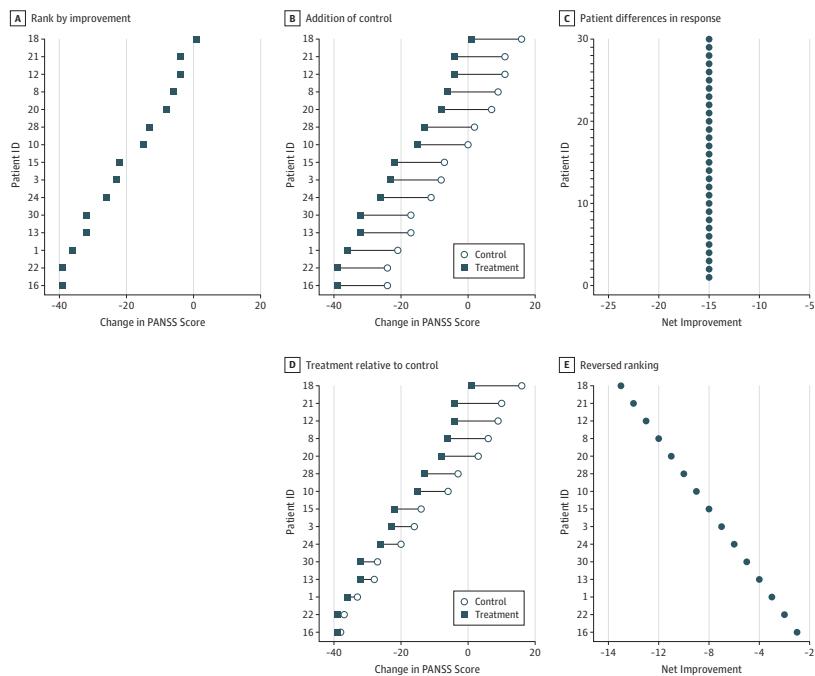
$$\log VR = \log \left(\frac{SD_{Tx}}{SD_{Cx}} \right) + \frac{1}{2(n_{Tx} - 1)} - \frac{1}{2(n_{Cx} - 1)}$$

in which SD_{Tx} was the reported sample SD for treatment, SD_{Cx} was the reported sample SD for control, n_{Tx} was the treatment sample size, and n_{Cx} the control sample size.¹⁰ The corresponding sampling variance ($SD^2_{\log VR}$) for each comparison between antipsychotic and placebo drugs can be expressed as follows:

$$SD^2_{\log VR} = \frac{1}{2(n_{Tx} - 1)} + \frac{1}{2(n_{Cx} - 1)}$$

We did not find an association between the pretreatment and posttreatment outcome difference scores and their respective SDs in the data for the control group ($\beta = 0.16$; $P = .15$; eFigure 1A in the Supplement) or the treatment group ($\beta = -0.05$; $P = .63$; eFigure 1B in the Supplement). For this reason, we did not consider the log coefficient of variation ratio ($\log CVR$) as an additional index for comparing variabilities.¹⁰

Figure 2. Consequences of Between-Patient Variation



Two simulated scenarios are shown: one with a constant treatment effect across patients (A-C), and one with a reversed ranking after a control condition is taken into account (D, E). A, Using the same ranking from the simulated parallel trial, we show that ranking is a flawed approach to quantifying symptom improvement. B, When adding a control condition to the initial parallel trial, the seeming differences in improvement from patients in the treatment group vanish. C, The crossover trial simulation eliminates spurious differences in the outcome

and reveals no between-patient differences in response. Although seemingly unlikely, such a scenario cannot be ruled out from the results of a parallel group trial. In another scenario, a variable treatment effect is added to a parallel trial (A). D, Differences in improvement may reverse the ranking if patients varied in their response to the treatment compared with controls. E, The patient who appeared to have improved the most in D had actually the smallest net improvement. PANSS indicates Positive and Negative Syndrome Scale.

We weighted each log VR with the inverse of this sampling variance¹¹ and entered it into a random-effects model. This approach allows for the quantification of the true individual response, after adjusting for within-patient variability and regression to the mean.^{5,9} Results were back-transformed from the log scale for better interpretability, with a variability ratio higher than 1, indicating greater variability under treatment compared with control, and a ratio lower than 1, indicating less variability under treatment compared with control.

The analysis was performed from October 31, 2018, to March 29, 2019, with the R package metafor, version 2.0.0,¹¹ and the manuscript was produced with the R package knitr, version 1.20, in RStudio (R Foundation for Statistical Comput-

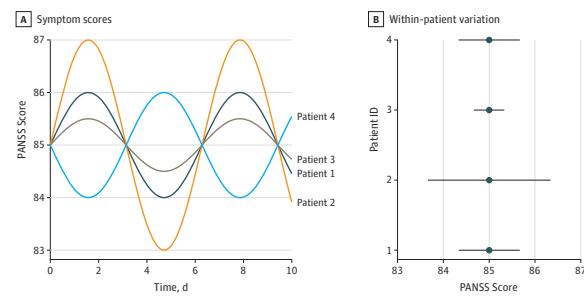
ing). All the data and code we used are freely available online to ensure reproducibility (<https://doi.org/10.17605/OSF.IO/QARVS>).

Results

Simulation

We simulated an RCT of 30 patients with schizophrenia randomized to either the treatment or the control group. The individual pretreatment and posttreatment outcome differences (Figure 1A) might tempt us to infer that some patients in the treatment group responded better than others. We might then rank these patients according to their outcome and clas-

Figure 3. Random Within-Patient Fluctuations



Four patients from the simulated trial were measured repeatedly over time using the Positive and Negative Syndrome Scale (PANSS). A. The differences in a patient's symptom severity scores from time point to time point are independent of any intervention. B. Measuring repeatedly and calculating the means over all time points account for random within-patient fluctuations and reveal that patients had the same mean PANSS score. What differed was the amount of random fluctuation, which might be a highly unlikely scenario but, until tested, cannot be assumed is not true.

sify them as either responders or nonresponders. However, such ranking and classification can be misleading.

Although seemingly different (Figure 2A), adding a simulated crossover condition to the initial parallel trial may reveal that the apparent differences in improvement among patients in the treatment group vanish (Figure 2B) and the treatment effect may actually be constant across patients (Figure 2C). Such a scenario cannot be ruled out from the results of a parallel group trial. In addition, the same ranking (Figure 2A) may reflect yet another scenario, in which differences in improvement as calculated from a crossover condition may reverse the ranking (Figure 2D), such that patients who appeared to have improved the most had actually the smallest net improvement (Figure 2E). Apparent outcome differences among patients in an RCT may still be compatible with a constant treatment effect.

Next, outcome differences may also be found within patients. Assessing patients repeatedly over time might reveal that symptoms fluctuate randomly around the same mean score (Figure 3). This fluctuation shows that within-patient variability alone may suggest differences in treatment response that are a mere reflection of random fluctuation.

Again, we can add a simple crossover condition to the simulated parallel group trial (Figure 4A), in which each patient received both the treatment (antipsychotic drug) and control (placebo). Only by running the crossover trial once again (Figure 4B) can we determine whether the differences observed in the first crossover trial are indeed stable features of the patients. The net improvement from crossover trial 1 may not replicate in crossover trial 2, which indicates that the response differences are still not stable features of the patients (Figure 4C). For that stability to be the case, we would have to see a similar outcome in crossover trial 1 (Figure 4A) as in crossover trial 2 (Figure 4D), in which case we have identified a substantial treatment-by-patient interaction (Figure 4E).

A careful distinction of the sources of variation in a simulated RCT has shown that it is not trivial to distinguish the source of primary interest (treatment-by-patient interaction) from components that tell nothing about individual re-

sponse. In the meta-analysis, we assessed whether evidence exists for such treatment-by-patient interaction across antipsychotic drug trials.

Meta-analysis

We investigated 75 comparisons of antipsychotic drug with placebo in 52 RCTs.¹⁷⁻⁶⁸ None of these studies used a design such as repeated crossovers that would have allowed for a direct estimate of individual responses. Overall, a total of 15 360 patients with a schizophrenia or schizoaffective diagnosis were included, of whom 8550 (55.7%) had been randomized to the treatment group and 6810 (44.3%) to the control group (more details can be found in the eResults in the Supplement).

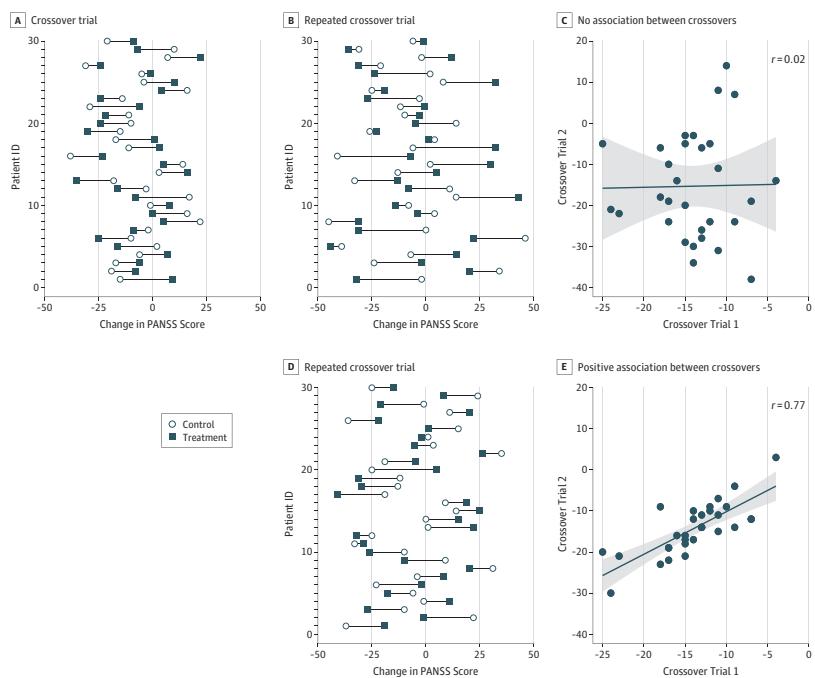
We found an overall lower variability in treatment compared with control (variability ratio = 0.97; 95% CI, 0.95-0.99; $P = .01$; Figure 5¹⁷⁻⁶⁸). This finding indicates that the overall variability across treatment groups was 3% lower compared with that in the control groups. Furthermore, we compared the variances in individual antipsychotic drug outcome and found the same pattern, with lower variability across treatment compared with control (variability ratio = 0.97; 95% CI, 0.95-1.00; $P = .02$; eFigure 2 in the Supplement).

No evidence was found that antipsychotic treatment increased the outcome variance compared with the control. Instead, the outcome variance was slightly lower in the treatment than in the control group.

Discussion

A widespread belief among clinicians and researchers is that patients differ substantially in their antipsychotic treatment response, but finding evidence for this assumption is complex. A likely explanation, supported by the simulations conducted for this study, is that taking an observed treatment response as the true treatment response is tempting, compelling us to ignore the components of variation most likely encountered: random variation within patients and differences between patients. The existing empirical evidence for such in-

Figure 4. Estimating Treatment-by-Patient Interaction With Repeated Crossover Trials



The same simulation is shown of 30 patients with schizophrenia in a parallel trial assessed with the Positive and Negative Syndrome Scale (PANSS). A, Patients received both the antipsychotic and placebo drugs in a crossover trial. B, Only by running a crossover trial more than once can we identify whether individual response is a trait or a permanent feature of the patient. C, Associating the net improvement from crossover trials 1 and 2 (solid dots) shows that advantages from the first trial do not replicate in the second ($r = 0.02$; 95% CI). In this scenario, response to treatment is not a permanent feature of the patient.

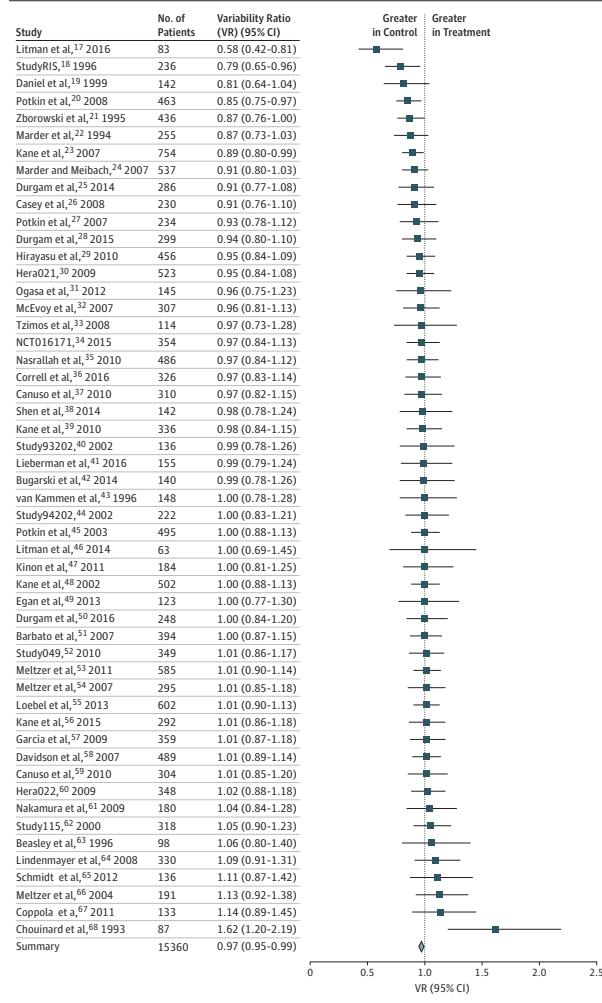
D, Another scenario using the same crossover trial 1 (A) and repeating it, we might observe that the response was almost identical to the response in crossover trial 1. E, Patients responded similarly in both trials ($r = 0.77$; 95% CI). In this scenario, we can consider response to treatment as a trait in these patients. Repeatability of the association from one crossover trial to the next in the same sample is the only way to separate treatment-by-patient interaction from random within-patient variation and to determine whether response is a trait or a state.

dividual differences is weaker than expected: No evidence was found that the antipsychotic drug increased the outcome variance compared with the placebo. Instead, the outcome variance was slightly lower in the treatment group. With this finding, we still cannot rule out that subsets of patients responded differently to treatment, but the overall small difference in variances suggests that the average treatment effect is a reasonable assumption for the individual patient. By assuming heterogeneity in treatment outcomes, we might ultimately introduce noise into clinical practice by refusing to go with the best available evidence, the average treatment effect derived from RCTs.²

Although RCTs are questioned regularly, sometimes using questionable arguments,⁶⁹ they remain the criterion stan-

dard in clinical research. They provide unbiased estimates of the relative efficacy of an intervention, which even the largest observational studies cannot provide.⁷⁰ In addition, appreciating the role of randomization in RCTs is important. Randomization is not compatible with the notion that specific features, such as placebo response, increase in one but not the other group in an RCT. If evidence existed of an enhanced placebo response over time, as has been suggested repeatedly in the past years,¹⁴ this response would have been apparent in both the control and the treatment groups because of randomization and thus would have canceled out. Furthermore, the concept of placebo response, although regularly investigated,⁷¹ cannot be studied by looking at the observed responses in con-

Figure 5. Lower Variability in the Treatment Group Compared With the Control Group Across Antipsychotic Drug Parallel Trials



The forest plot shows the VR together with its 95% CI for treatment vs control across 52 studies. Each study is listed with its respective citation number. The overall VR was lower for treatment compared with control group.

trol groups,⁷² for the same reasons that this approach does not work for the treatment groups, as this study has shown.

Comparing the variabilities between treatment and control groups may provide valuable insight into the presence of indi-

vidual response and the scope of personalized medicine. Recently, other groups have taken a similar approach to assess the presence of individual differences in brain structure¹² and immunological parameters in psychosis.¹³ We assumed that, in the presence of

a personal element of response to treatment, the variance in the treatment group should be higher compared with the control group, which in turn would require further investigation (eg, with n-of-1 trials).⁷³⁻⁷⁵ However, our results indicate that overall variability in the treatment groups was slightly lower, if only by a modest amount (1% to 5%). One explanation might be that the treatment had a stabilizing quality⁹ that reduced the variability in the treatment group. An example for such variance stabilization might be the floor effect, in which the assessment instrument is too coarse to capture patient improvements over a certain level.

Nevertheless, given the slightly lower variability under treatment compared with control found in this study, we cannot rule out that individual differences in response to antipsychotic drugs might still exist. A subset of ill patients may have responded well to treatment, whereas less affected patients may not have improved, resulting in an overall decreased variance under treatment than under control.⁹ Yet, the finding of a narrow CI around an overall only slightly lower variability suggests that substantial differences in drug response are rather unlikely. Thus, analyses aimed at estimating individual response might be premature until these differences have been shown to exist and to be clinically relevant.

As the simulations have shown, labeling patients as responders might be misleading. The label suggests that true response has been established as a permanent feature of the patient, even though the label is a mere reflection of the observed response, which includes true response plus regression to the mean, some placebo effect, and random terms such as measurement error. Thus, response rates that are calculated in RCTs reflect observed but not true response. We suggest that biomarker research aimed at identifying response to treatment of individuals or subgroups should consider the possibility that

treatment outcome is less heterogeneous than anticipated and might even be close to constant across individuals.

Limitations

This meta-analysis has some limitations. First, the calculation of the pretreatment and posttreatment outcome difference scores varied between studies. Although some RCTs calculated the differences between outcome and baseline PANSS scores, others used analysis of covariances with the baseline PANSS scores and additional variables as covariates. Thus, some of the included SDs of change were adjusted for covariates but others were not. Second, the use of pretreatment and posttreatment outcome difference scores might lead to a loss of information and might not be sensitive enough to capture differences in response to treatment.⁷⁶ Third, we assumed that individual responses to treatment were reflected by increased variance in the treatment group. Yet, this increased variance could have also indicated the presence of subgroups who responded differently to the treatment.⁹ Such a case would argue for stratified medicine rather than personalized medicine, in which subgroups of patients receive varying treatments. As any interaction, a treatment-by-patient interaction is ultimately scale dependent, which means it can be removed by transformation of the scale.⁷⁷

Conclusions

Until the differences in individual response to treatment have been demonstrated with careful designs, the overall small differences in outcome variance suggest that the average treatment effect is a reasonable assumption for the individual patient.

ARTICLE INFORMATION

Accepted for Publication: April 27, 2019.

Published Online: June 3, 2019.

doi:10.1001/jamapsychiatry.2019.1530

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Author Contributions: Drs Homan and Winkelbeiner had full access to all of the data in the study and take responsibility for the integrity of the data and the accuracy of the data analysis.

Concept and design: Winkelbeiner, Leucht, Homan. **Acquisition, analysis, or interpretation of data:**

All authors.

Drafting of the manuscript: Winkelbeiner, Homan. **Critical revision of the manuscript for important intellectual content:** All authors.

Statistical analysis: Winkelbeiner, Homan.

Obtained funding: Winkelbeiner. **Administrative, technical, or material support:** All authors. **Supervision:** Homan.

Conflict of Interest Disclosures: Dr Winkelbeiner reported grants from Swiss National Science Foundation during the conduct of the study. Dr Leucht reported personal fees from LB Pharma International, H Lundbeck A/S, Otsuka Pharmaceutical, Teva Pharmaceutical Industries Ltd, LTS Lohmann Therapy Systems, Gedeon Richter, Recordati SpA, MSD, Boehringer Ingelheim and Sandoz, Janssen Pharmaceutical, Eli Lilly & Company, SanofiAventis, and Servier Laboratories outside of the submitted work; reanalysis of a clinical trial together with Geodon Richter and the publication of its results; and honoraria from Johnson & Johnson, MSD, Angelini, and Sunovion. Dr Kane reported grants from Otsuka, Lundbeck, and Janssen, as well as other from Allergan, Allergan, Forum, Genentech, Lundbeck, Intracellular Therapies, Janssen, Johnson & Johnson, Merck, Neurocrine, Otsuka, Pierre Fabre, Reviva, Roche, Sunovion, Takeda, Teva, Vanguard Research Group, and LB Pharmaceuticals outside of the submitted work. No other disclosures were reported.

Meeting Presentation: The results of this study were presented at the World Congress of Biological

Psychiatry, June 3, 2019, Vancouver, British Columbia, Canada.

Additional Contributions: The authors thank Majnu John, PhD, Department of Mathematics, Zucker School of Medicine at Northwell/Hofstra, for advice on the analysis of the current study, as well as Stephen Senn, PhD, Methodology and Statistics, Luxembourg Institute of Health, and Daniel Guinart, MD, Department of Psychiatry, Zucker School of Medicine at Northwell/Hofstra, for helpful comments on the manuscript. These individuals received no additional compensation, outside of their usual salary, for their contributions.

Additional Information: All data and code are freely available online to ensure reproducibility (<https://doi.org/10.17605/OSF.IO/QARVS>).

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Supplementary Online Content

Winkelbeiner S, Leucht S, Kane JM, Homan P. Evaluation of differences in individual treatment response in schizophrenia spectrum disorders: a meta-analysis. *JAMA Psychiatry*. Published online June 3, 2019. doi:10.1001/jamapsychiatry.2019.1530

eResults

eFigure 1. No Associations Between Means and SDs

eFigure 2. Variability for Treatment vs Control Across All Investigated Antipsychotic Drugs

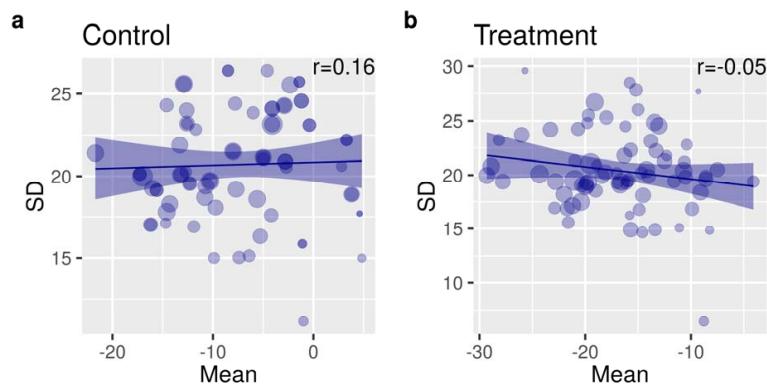
This supplementary material has been provided by the authors to give readers additional information about their work.

eResults

For the studies we had access to, 26 (35%) used an analysis of covariance model, 1 (1%) a mixed model for repeated measures, 25 (33%) an analysis of variance, and 8 (11%) a constrained longitudinal data analysis, whereas information was not available from 15 (20%) studies.

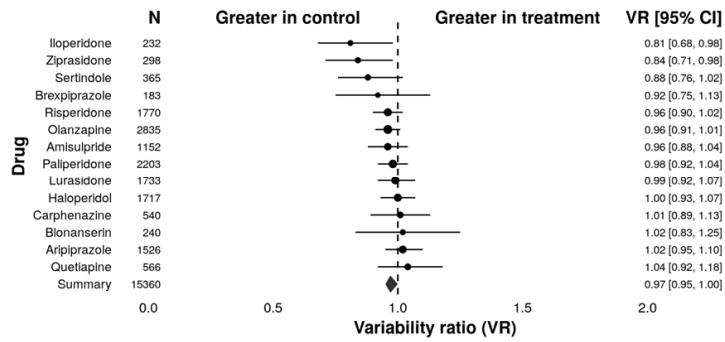
The models included covariates of which ‘baseline’ was the most frequent in 43 (57%) comparisons followed by ‘center’ in 25 (33%), ‘baseline-by-visit’ in 25 (33%), ‘body weight’ in 25 (33%), ‘age’ in 25 (33%), ‘height’ in 25 (33%), ‘duration of hospitalization’ in 25 (33%), ‘number of hospitalization’ in 25 (33%), and ‘age at onset’ in 25 (33%).

eFigure 1. No Associations Between Means and SDs



a, b. The mean pretreatment and posttreatment difference scores were not significantly associated with the SDs across studies for control ($\beta = 0.16$; $P = .15$) or for treatment ($\beta = -0.05$; $P = .62$).

eFigure 2. Variability for Treatment vs Control Across All Investigated Antipsychotic Drugs



The forest plot shows the result of a random-effects model of the variability ratio (VR) of treatment versus control across all investigated antipsychotic drugs ($VR = 0.97$; 95% CI, 0.95; 1.00; $P = .02$). The size of the dots reflects the sample size of the respective study. N, number of patients.

5.5 Winkelbeiner et al. 2018, Schizophrenia Research



Letter to the Editor

Decreased blood flow in the right insula and middle temporal gyrus predicts negative formal thought disorder in schizophrenia



Dear Editors,

Formal thought disorder (FTD) causes severe disturbances of language and communication, and affects 50%–80% of people with schizophrenia (Roche et al., 2015). It is part of the disorganization syndrome that has been found to distinguish patients with schizophrenia from patients with psychotic bipolar disorder, and may, thus, inform differential diagnosis (Palaniyappan et al., 2018). The breakdown of effective communication in FTD is associated with structural and functional alterations of the fronto-temporal language network (Cavelti et al., 2018; Horn et al., 2009; Kircher et al., 2008; Palaniyappan et al., 2015; Strik et al., 2017; Sumner et al., 2017) but might extend to areas outside this network including the amygdala (Cavelti et al., 2018; Spalletta et al., 2010; Sumner et al., 2017), hippocampus (Cavelti et al., 2018; Kircher et al., 2008; Spalletta et al., 2010; Sumner et al., 2017), and insula (Cavelti et al., 2018; Horn et al., 2009; Palaniyappan et al., 2015; Sumner et al., 2017). This range of FTD-associated brain areas might be partly due to the variety of symptoms subsumed under "FTD". Thus, a differentiation into potential subtypes such as positive FTD (i.e., unanticipated, bizarre, or inappropriately expressed speech) and negative FTD (i.e., speech reduced in quantity, content, or fluency) as suggested by factor analytical studies might be more accurate (Roche et al., 2015). Positive FTD has been associated with decreased cortical thickness of the superior temporal gyrus (STG) (Horn et al., 2009), increased activity in the middle temporal gyrus (MTG) (Kuperberg et al., 2007), and decreased volume of the amygdala (Spalletta et al., 2010) and hippocampus (Kircher et al., 2008; Spalletta et al., 2010), whereas negative FTD has been associated with decreased gray matter volume in areas including the MTG and insula (Palaniyappan et al., 2015). However, no study to date has investigated both structural and functional measures to determine if positive and negative FTD reflect two neurophysiologically distinct entities. Therefore, we examined cortical thickness, subcortical volume, and blood perfusion of language (MTG and STG) and language-associated brain areas (amygdala, hippocampus, and insula) as predictors of positive and negative FTD in schizophrenia.

We included twenty-four right-handed and German-speaking patients with a schizophrenia spectrum disorder according to the DSM-IV-TR between 18 and 65 years of age. Severity of FTD was assessed using the Thought, Language, and Communication (TLC) scale, with the subscales Disorganization and Emptiness providing measures of positive and negative FTD. All subjects gave written informed consent. The study was approved by the ethics committee of the Kanton Bern, Switzerland (see the Supplementary Material for details on the study sample and comparisons with healthy controls).

A 3 T Magnetom Verio system (Siemens Erlangen, Germany) was used to create the high-resolution 3D T1-weighted images with a gradient echo and the absolute cerebral blood flow (CBF) images with pseudo continuous arterial spin labeling. Structural images were processed using FreeSurfer 5.1.0, perfusion images were analyzed using FSL 5.0. Mean regional CBF (rCBF), cortical thickness, and subcortical volume values were extracted for every region of interest (see Supplementary Fig. 1). We used multivariable linear regression analyses in R 3.1.3 with the TLC Total score ($n = 24$), Emptiness score ($n = 16$), and Disorganization score ($n = 16$) as dependent variables, the normalized rCBF, cortical thickness, and subcortical volume as independent variables, and age and whole brain global cortical thickness or subcortical volume, respectively, as covariates. The missing values for the TLC subscales were due to missing single items, while the total scores were obtained for all participants. Results were corrected to adjust for false discovery rate (FDR) and considered significant for $p < 0.05$ (see the Supplementary Material for more details).

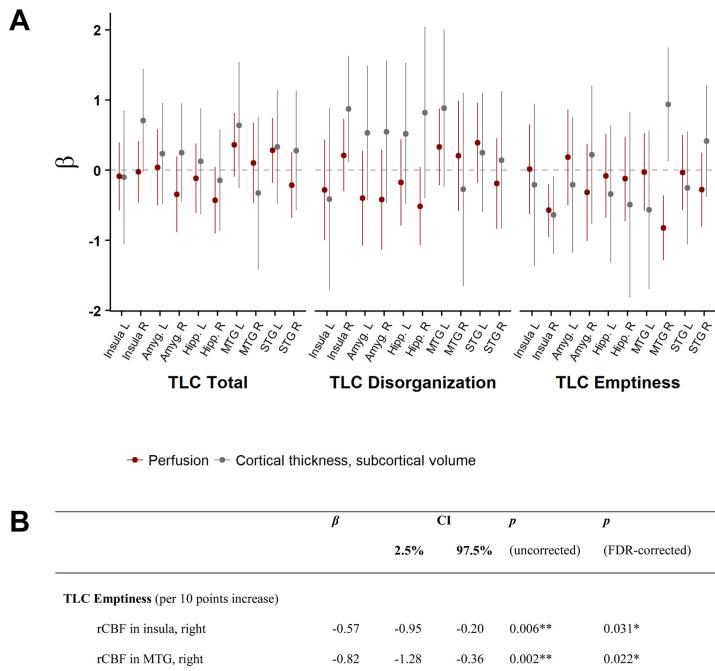
Negative FTD (TLC Emptiness) was significantly correlated with decreased rCBF in the right insula (FDR-corrected $p = 0.031$; Fig. 1) and in the right MTG (FDR-corrected $p = 0.022$; Fig. 1). Positive FTD (TLC Disorganization) did not significantly correlate with structural or metabolic measures ($p > 0.050$; Fig. 1).

This study provided evidence that metabolic changes in the right insula and MTG contribute to negative FTD in schizophrenia. This finding might be best understood in the light of the disconnection hypothesis which suggests that a perturbed lateralization of usually left-sided language areas leads to more diffuse language processing (Kircher et al., 2008). As the insula is also involved in the integration and sequencing of information into a stringent stream of consciousness (Strik et al., 2017), the decreased rCBF found in this region could explain deficient planning of utterances of speech underlying FTD. Yet, we did not find evidence for structural correlates of negative FTD, as well as structural or metabolic correlates of positive FTD. While this is in contrast to previous studies (Horn et al., 2009; Kircher et al., 2008; Kuperberg et al., 2007; Palaniyappan et al., 2015; Spalletta et al., 2010), it is in line, nonetheless, with frequently reported null findings (for a review see Sumner et al., 2017). The most likely explanation is the small sample size of the current study that limits a conclusive decision whether the non-significant findings actually reflect the absence of effects altogether or whether the effects were present but too small to be detected.

While this study provided preliminary evidence that abnormal blood perfusion contributes to negative FTD, studies investigating larger samples are needed to elucidate whether positive and negative FTD effectively reflect distinct entities of FTD with different neurophysiological characteristics.

Funding

This research has been supported by grants from the Swiss National Science Foundation (#320030_146789, #152619, #127359).



Abbreviations. CI, confidence interval; rCBF, regional cerebral blood flow; FDR, false discovery rate; MTG, middle temporal gyrus; TLC, Thought, Language, and Communication scale.

Multivariate linear regression; Significance at: *, $p < 0.05$; **, $p < 0.01$.

Fig. 1. A priori defined regions of interest (ROI) with cortical thickness or subcortical volume and regional cerebral blood flow (rCBF). A. Standardized regression coefficients with 95% confidence intervals are shown. Mean beta values (dots) represent the partial correlations (positive > 0 ; negative < 0) of regional cerebral blood flow (perfusion; red), cortical thickness, and subcortical volume (gray) in the ROIs (amygdala (Amyg.), hippocampus (Hipp.), insula, middle temporal gyrus (MTG), and superior temporal gyrus (STG)) with formal thought disorder (FTD). Confidence intervals not touching the zero line indicate an uncorrected significant effect at $p < 0.05$. B. Significant results of multivariable linear regression analyses. Partial correlations of the Thought, Language, and Communication (TLC) scores for negative FTD (TLC Emptiness) with metabolic measures before and after false discovery rate (FDR)-correction.

Contributors

AF, WS, HH, and PH designed the study. MC collected the data. SW, AF, and PH analyzed the data. SW and MC wrote the first draft of the manuscript. All authors contributed to and approved the final manuscript versions.

Conflicts of interest

None.

Acknowledgement

The authors thank Karin Laimböck for helping with the data collection.

Appendix A. Supplementary material

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.schres.2018.06.009>.

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5 March 2018
Available online xxxx

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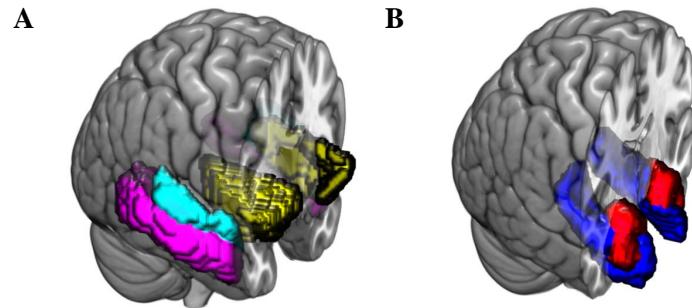
Supplementary Material for
“Decreased blood flow in the right insula and MTG predict negative formal thought disorder in schizophrenia”

This file includes:

Supplementary Figure 1

Supplementary Tables 1 - 3

Figure



Supplementary Figure 1. Regions of interest (ROIs). ROI masks were created using the Harvard-Oxford atlas (Desikan et al., 2006) and the Jülich histological atlas (Eickhoff et al., 2007). ROIs were chosen bilaterally based on previous findings of reduced laterality of the language system (Kircher et al., 2002; Palaniyappan and Liddle, 2012). **A.** Cortical ROIs including the insula (yellow), the middle temporal gyrus (cyan), and the superior temporal gyrus (violet). **B.** Subcortical ROIs including the amygdala (red) and the hippocampus (blue).

Materials and methods

Subjects

This study was conducted at the University Hospital of Psychiatry in Bern, Switzerland, between 2011 and 2014. Our sample of 37 subjects included 24 patients with a schizophrenia spectrum disorder according to the DSM-IV-TR (Association, 2000) and 13 healthy controls, recruited among the employees of the hospital and through personal contacts. Inclusion criteria for patients were a diagnosis of a schizophrenia spectrum disorder confirmed by an experienced clinician, age between 18 and 65 years, sufficient fluency in German, and right-handedness according to the Edinburgh Inventory (Oldfield, 1971). Inclusion criteria for healthy controls further comprised no current major psychiatric DSM-IV Axis I diagnoses. Exclusion criteria for both patients and controls were left-handedness, pregnancy, any contraindications for MRI (e.g., metal-containing implants such as a pacemaker or cochlear implant, claustrophobia), history of serious neurological issues, or current abuse of alcohol and/or psychoactive substances (apart from nicotine). General psychopathology was assessed using the Positive and Negative Syndrome Scale (PANSS) (Kay et al., 1987). Severity of FTD was assessed using the Thought, Language, and Communication (TLC) (Andreasen, 1986) scale. The TLC subscales TLC Disorganization and TLC Emptiness provided measures of positive and negative FTD, respectively (Nagels et al., 2013).

All subjects gave written informed consent. Assessment of psychopathology as well as magnetic resonance imaging (MRI) were conducted on the same day by a trained clinician. The study was approved by the ethics committee of the Kanton Bern, Switzerland.

Data acquisition

Magnetic resonance imaging (MRI) was performed on a 3T Magnetom Verio system (Siemens Erlangen, Germany) with a 12-channel array head coil. Structural images were acquired using high-resolution 3D T1-weighted imaging with a gradient echo (MP-RAGE). The 160 sagittal slices of 1.0 mm thickness, 256 x 256 mm² field of view (FOV), and a matrix size 256 x 256 had a voxel size of 1 x 1 x 1 mm³. Further scan parameters were 2.30 ms repetition time (TR), 2.98 ms echo time (TE), and 900 ms inversion time (TI). Absolute cerebral blood perfusion was measured using pseudo continuous arterial spin labeling (pCASL) (Dai et al., 2008; Wu et al., 2007). The following parameters were used: TR/TE = 4000/30 ms, FOV= 230 x 230 mm², matrix size 64 x 64; six slices at a distance of 1.5 mm; slice thickness 6.0 mm; bandwidth 1446 kHz per pixel; echo spacing 0.78 ms; number of measurements n = 100. Labeling was performed at 90 mm below the isocenter of the imaging region and a post-labeling delay of 1.25 s was set (to allow the labeled water protons to perfuse into the imaging slices), with a label time of 1.6 s.

Data analysis

Cortical thickness and subcortical volume were calculated using the T1-weighted MR images and Freesurfer 5.1.0 (<http://www.surfer.nmr.mgh.harvard.edu/>). The technical details of these procedures have been described in prior publications (Dale et al., 1999; Fischl et al., 1999). For further analyses, we used the cortical thickness and subcortical volume estimates of the ROIs that Freesurfer calculated based on the Desikan-Killiany atlas' parcellation (Desikan et al., 2006) and segmentation of the whole brain.

Perfusion images were analyzed with the Functional MRI of the Brain (FMRIB) Software Library (FSL 5.0) (Smith et al., 2004). First, the raw pCASL data was motion-

corrected with the FMRIB Linear Registration Tool (MCFLIRT) (Jenkinson et al., 2002) and outliers were detected and removed. Second, in order to create a mean difference map for each subject, control-tag pairwise differences were computed. These maps were normalized by the equilibrium magnetization (M_0) of arterial blood estimated via the magnetization of cerebral spinal fluid (CSF). Third, Bayesian Inference for Arterial Spin Labeling MRI (BASIL) (Chappell et al., 2009) was employed to calculate resting state perfusion, which provides blood flow in absolute units ([ml/100g/min]), with the following parameters: BAT = 1.25, TR = 4.0, TE = 3.0, T1 (blood) = 1.65 s, and alpha = 0.85. The application of a gray matter (GM) mask and partial volume correction (Chappell et al., 2011) ensured that only GM voxels were included in the CBF calculation. Fourth, after spatial normalization and transformation into MNI space with FLIRT (Jenkinson et al., 2002) and FNIRT, the FMRIB Non-Linear Registration Tool (Andersson et al., 2007), perfusion data was spatially smoothed with a 6 mm FWHM Gaussian kernel. Lastly, anatomical perfusion masks were created for the ROIs (amygdala, hippocampus, insula, MTG, and STG) based on the Harvard-Oxford atlas (Desikan et al., 2006) and the Jülich histological atlas (Eickhoff et al., 2007) in MNI space (see Supplementary Figure 1). The mean regional CBF (rCBF) for every ROI was extracted by masking the whole brain with the ROI-specific mask, and normalized by dividing the regional by the global CBF. The normalized rCBF for each ROI was then used for further analyses.

Statistical analysis

In order to examine the relationship of cortical thickness, subcortical volume, and rCBF measures in the ROIs with severity of FTD, a series of multivariable linear regression analyses was performed, using the statistical software package R 3.1.3. The TLC Sum score ($n = 24$), TLC Emptiness score ($n = 16$), and TLC Disorganization score ($n = 16$) for all schizophrenia patients were entered as dependent variables, the normalized rCBF values, cortical thickness,
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and subcortical volume values of the ROIs as independent variables, and age and whole brain global cortical thickness or subcortical volume, depending on the ROI being cortical or subcortical, as covariates. The missing values for the TLC Emptiness and TLC Disorganization scales are due to missing single TLC items that are needed for the calculation of these subscales. To account for multiple comparisons, the false discovery rate (FDR) method was employed. Results below $p < 0.05$ (FDR corrected) were considered significant.

Tables

Supplementary Table 1. Patient's demographic and clinical characteristics.

Characteristic	Controls	Patients	Group comparison		
	M ± SD (n = 13)	M ± SD (n = 24)	Statistic	Value	p
Gender no. F/Ma	8/5	7/17	<i>Fisher's exact</i>	NA	0.083
Age (years)	29.31 ± 3.99	34.21 ± 10.91	<i>Wilcoxon</i> -test	133	0.473
Education (years)	17.50 ± 1.08	12.75 ± 2.91	<i>Wilcoxon</i> -test	220	0.001***
Diagnosis DSM-IV-TR 295.9/295.7/295.4	NA	16/2/6	NA		
Age of onset	NA	24.77 ± 7.48	NA		
Duration of illness (years)	NA	9.51 ± 7.84	NA		
CPZE (mg)	NA	957.71 ± 1681.33	NA		
PANSS					
Positive Syndrome	NA	16.38 ± 5.36	NA		
Negative Syndrome	NA	12.75 ± 4.99	NA		
General Psychopathology	NA	28.83 ± 6.92	NA		
Total Score	NA	57.96 ± 12.05	NA		
TLC (median [range])					
Emptiness	NA	2.00 [0.00 - 26.00]	NA		
Disorganization	NA	2.00 [0.00 - 8.00]	NA		
Total Score	NA	8.00 [1.00 - 30.00]	NA		

Abbreviations: F, female; Ma, male; M, mean; NA, not applicable; DSM-IV-TR, Diagnostic and Statistical Manual of Mental Disorders; CPZE, chlorpromazine equivalents; PANSS, Positive and Negative Syndrome Scale; SD, standard deviation; TLC, Thought, Language, and Communication scale.

Significant at: *, p < 0.05; **, p < 0.01; ***, p < 0.001.

Supplementary Table 2. Mean (\pm SD) cortical thickness and subcortical volume values.

Regions of interest	Controls	Patients	Group differences		
	$M \pm SD$	$M \pm SD$	t	df	p
	[range]	[range]			
Amygdala ($\times 10^3$ mm 3)					
Left	1.65 \pm 0.21 [1.27 - 2.00]	1.52 \pm 0.30 [0.51 - 1.94]	-1.45	35	0.157
Right	1.68 \pm 0.24 [1.21 - 2.05]	1.62 \pm 0.23 [1.06 - 2.02]	-0.81	35	0.424
Hippocampus ($\times 10^3$ mm 3)					
Left	4.01 \pm 0.31 [3.54 - 4.50]	3.77 \pm 0.57 [2.23 - 4.81]	-1.40	35	0.172
Right	4.14 \pm 0.36 [3.57 - 4.61]	3.93 \pm 0.48 [3.01 - 4.79]	-1.38	35	0.178
Insula (mm)					
Left	3.02 \pm 0.11 [2.83 - 3.23]	2.94 \pm 0.17 [2.60 - 3.39]	-1.52	35	0.137
Right	2.98 \pm 0.15 [2.65 - 3.13]	2.95 \pm 0.20 [2.63 - 3.46]	-0.54	35	0.595
MTG (mm)					
Left	2.88 \pm 0.11 [2.72 - 3.05]	2.84 \pm 0.18 [2.47 - 3.29]	-0.67	35	0.504
Right	2.85 \pm 0.13 [2.59 - 3.02]	2.80 \pm 0.16 [2.55 - 3.19]	-0.86	35	0.395
STG (mm)					
Left	2.85 \pm 0.10 [2.74 - 3.01]	2.81 \pm 0.18 [2.47 - 3.17]	-0.74	35	0.462
Right	2.86 \pm 0.14 [2.64 - 3.18]	2.79 \pm 0.15 [2.57 - 3.15]	-1.46	35	0.154
Whole brain					
Volume ($\times 10^6$ mm 3)	505.86 \pm 51.31 [427.58 - 562.06]	482.38 \pm 65.16 [346.26 - 607.20]	-0.71	35	0.485
Thickness (mm)	2.46 \pm 0.09 [2.27 - 2.61]	2.44 \pm 0.11 [2.15 - 2.69]	-1.12	35	0.270

Abbreviations. M, mean; MTG, middle temporal gyrus; SD, standard deviation; STG, superior temporal gyrus.

Significant at: * $p < 0.05$.

Supplementary Table 3. Mean (\pm SD) normalized regional and absolute global CBF.

Regions of interest	Controls		Patients		Group differences		
	$M \pm SD$		$M \pm SD$		t	df	p
	[range]	[range]	[range]	[range]			
	(n = 13)		(n = 24)				
rCBF in amygdala							
Left	0.70 \pm 0.10 [0.54 – 0.86]		0.81 \pm 0.11 [0.62 – 1.04]		2.86	35	0.007**
Right	0.67 \pm 0.11 [0.52 – 0.88]		0.75 \pm 0.12 [0.52 – 1.03]		2.09	35	0.044*
rCBF in hippocampus							
Left	0.89 \pm 0.15 [0.67 – 1.12]		0.93 \pm 0.11 [0.76 – 1.11]		0.95	35	0.347
Right	0.80 \pm 0.17 [0.62 – 1.14]		0.80 \pm 0.09 [0.63 – 0.95]		-0.03	35	0.978
rCBF in insula							
Left	1.21 \pm 0.14 [1.01 – 1.51]		1.15 \pm 0.19 [0.83 – 1.51]		0.93	35	0.361
Right	0.93 \pm 0.21 [0.77 – 1.42]		0.81 \pm 0.15 [0.59 – 1.30]		-1.95	35	0.059
rCBF in MTG							
Left	1.34 \pm 0.27 [0.98 – 1.79]		1.38 \pm 0.20 [0.87 – 1.68]		0.55	35	0.583
Right	1.20 \pm 0.15 [1.01 – 1.57]		1.22 \pm 0.14 [0.99 – 1.56]		0.53	35	0.598
rCBF in STG							
Left	1.27 \pm 0.30 [0.86 – 1.91]		1.30 \pm 0.18 [0.88 – 1.52]		0.393	35	0.697
Right	1.05 \pm 0.21 [0.84 – 1.57]		1.13 \pm 0.17 [0.93 – 1.56]		1.32	35	0.233
Global CBF [ml/100g/min]							
	47.55 \pm 12.95 [30.79 – 82.73]		40.67 \pm 9.27 [24.10 – 63.35]		-1.87	35	0.070

Abbreviations. rCBF, regional cerebral blood flow; M, mean; MTG, middle temporal gyrus; SD, standard deviation; STG, superior temporal gyrus.

Significant at: *, $p < 0.05$; **, $p < 0.01$.

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