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Peer Review File

Observation of conformal field theory spectra in a quantum simulator

Corresponding Author: Professor Manuel Endres

Version 0:

Reviewer comments:

Referee #1

(Remarks to the Author)
This manuscript reports experimental measurements of properties of conformal field theories (CFTs) using a programmable quantum simulator based on Rydberg atom chains, focusing on the (1+1)-dimensional Ising and tricritical Ising (TCI) theories. To this end, it introduces a modulation spectroscopy technique which induces direct transitions between allowed states when these can be spectroscopically resolved, or enables measurement of the dynamical structure factor (DSF).

I find the results, and in particular the Ising results, exciting and impressive. The experiments are technically sophisticated and demonstrate a high level of control. The adaptation of modulation spectroscopy to atom arrays (previously introduced by the last author in the context of optical lattice quantum simulators) is, in my view, a major contribution to the experimental toolbox of programmable quantum simulators. The work builds naturally on recent progress in Rydberg arrays (refs. 39–42), yet clearly goes beyond it by providing a direct spectroscopic window into low-energy excitations of critical systems. To my knowledge, no other current platform enables this level of detailed experimental access to CFT-related physics. I therefore view this manuscript as potentially highly impactful, and as an important step toward systematic experimental studies of CFTs.

That being said, I believe the manuscript should be revised before it can be suitable for publication in Nature. The main issue is not the quality of the data, but the interpretation and presentation of several of the results, especially in the TCI part, and to lesser extent the DSF part. With more careful wording, clearer separation between experimental observations and numerically supported interpretations, and a slightly more conservative framing of some claims, I believe the paper would be very strong.

The central claim of the paper is that the experiment resolves finite-size spectra consistent with those expected from the Ising and tricritical Ising CFTs, and that boundary control allows access to different sectors. For the Ising case, the data convincingly support this statement: the observed peaks, their scaling with system size, and the parity selection rules together provide strong evidence that the measured spectra reflect the expected low-energy structure. To complete the picture, a measurement of the degeneracy would be instructive, but even without it the results are striking.
For the tricritical Ising part, the situation is more nuanced. The manuscript presents several lines of evidence in support of realizing the TCI point and its associated boundary conformal field theory. While the data are certainly suggestive, I do not find that they collectively provide sufficiently strong evidence to support a claim of experimental observation of the TCI universality class.
First, the collapse of the boundary order parameter is consistent with the expected behavior near the TCI point, but could also be explained by other means, such as just being in the disordered phase.
Second, the primary spectroscopic signature - the ratio E_2/E_1 - is extracted from a partially resolved spectral feature using a model-dependent two-Gaussian fit. The underlying data do not clearly resolve two distinct peaks, and the data and fit are not sufficiently convincing to support a firm conclusion.
Third, the boundary-tuning experiments, while conceptually appealing, do not provide independent confirmation. The ratio observed in the strongly pinned (fixed boundary) case is equally consistent with ordinary Ising behavior. The identification of the intermediate (unstable) boundary condition relies on another noisy and model-dependent fit yielding $E_2/E_1 = 2.5(0.4)$,

which deviates significantly from the predicted value 10/3 and is in fact closer to the value of 2 within uncertainties, and thus also consistent with Ising.

Finally, the quench dynamics measurements offer only qualitative support. The plotted oscillations decay within approximately 1.5 cycles, making reliable frequency extraction difficult; moreover, the description “long lived oscillations” is somewhat tenuous given the data. The fit parameters and uncertainties are not given. The apparent agreement with theory strongly relies on a very broad regime that could be explained away by uncertainty in V_2.

Taken together, these observations form an encouraging picture that is roughly suggestive of the TCI interpretation, especially when supported by numerical modeling. However, the evidence, which is centered on fits to data shown in 4b, 5c, ED fig.6, and is consistent only under the assumption of broad variation due to noise, remains underconstrained. I therefore recommend that the authors soften their language throughout the manuscript, framing the results as suggestive of TCI behavior rather than as a direct observation.

The discussion of the dynamical structure factor is interesting and promising, but should also be presented more cautiously. Experimentally, the approximately constant low-frequency response is demonstrated for a single system size. While this is consistent with expectations for the Ising case, it is not yet sufficient to claim an experimental extraction of a universal scaling function. It would be more appropriate to present this as a proof-of-principle demonstration of access to dynamical response.

Additionally, I encourage the authors to more clearly distinguish between what is directly established experimentally and what relies on numerical modeling or theoretical input. In several places, numerical results are described in a way that could be misread as experimental observations, and clarifying this would improve the manuscript. It would also help to more explicitly note that the observed spectra reflect both the underlying theory and the selection rules imposed by the probe.

Specific comments:

line 47: it is unclear why the PYHP error correcting code is mentioned here.

Fig. 1: the data along the dotted line in panel d and the data presented in panel e represent the same measurements but show different frequencies; this is only clarified later in the Methods via a different Rabi frequency. This should be explained in the caption, or the frequencies should be normalized to Omega.

line 134: $V_1 \to \infty$ is stated, but V_1 does not appear in Eq. (1).

line 139: H_2 is mentioned as containing higher-order terms, and the Methods section is invoked, but I could not find a discussion of H_2 in the Methods.

line 178: here "Rabi oscillation cycle" is mentioned. The only Rabi frequency that has been introduced is the single atom Rabi frequency. I'm assuming the authors are not referring to that here, because it is significantly faster than typical modulation frequencies, and so this would imply that not even half of a modulation cycle is carried out in most cases. I'm guessing that the authors are referring to the many body coherent oscillation frequency, but this requires clarification.

line 228: the statement about coupling to degenerate states is misleading; coupling depends on the modulation operator and need not include all states in a degenerate manifold.

line 248: it would be helpful to briefly explain when and why the CFT description breaks down. Currently this sentence appears without context or explanation

line 272: it's inaccurate that the “expected pattern” was detected in either the odd or even case, since the degeneracies, which are a central component of the pattern, were not measured.

lines 277–284: this paragraph describes numerical rather than experimental results, but this is not stated.

lines 284–287: "observing the onset of linear dispersion" from two points seems too strong.

throughout: “envelop” should be replaced with “envelope”.

Fig. 6c: this panel represents numerical results and should be labeled as such in caption.

line 924: typical AODs, in particular when used in high-resolution contexts, have a long rise time (often over 1us) and therefore weak response at high frequency modulation. Can you comment on using such AODs to perform modulations which at times reach ~10 MHz? Was the modulation amplitude calibrated according to the AOD frequency response?

line 998: ED Fig. 2 appears to be numerical and this should be stated explicitly.

line 1013: a word seems to be missing.

line 1062: does the equidistant spectrum influence other measurements? e.g. in the Ising spectrum shown in figures 1-3, is the response really due only to levels at that energy, or is some of the population excited further up the ladder?

As a sidecomment: coupled equidistant levels would in principle enable creation of a many body coherent states (in analogy to the QHO) via resonant drive: could this be used to detect the CFT?

line 1069: could one directly measure populations of specific levels instead of using δn, and if not, why?

line 1097: it would be helpful to provide single-atom Rabi and Ramsey decay times, and comment on their relation to the observed many-body decay.

line 1240: why is degeneracy of the first excited state important for Eq. (3)?

lines 1248 and 1371: the choice of operator O appears somewhat arbitrary. Couldn't O have been chosen to be some other diagonal operator that gives 0 for the ground state and +1 for whatever set of levels, extending the validity regime of Eq. 3?

line 1280: this statement is unclear, as any pure state can be written as a superposition of eigenstates. Why does the next sentence follow?

line 1284: why is a Gibbs state assumed?

line 1435: “irrelevant” is presumably meant in the RG sense; how does this apply in a finite system?

line 1934: why must K and Q be sums of local operators?

line 2031: fit results for frequencies and decay times, including uncertainties, are missing.

Question to the authors:

In Cardy’s discussion of boundary CFTs, boundary conditions correspond to RG fixed points that emerge under coarse graining. In the present work, one boundary condition appears to arise naturally, while another is enforced microscopically through engineered detunings. In a finite system, where RG flow is not fully developed, is there a meaningful conceptual distinction between these two realizations of boundary conditions?

Beyond these detailed comments, my main suggestions for improving the paper are: (i) to more cautiously calibrate the

strengths of claims throughout the manuscript, in particular with regard to the TCI and the DSF, or alternatively provide stronger corresponding experimental evidence; and (ii) more clearly separate experimental results from numerical results and numerically supported interpretations.
In summary, this is an impressive and important piece of work that introduces a powerful experimental technique and provides compelling results, especially in the Ising regime. With a more careful presentation, I would be supportive of publication in Nature.

Referee #2

(Remarks to the Author)
The manuscript reports on the experimental observation of conformal field theory (CFT) spectra. Many important theoretical results, covering a broad range of areas of physics, have been obtained over the years from the powerful tools of CFT since its inception more than 40 years ago. Yet, in the past, only some aspects of select theoretical CFT results have been probed directly in experiment. In particular, the large amount of exact and universal information contained in finite-size spectra of CFTs has never been experimentally accessed. The present manuscript is the first to accomplish this. This is a remarkable, highly original, and very significant achievement of this work. The method the authors use for this purpose is 'modulation spectroscopy' which operates in the time-domain. This method, as well as the results obtained using it, are both clearly explained in the manuscript (including the methods section). Specifically, the manuscript reports results for finite-size spectra of a Rydberg chain tuned to its quantum phase transition in the critical and tricritical Ising universality class, for various boundary conditions. Those experimentally obtained spectra are shown to agree excellently with exact analytical results known for these two critical points. The authors also measured responses at the critical point, allowing them to extract the dynamical structure factor determined by CFT correlation functions.

The manuscript provides explicit and understandable summaries of theoretical background on the properties of the considered CFTs, so that non-expert readers can directly appreciate the relationship with the experimentally obtained results. Abstract and introduction provide a clear presentation of the context in which this work is situated, conceptually and historically. The conclusion section provides perspectives of the expected impact of this paper on future questions, involving the use of programmable quantum simulators.

In view of these comments, I strongly recommend publication of the manuscript in Nature.

Referee #3

(Remarks to the Author)
This paper presents an experimental observation of the spectra and other universal features of various conformal field theories (CFT) realizable in a Rydberg atom quantum simulator, such as the Ising CFT and the tricritical Ising CFT. The authors present a protocol they call the modulation-ramp-probe sequence: prepare the ground state (near the critical point), apply a modulation to couple to excited states with given modulation frequency, ramp deep into the disordered or ordered phases, then image the population transferred onto excited states which deep in the phases correspond to density fluctuations. They demonstrate that one can observe signatures of the CFT spectra, in particular, their universal energy ratios, while performing finite size scaling; they also show how to probe different symmetry resolved sectors and observe the effect of different boundary conditions.

Overall, the science demonstrated and methods implemented are impressive --- it is nice that one can directly experimentally image some of the predictions from CFT, especially their excitation spectra, which as far as I understand, have remained largely theoretical. Thus I am appreciative on this front.

That said, one criticism I have is that the observation relies heavily on known understanding and numerical studies of the effective model realised in the experimental system. For example, the phase diagram of Fig 1 is a numerically obtained figure, from which one determines the critical detuning to stop the experimental adiabatic state preparation at. Without such prior knowledge, would one be able to reliably and robustly prepare (an approximation) of the critical ground state?

Secondly, the modulation spectroscopy appears to have limitations --- for example, it cannot resolve the degeneracies in the CFT spectra. Can the authors comment on how this may be overcome?

Third, the choice of global detuning to use as the probe operator is, I can understand, the easiest modulation to perform experimentally. But is there a good understanding of how robust or universal such a modulation would be couple to excited states in the CFT spectrum for more generic CFTs? What if there were some reason the operator does not couple to a given excited state, or has very weak overlap? Then the spectroscopy might fail. Of course, for the cases studied in this paper, the modulation seems to work just fine, but "just fine" entails a double checking and confirmation with numerics to declare post facto success.

Thus my main question is really how generic and how universal the methods employed in this paper are, especially in light of the need to have prior knowledge (theoretically and numerically) of the CFT spectra to be probed. In which case the question of probing the CFT spectra becomes more like a post facto verification as opposed to an a priori spectroscopy of unknown spectra. Notwithstanding the above comments, I want to stress that I am still very appreciative of the involved and impressive experimental techniques and high quality data presented, as well as the excellent manuscript, so I am still favourable of publication if my questions are well addressed.

Referee response for "Observation of conformal field theory spectra in a quantum simulator"

We thank all referees for their careful assessment of our work and their insightful comments. These changes helped us to further strengthen the manuscript, and have helped emphasize its broad interest across several communities and stress its substantial advancement to the field. Below, reviewer comments are in 'blue', while our responses are black, and quotes from the revised manuscript are highlighted in "red".

RESPONSE TO REVIEWER 1

'This manuscript reports experimental measurements of properties of conformal field theories (CFTs) using a programmable quantum simulator based on Rydberg atom chains, focusing on the (1+1)-dimensional Ising and tricritical Ising (TCI) theories. To this end, it introduces a modulation spectroscopy technique which induces direct transitions between allowed states when these can be spectroscopically resolved, or enables measurement of the dynamical structure factor (DSF).'

'I find the results, and in particular the Ising results, exciting and impressive. The experiments are technically sophisticated and demonstrate a high level of control. The adaptation of modulation spectroscopy to atom arrays (previously introduced by the last author in the context of optical lattice quantum simulators) is, in my view, a major contribution to the experimental toolbox of programmable quantum simulators. The work builds naturally on recent progress in Rydberg arrays (refs. 39–42), yet clearly goes beyond it by providing a direct spectroscopic window into low-energy excitations of critical systems. To my knowledge, no other current platform enables this level of detailed experimental access to CFT-related physics. I therefore view this manuscript as potentially highly impactful, and as an important step toward systematic experimental studies of CFTs.'

We thank the referee for their positive evaluation of our manuscript.

'That being said, I believe the manuscript should be revised before it can be suitable for publication in Nature. The main issue is not the quality of the data, but the interpretation and presentation of several of the results, especially in the TCI part, and to lesser extent the DSF part. With more careful wording, clearer separation between experimental observations and numerically supported interpretations, and a slightly more conservative framing of some claims, I believe the paper would be very strong.'

We thank the referee for the positive assessment of our data quality and for the constructive suggestions regarding presentation. We agree that careful distinction between direct experimental observations and numerically supported interpretations is important, and we have revised the manuscript accordingly. The principal changes are:

1. In the TCI section, we have softened assertive claims to reflect the degree of experimental support. Key examples include: reframing the σ_{edge} collapse as one component of a multi-pronged characterization rather than standalone evidence; explicitly noting that the two-Gaussian decomposition of the TCI spectral feature is motivated by numerics rather than independently resolved; changing "demonstrated experimentally" to "find experimental signatures" for the TCI boundary conditions; and recasting the quench dynamics as a proof-of-principle demonstration.
2. Throughout the manuscript, we have revised language to clearly separate experimental observations from numerically supported interpretations.
3. For the DSF results, we have added qualifying language emphasizing that the approximately constant low-frequency response constitutes "an encouraging first indication of access to the dynamical scaling regime" rather than a definitive extraction of the universal scaling function.

That said, we respectfully note that the case for TCI rests on the mutual consistency of several independent lines of evidence—boundary order parameter collapse at criticality, spectral ratios inconsistent with Ising, recovery of Ising-like ratios under boundary detuning, and non-monotonic dependence on boundary detuning strength—none of which is naturally explained by alternative scenarios such as being in the disordered phase or at an ordinary Ising critical point. We have added clarifying remarks throughout to make this reasoning more transparent. We address each specific point below.

'The central claim of the paper is that the experiment resolves finite-size spectra consistent with those expected from the Ising and tricritical Ising CFTs, and that boundary control allows access to different sectors. For the Ising case, the data convincingly support this statement: the observed peaks, their scaling with system size, and the parity selection rules together provide strong evidence that the measured spectra reflect the expected low-energy structure. To complete the picture, a measurement of the degeneracy would be instructive, but even without it the results are striking.'

We thank the referee for this positive assessment of our Ising spectroscopy results.

Regarding the resolution of degeneracies, we refer the referee to our response to a similar question below. As discussed there, a frequency-only modulation spectrum cannot, by itself, distinguish states that are exactly degenerate in energy. More generally, resolving individual states within a degenerate manifold requires access to a set of perturbing operators $\{\hat{O}_\mu\}$ whose transition matrix elements provide independent information within that subspace. Specifically, the transition strengths $|\langle e|\hat{O}_\mu|g\rangle|^2$ must depend on both the state $|e\rangle$ and the operator label μ, so that measurements with different perturbations yield sufficient information to distinguish the states.

We make theoretical progress in this direction for the Ising critical point by considering spatially structured detuning modulations, $\hat{K}_k = \sum_j \cos(kj+\alpha)\hat{n}_j$, and measuring the response as a function of wavevector k. In the free-fermion description of the Ising CFT, different two-fermion states within a degenerate energy manifold can be distinguished through their wavevector-dependent transition matrix elements $|\langle e|K_k|g\rangle|^2$ (see Ext. Data Fig. 4). Whether analogous methods can be employed to resolve degenerate manifolds in more general interacting or unknown CFTs is an interesting open question that we highlight in the revised manuscript.

'For the tricritical Ising part, the situation is more nuanced. The manuscript presents several lines of evidence in support of realizing the TCI point and its associated boundary conformal field theory. While the data are certainly suggestive, I do not find that they collectively provide sufficiently strong evidence to support a claim of experimental observation of the TCI universality class.'

'First, the collapse of the boundary order parameter is consistent with the expected behavior near the TCI point, but could also be explained by other means, such as just being in the disordered phase.'

We agree with the referee that a small σ_{edge} alone does not uniquely identify the TCI point, since the disordered phase would also exhibit suppressed boundary order. However, the σ_{edge} measurements are not performed at a single isolated parameter point; rather, they track the evolution of the boundary order parameter along the phase boundary, where each configuration is independently tuned to its critical detuning Δ_c via finite-size scaling (Methods, Ext. Data Fig. 2). At the Ising critical points (configurations 1 and 2), the "fixed" boundary condition is the unique stable boundary condition, and accordingly we observe sizable σ_{edge}. The near-complete collapse of σ_{edge} at configuration 3, while the system remains tuned to criticality, signals a qualitative change in the boundary physics that is inconsistent with the Ising CFT. If the system were simply in the disordered phase, one would additionally expect spectral ratios inconsistent with any CFT prediction across all three fixed points (See Ref. Fig. 1).

That said, we agree with the referee that σ_{edge} should not be presented as standalone evidence for TCI. We have revised the manuscript to frame it more carefully as one component of a multi-pronged characterization.

The manuscript now reads "First, we assess edge CDW order in $L = 26, 27$ chains at the critical detuning Δ_c (Methods, Ext. Data Fig. 2), directly after quasi-adiabatic state preparation without modulating. The operator $\hat{\sigma}_{i+1/2} = (-1)^{i+1}(\hat{n}_i - \hat{n}_{i+1})$ defines a local CDW order parameter, which we measure at the boundaries to obtain $\sigma_{\text{edge}} \equiv \left(|\langle\hat{\sigma}_{3/2}\rangle| + |\langle\hat{\sigma}_{L-1/2}\rangle|\right)/2$ at configurations 1, 2, and 3 in Fig. 4a. Upon tiptoeing towards the TCI point, we observe a reduction and eventual nearly complete collapse of σ_{edge} (Fig. 4a main panel), which is suggestive of a qualitative change in boundary physics, consistent with the emergence of a stable "free" boundary condition characteristic of the TCI CFT."

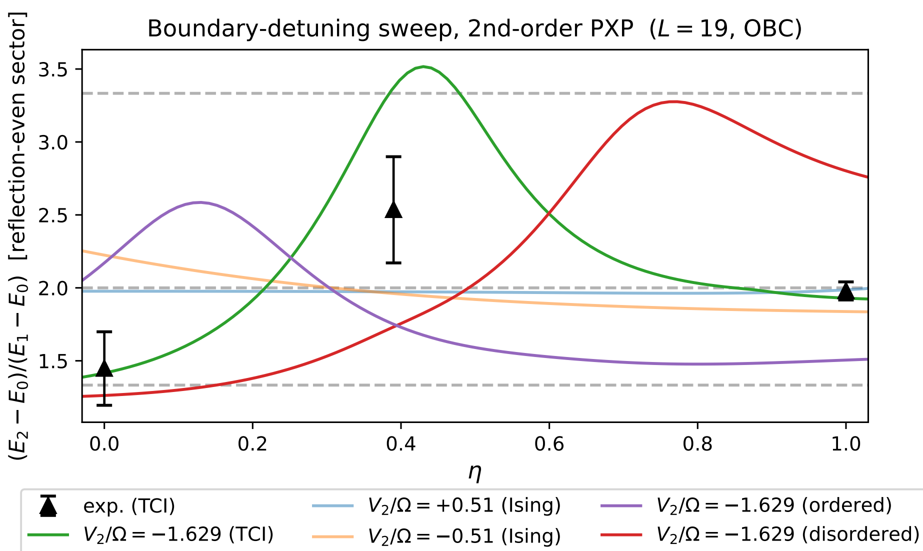
'Second, the primary spectroscopic signature - the ratio E_2/E_1 - is extracted from a partially resolved spectral feature using a model-dependent two-Gaussian fit. The underlying data do not clearly resolve two distinct peaks, and the data and fit are not sufficiently convincing to support a firm conclusion.'

We agree with the referee that the two peaks are not fully resolved in the experimental data. However, the two-Gaussian decomposition is not a blind fit; rather, it is motivated by the theoretically predicted spectrum, which contains two excitations in this energy range. We have revised the manuscript to present this more cautiously, stating that the spectral line shape is consistent with the predicted E_2/E_1 ratio, rather than claiming a definitive spectroscopic extraction. Now it reads "At the TCI point, by contrast, we observe only one skewed spectral feature. Motivated by the theoretically predicted spectrum, we fit the feature with a sum of two Gaussians whose centers we associate with even-parity energies E_1 and E_2 (Fig. 4b, right)." We view this as further motivation for future experiments with improved spectral resolution to fully resolve the two excitations.

'Third, the boundary-tuning experiments, while conceptually appealing, do not provide independent confirmation. The ratio observed in the strongly pinned (fixed boundary) case is equally consistent with ordinary Ising behavior. The identification of the intermediate (unstable) boundary condition relies on another noisy and model-dependent fit yielding $E_2/E_1 = 2.5(0.4)$, which deviates significantly from the predicted value 10/3 and is in fact closer to the value of 2 within uncertainties, and thus also consistent with Ising.'

Resolving the ratio $E_2/E_1 = 10/3$ is difficult because this boundary condition is unstable and requires precise tuning of multiple control parameters. Consequently, small deviations from the ideal tuning can lead to appreciable shifts in the measured spectrum. While our current precision does not allow a stringent quantitative test of the predicted value (10/3), the observed enhancement of (E_2/E_1) relative to the Ising value of (2) is a qualitative signature that the system is tuned into the vicinity of the unstable boundary fixed point. The more robust distinction from the ordinary Ising case comes instead from the behavior at $\eta = 0$. In an Ising critical chain, the natural boundary condition for the boundary spin is fixed: accessing the free boundary fixed point would require explicitly tuning the boundary field. By contrast, at the TCI point the free boundary fixed point is stable. Therefore, in the absence of an applied boundary field, the system naturally flows to the free boundary condition, for which the expected ratio is $E_2/E_1 = 4/3$. This observation is not naturally explained by ordinary Ising boundary criticality and provides the clearest evidence that the boundary physics is governed by the TCI universality class.

Further confidence that the TCI point was experimentally accessed can be gained by qualitatively comparing the boundary detuning sweep in Fig. 4 with analogous sweeps at other Hamiltonian parameters, as shown in Ref. Fig. 1. At Ising criticality, the energy ratio varies monotonically with the boundary detuning strength, η, in contrast to both the experimental and numerical TCI curves. Nearby ordered and disordered points at the same interaction strength can also display non-monotonicity, but they are clearly distinguished from the TCI case at strong boundary detuning, $\eta = 1$.



Ref. Fig. 1 | Excited state gap ratio $(E_2 - E_0)/(E_1 - E_0)$ in the reflection-even sector as a function of boundary detuning strength η for an $L = 19$ open chain. Experimental data prepared at the TCI point are compared with numerical calculations at the TCI point, two nearby off-critical points with $\Delta/\Omega = (1 \pm 0.05)\Delta_c/\Omega$ where Δ_c is the TCI critical detuning, and two Ising critical points with $V_2/\Omega = \pm 0.51$.

'Finally, the quench dynamics measurements offer only qualitative support. The plotted oscillations decay within approximately 1.5 cycles, making reliable frequency extraction difficult; moreover, the description "long lived oscillations" is somewhat tenuous given the data. The fit parameters and uncertainties are not given. The apparent agreement with theory strongly relies on a very broad regime that could be explained away by uncertainty in V_2.'

We agree with the referee that extracting a reliable oscillation frequency from a fast-decaying signal is challenging, and that the V_2 uncertainty broadens the range of agreement with theory. Regarding the latter point, we note that the V_2 uncertainty affects both the quench and spectroscopy predictions in the same way, so the internal consistency between the two independent techniques (orange and green data points in Ext. Data Fig. 6d) is not inflated by this uncertainty. The primary value of the quench measurement is thus as a complementary check: the trend of E_1 with boundary detuning η extracted from quench dynamics agrees with the trend from modulation spectroscopy, providing qualitative support that does not rely on a single technique.

Following the referee's suggestion, we have softened the language throughout, framing the quench dynamics as a proof-of-principle demonstration rather than a quantitative extraction. We have also added fit parameters and uncertainties in Ext. Data Fig. 6d. We respectfully note that the term "long-lived oscillations" is used exclusively to describe our numerical findings (Ext. Data Fig. 6c), not the experimental data; to make this distinction clearer, we now explicitly refer to "damped oscillations" when discussing the experimental measurements.

We have restructured the corresponding Methods paragraph to present the numerical predictions first and the experimental results second, making the distinction transparent. The revised text reads "Here we prepare a non-equilibrium low-energy TCI state by adiabatically ramping our system close to the TCI point and then quenching the detuning to Δ_c (Ext. Data Fig. 6a). To understand the expected dynamics, we first numerically simulate the quench for different local detuning strengths η. For all $\eta \in [0, 1]$, we observe long-lived oscillations (Ext. Data Fig. 6c), with the fitted frequencies consistent with the first excited state energy E_1 (yellow data points in Ext. Data Fig. 6d), suggesting an oscillation between the ground state and the first excited state of the TCI CFT. Experimentally, after holding at the TCI point for varying durations, we measure the sum of local fields, $\sum_i \sigma_i$, and observe damped oscillations (Ext. Data Fig. 6b). We record the oscillations at boundary detunings $\eta = 0.5, 0.75$ and fit the data to a trial function $a + b\cos(\omega t + \phi)e^{-\frac{t}{\tau}}$; the fit frequencies $\omega = 2\pi f$ (green data points in Ext. Data Fig. 6d) are compared with the numerically obtained E_1, where we see qualitative agreement, up to the uncertainty in V_2. Due to the fast decay of the experimental oscillations, we regard the observed oscillation as a proof-of-principle qualitative demonstration of quench dynamics near the TCI point."

'Taken together, these observations form an encouraging picture that is roughly suggestive of the TCI interpretation, especially when supported by numerical modeling. However, the evidence, which is centered on fits to data shown in 4b, 5c, ED fig.6, and is consistent only under the assumption of broad variation due to noise, remains underconstrained. I therefore recommend that the authors soften their language throughout the manuscript, framing the results as suggestive of TCI behavior rather than as a direct observation.'

We thank the referee for this balanced assessment. Following the referee's recommendation, we have softened the language throughout the TCI section of the manuscript, ensuring that claims are framed as consistent with TCI behavior rather than as definitive observations. Specific revisions include cautious reframing of the σ_{edge} collapse, explicit acknowledgment that the two-Gaussian decomposition in Fig. 4b is theory-motivated rather than independently resolved, and recasting the quench dynamics as a proof-of-principle demonstration (as detailed in our point-by-point responses above).

That said, we wish to emphasize that the case for TCI rests not on any single measurement but on the mutual consistency of several independent observations: (i) the collapse of σ_{edge} while the system remains tuned to criticality, which is inconsistent with the Ising CFT where only fixed boundary conditions are stable; (ii) the spectral ratio $E_2/E_1 = 1.45(25)$ at $\eta = 0$, consistent with the free-boundary-condition TCI prediction of $4/3$ and inconsistent with the Ising value of 2; (iii) the recovery of $E_2/E_1 \approx 2$ upon applying boundary detunings ($\eta = 1$), consistent with TCI fixed boundary conditions; and (iv) the non-monotonic dependence of E_2/E_1 on η, which is a qualitative signature of the unstable intermediate fixed point unique to TCI. To further substantiate this last point, we include in this response a comparison of the measured $E_2/E_1(\eta)$ with numerical calculations at the TCI point, at two nearby off-critical detunings, and at the Ising critical points (Ref. Fig. 1). The non-monotonic structure observed in both the experimental and numerical TCI data is absent in the Ising cases, providing additional evidence that the boundary physics is governed by TCI rather than Ising universality.

'The discussion of the dynamical structure factor is interesting and promising, but should also be presented more cautiously. Experimentally, the approximately constant low-frequency response is demonstrated for a single system size. While this is consistent with expectations for the Ising case, it is not yet sufficient to claim an experimental extraction of a universal scaling function. It would be more appropriate to present this as a proof-of-principle demonstration of access to dynamical response.'

We thank the referee for this suggestion and agree that a cautious framing is appropriate given the single system size. We would like to point out that our manuscript already adopts this tone in key places: the technique is introduced explicitly as a "proof-of-principle" demonstration of the modulation-probe sequence, and the discussion of the low-frequency response is presented as being "consistent with" expectations rather than as a definitive extraction. Nonetheless, to ensure this cautious framing is maintained throughout, we have added further qualifying language in the discussion of the DSF results, reinforcing that this constitutes a proof-of-principle access to dynamical response rather than a claim of having extracted the universal scaling function. It now reads "$\cdots$, we indeed find an almost-constant response at low frequencies ($\omega \lesssim \Omega$) (Fig. 6b), providing an encouraging first indication of access to the dynamical scaling regime.".

'Additionally, I encourage the authors to more clearly distinguish between what is directly established experimentally and what relies on numerical modeling or theoretical input. In several places, numerical results are described in a way that could be misread as experimental observations, and clarifying this would improve the manuscript. It would also help to more explicitly note that the observed spectra reflect both the underlying theory and the selection rules imposed by the probe.'

We thank the referee for this suggestion. We have carefully gone through the manuscript as well as the referee's comments and revised the language to clearly distinguish between direct experimental observations, numerically supported interpretations, and theoretical predictions.

'Specific comments:'

'line 47: it is unclear why the PYHP error correcting code is mentioned here.'

We thank the Referee for the comment. We removed the sentence, as we believe it inessential and are looking to decrease the length of the manuscript.

'Fig. 1: the data along the dotted line in panel d and the data presented in panel e represent the same measurements but show different frequencies; this is only clarified later in the Methods via a different Rabi frequency. This should be explained in the caption, or the frequencies should be normalized to Omega.'

We thank the referee for pointing out this potential confusion. We have added a clarifying note in the caption of Fig. 1 explaining that panels d and e show the spectroscopy at different Rabi frequencies. We also now reference the relevant Methods section directly in the caption for readers seeking further detail.

'line 134: $V_1 \to \infty$ is stated, but V_1 does not appear in Eq. (1).'

The dependence on V_1 is implicit in $\hat{H}_2$. We direct the referee to Eq. 8.

'line 139: H_2 is mentioned as containing higher-order terms, and the Methods section is invoked, but I could not find a discussion of H_2 in the Methods.'

We direct the referee to Eq. 8, found in the "Determining the critical point" section of the Methods section. We have also included a more clear direction in the main text, replacing "(Methods)" with "(Eq. 8 in Methods)".

'line 178: here "Rabi oscillation cycle" is mentioned. The only Rabi frequency that has been introduced is the single atom Rabi frequency. I'm assuming the authors are not referring to that here, because it is significantly faster than typical modulation frequencies, and so this would imply that not even half of a modulation cycle is carried out in most cases. I'm guessing that the authors are referring to the many body coherent oscillation frequency, but this requires clarification.'

The referee is correct – we are referring to the many-body coherent oscillation frequency, not the single-atom Rabi frequency. We have clarified this in the revised manuscript by replacing "Rabi oscillation cycle" with "many-body Rabi oscillation cycle".

'line 228: the statement about coupling to degenerate states is misleading; coupling depends on the modulation operator and need not include all states in a degenerate manifold.'

We agree with the referee that the modulation operator need not include all states in a degenerate manifold, and it is indeed the observation we had in Fig. 1e. Yet, we believe there is a misunderstanding. Therefore, we change the language to clarify this point. Now it reads "Despite the degeneracy in the Ising CFT spectrum, it is lifted in a finite-size system by irrelevant perturbations. In each ideally degenerate manifold, only one state strongly couples to the ground state under global modulation (Fig. 1e)."

'line 248: it would be helpful to briefly explain when and why the CFT description breaks down. Currently this sentence appears without context or explanation'

We thank the referee for pointing out that our original sentence lacked sufficient context. We expect the leading order, fixed-point CFT description to hold only for energies well below the scale set by the microscopic model, $\frac{E-E_0}{\hbar} \ll \Omega$. In this regime, the relevant excitations are emergent long-wavelength fermions, whose small-k dispersion is well approximated by the continuum CFT. At energies which are comparable to the microscopic scale, non-universal corrections contribute, manifesting in non-linearity in the dispersion relation.

We've updated the text as follows:

"The $L = 7$ spectrum does not, however, collapse to the universal ratio. The leading low-energy CFT description we are using assumes excitation energies well below the microscopic scale of the Rydberg Hamiltonian, $(E_e - E_0)/\hbar \ll \Omega$. At such small system sizes, the energy spacings are sufficiently large for even the second even-parity excited state energy to be comparable to the Rabi frequency and therefore non-universal corrections, including non-linearity of the dispersion and irrelevant interaction terms, are expected to become appreciable."

'line 272: it's inaccurate that the "expected pattern" was detected in either the odd or even case, since the degeneracies, which are a central component of the pattern, were not measured.'

We thank the referee for the comment. We change the language to "a similar pattern of energy levels, whose energy ratio we also detect experimentally", which accurately describe what we actually obtain from the modulation spectroscopy.

'lines 277–284: this paragraph describes numerical rather than experimental results, but this is not stated.'

We thank the referee for the comment. We now emphasize that this part is a numerical result. The manuscript now reads "Further numerical evidence for linear dispersion comes from finite-wavevector modulation, $\cdots$"

'lines 284–287: "observing the onset of linear dispersion" from two points seems too strong.'

We agree with the referee that the claim is too ambitious, given we only have two points to draw from. We've softened the claim as follows: "When interpreted as the onset of linear dispersion, our parity-resolved spectrum in Fig. 3b agrees well with the numerically calculated velocity (Fig. 3c)."

'throughout: "envelop" should be replaced with "envelope".'

We thank the referee for this comment and we have corrected these typos.

'Fig. 6c: this panel represents numerical results and should be labeled as such in caption.'

We thank the referee for pointing out this accidental omission. We added "Numerical demonstration of $\cdots$" to the caption.

'line 924: typical AODs, in particular when used in high-resolution contexts, have a long rise time (often over 1μs) and therefore weak response at high frequency modulation. Can you comment on using such AODs to perform modulations which at times reach ∼10 MHz? Was the modulation amplitude calibrated according to the AOD frequency response?'

We thank the referee for this interesting question. We note that our setup uses an AOD operating in a fast longitudinal mode ($v = 6$ km/s) with a small beam waist ($w = 0.25$ mm). This configuration sacrifices resolution (yielding only < 10 resolvable spots) in favor of speed. The resulting rise time is $\tau_r = 1.3w/v = 0.05$ μs. The frequency modulation transfer function is

$$M = e^{-\frac{1}{8}\pi^2 f_m^2 \tau_r^2},$$

giving a modulation bandwidth of $f_m = \sqrt{8 \ln 2}/(\pi\tau_r) = 0.6v/w = 13$ MHz. This exceeds the Rabi frequencies and modulation frequencies used in the majority of our measurements. We agree that accurately probing the system response beyond this bandwidth would require a more careful calibration of the AOD frequency response.

'line 998: ED Fig. 2 appears to be numerical and this should be stated explicitly.'

We thank the referee for pointing out this accidental omission. We change the title of ED Fig. 2 to "Numerically locating the Ising...".

'line 1013: a word seems to be missing.'

We thank the referee for the comment and change the sentence to ""We then vary V_2 to compare the ...""

'line 1062: does the equidistant spectrum influence other measurements? e.g. in the Ising spectrum shown in figures 1-3, is the response really due only to levels at that energy, or is some of the population excited further up the ladder?'

From Eq. 3 of the main text one can see that, to leading order in modulation amplitude A (we choose A to be small compared to Ω so that this is a good approximation), excited states which are not targeted by $f = \frac{E_e - E_g}{\hbar}$ are exponentially suppressed by the Gaussian envelope. The transition to the second equidistant state requires injecting two excitations, therefore its strength will be $\propto A^4$, which is also suppressed by the small modulation amplitude.

'As a sidecomment: coupled equidistant levels would in principle enable creation of a many body coherent states (in analogy to the QHO) via resonant drive: could this be used to detect the CFT?'

We thank the referee for the interesting question. A similar idea was discussed in Ref. arXiv:1510.07142, where excitations associated with a primary operator were interpreted as coherent states of the conformal group. In this sense, resonant driving could help reveal the tower structure associated with a given primary operator. However, such a probe would be sensitive only to the particular tower addressed by the drive, so a full identification of the CFT would still require resolving different primary operators.

'line 1097: it would be helpful to provide single-atom Rabi and Ramsey decay times, and comment on their relation to the observed many-body decay.'

The single-atom Rabi coherence time is primarily limited by shot-to-shot intensity noise, while the single-atom Ramsey coherence time is primarily limited by laser frequency noise. In the many-body setting, however, both noise sources couple nontrivially to both types of coherent oscillations. For instance, laser frequency noise at the modulation frequency f effectively modulates the drive amplitude, contributing to dephasing of the many-body Rabi oscillation. We have added the following remarks to the manuscript:

"We postulate that the dominant factors limiting the contrast decay of both the Rabi and Ramsey oscillations are the intensity noise and frequency noise of the Rydberg laser, which also limit the single-atom Rabi and Ramsey coherence times to 8 μs and 4 μs, respectively. In the many-body case, both noise sources couple nontrivially to the Rabi and Ramsey coherent oscillations."

'line 1069: could one directly measure populations of specific levels instead of using δn, and if not, why?'

'line 1240: why is degeneracy of the first excited state important for Eq. (3)?'

'lines 1248 and 1371: the choice of operator O appears somewhat arbitrary. Couldn't O have been chosen to be some other diagonal operator that gives 0 for the ground state and +1 for whatever set of levels, extending the validity regime of Eq. 3?'

We thank the referee for these detailed questions about the modulation spectroscopy protocol. We address each point in turn.

Measuring populations of specific levels (line 1069): With the *modulation-ramp-probe* sequence, directly resolving populations in individual excited states is generally difficult. After the adiabatic ramp into the ordered or disordered phase, many low-energy critical excited states map into the same degenerate manifold (split only by a fraction of V_2), effectively mixing them. The measured δn therefore reports the *total* population transferred out of the ground state, but not how that population is distributed among individual excited states. For small system sizes (e.g., $L = 7$), the residual splitting on the order of V_2 is large enough to identify whether the system is in the ground state, the first excited state, or higher excited states; for the larger systems studied here, this is not feasible.

Role of degeneracy for Eq. (3) (line 1240): The degeneracy is important for interpreting the peak *heights*, not the peak positions. The peak height for a given excited state $|e\rangle$ is proportional to $(O_{ee} - O_{gg})|K_{ge}|^2$ (Eq. 7). In our protocol, all low-energy excited states of the critical Hamiltonian are mapped by the adiabatic ramp into the same degenerate excited manifold of the final Hamiltonian, ensuring that $O_{ee} - O_{gg} = 1$ uniformly. This allows us to drop the O-dependent factor and interpret peak heights directly as transition strengths $|K_{ge}|^2$. If the excited states after ramping were non-degenerate, O_{ee} would vary from state to state, and the peak heights would reflect an unknown, state-dependent weighting. We note that this requirement concerns only the interpretation of peak heights; the peak positions (i.e., the excitation energies) are unaffected regardless of degeneracy.

Choice of operator $\hat{O}$ (lines 1248, 1371): We agree that $\hat{O}$ can in principle be chosen differently. The operator the referee proposes—infidelity (1− fidelity with the ground state)—gives $O_{gg} = 0$ and $O_{ee} = 1$ for all excited states by construction, and its response theory has been developed in [Tsai, Sun et al., PRX Quantum **6**, 010331 (2025)]. This would indeed extend the validity regime of Eq. (3) beyond the first degenerate manifold. However, our choice of $\hat{O} = \sum_i (1 - \hat{n}_i)$ is more robust to experimental imperfections. As a concrete example, consider a shot in which the system ends in the many-body ground state but a single atom spontaneously decays to the electronic ground state during readout. This event reduces the fidelity from 1 to 0, producing a large spurious signal. By contrast, the same decay increases δn by 1 regardless of whether the system was in the ground or an excited state, so it constitutes common-mode noise that cancels in the difference $\delta n = \langle \hat{n}_g \rangle_{\text{modulated}} - \langle \hat{n}_g \rangle_{\text{ground state}}$. This common-mode robustness makes δn the preferred observable in practice.

To emphasize the advantage of this particular choice of $\hat{O}$, we made the following revision to the text: "To measure the excited state spectrum, we should choose $\hat{O}$ such that O_{gg} is distinct from all excited states' O_{ee}. Although measuring the ground-state infidelity would satisfy this criterion for all excited states, we choose $\hat{O}' = \sum_i (1 - \hat{n}_i)$ for its robustness against noise, including common-mode atom loss."

'line 1280: this statement is unclear, as any pure state can be written as a superposition of eigenstates. Why does the next sentence follow?'

We thank the referee for pointing out this ambiguity. The key distinction from the modulation-ramp-probe sequence is that the observable $\hat{Q}$ is not diagonal in the eigenbasis of $\hat{H}$. After a weak modulation of amplitude A, the state acquires an $O(A)$ admixture of excited states: $|\psi\rangle = |g\rangle + O(A)|e\rangle + O(A^2)$. For a diagonal observable $\hat{O}$ (as in the modulation-ramp-probe sequence), only the populations $|\langle e|\psi\rangle|^2 = O(A^2)$ contribute, giving a quadratic response. For a non-diagonal observable $\hat{Q}$, however, the cross terms $\langle g|\hat{Q}|e\rangle\langle e|\psi\rangle = O(A)$ survive, producing a response that is linear in A. We have revised the manuscript to make this reasoning explicit. It now reads: "Assuming we prepare the ground state, the modulation creates an $O(A)$ admixture of excited states. Since $\hat{Q}$ is not diagonal in the eigenbasis of $\hat{H}$, the change $\delta\langle\hat{Q}\rangle$ receives contributions from the off-diagonal matrix elements $K_{ge}Q_{eg}$ already at first order in A—in contrast to the modulation-ramp-probe sequence, where the diagonal observable $\hat{O}$ yields a response that is quadratic in A."

'line 1284: why is a Gibbs state assumed?'

Here we assume a Gibbs state in order to treat the general case of imperfect state preparation, in which the system may be left at a small but finite temperature. Given only the mean energy, the Gibbs (maximum-entropy) state is the least-biased description consistent with that constraint, so adopting it as our starting point keeps the derivation broadly applicable rather than tied to a single idealized state. From this assumption we derive the relation between the modulation signal and the underlying field theory. The choice is further motivated on physical grounds: for generic non-integrable many-body systems, the eigenstate thermalization hypothesis (ETH) predicts that local observables relax to values described by a Gibbs ensemble, and at criticality the finite-temperature Gibbs state gives rise to universal scaling functions such as the DSF scaling form. While we do not expect our adiabatic state preparation to produce a true Gibbs state, adopting it ensures the derivation remains general. Finally, in the zero-temperature limit the Gibbs state reduces to the many-body ground state, which is the regime physically relevant to our experiment.

'line 1435: "irrelevant" is presumably meant in the RG sense; how does this apply in a finite system?'

Here "irrelevant" is meant in the RG sense. The Rydberg Hamiltonian tuned to criticality realizes the Ising CFT up to irrelevant contributions [Phys. Rev. B 104, 235109]. For a finite system, coarse graining terminates at the scale set by L, so irrelevant microscopic perturbations do not disappear entirely. Instead, they contribute sub-leading finite-size corrections to the CFT spectrum which are expected to decrease with increasing L. The measured spectrum should therefore approach the universal fixed-point spectrum as system size increases.

'line 1934: why must K and Q be sums of local operators?'

The modulation operator and measurement operator need not to be sum of local operators. However, experimentally, both the global and the local detuning modulation is represented as a sum of local operators. Theoretically, the lattice counterparts of the primary CFT fields are local operators as well. Therefore, it is of interest to derive the response for modulating and measuring local operators. Yet, it remains an interesting open question on the response of long-range correlators, for example, in which case $\hat{Q}$ is not a local operator.

'line 2031: fit results for frequencies and decay times, including uncertainties, are missing.'

The fit frequencies and their uncertainties are provided in Extended Data Fig. 6d, together with a comparison to the numerically calculated eigenstate energies. Regarding the decay times, we acknowledge that the oscillations decay within less than one cycle, making a reliable extraction of the lowest excitation energy E_1 from the fit challenging. We have accordingly softened the language in the revised manuscript, framing this measurement as a proof-of-principle qualitative demonstration of quench dynamics rather than a quantitative extraction of the energy spectrum. We restructure the paragraph and now it reads "Here we prepare a non-equilibrium low-energy TCI state by adiabatically ramping our system close to the TCI point and then quenching the detuning to Δ_c (Ext. Data Fig. 6a). To understand the expected dynamics, we first numerically simulate the quench for different local detuning strengths η. For all $\eta \in [0, 1]$, we observe long-lived oscillations (Ext. Data Fig. 6c), with the fitted frequencies consistent with the first excited state energy E_1 (yellow data points in Ext. Data Fig. 6d), suggesting an oscillation between the ground state and the first excited state of the TCI CFT. Experimentally, after holding at the TCI point for varying durations, we measure the sum of local fields, $\sum_i \sigma_i$, and observe damped oscillations (Ext. Data Fig. 6b). We record the oscillations at boundary detunings $\eta = 0.5, 0.75$ and fit the data to a trial function $a + b\cos(\omega t + \phi)e^{-\frac{t}{\tau}}$; the fit frequencies are compared with the numerically obtained E_1 in Ext. Data Fig. 6d, where we see qualitative agreement. Due to the fast decay of the experimental oscillations, we regard the observed oscillation as a proof-of-principle qualitative demonstration of quench dynamics near the TCI point."

'Question to the authors:'

'In Cardy's discussion of boundary CFTs, boundary conditions correspond to RG fixed points that emerge under coarse graining. In the present work, one boundary condition appears to arise naturally, while another is enforced microscopically through engineered detunings. In a finite system, where RG flow is not fully developed, is there a meaningful conceptual distinction between these two realizations of boundary conditions?'

The TCI CFT hosts more primary fields than the Ising CFT, leading to a richer boundary fixed-point landscape, with stable free and fixed boundary conditions separated by an unstable intermediate fixed point. In our setup, the energetics of the unmodified Rydberg chain at tricriticality naturally place the system in the RG "basin of attraction" of the stable free boundary condition, as indicated by the suppressed edge CDW order and the extracted spectral ratios.

By contrast, landing in the basin of attraction of the fixed boundary condition for TCI requires sufficiently strong boundary detuning that induces edge CDW order. The difference in realizing the two fixed points is merely in the microscopic boundary perturbation required to reach the appropriate basin of attraction.

Finite system sizes also plays an important role. Even when a system is prepared within a given basin of attraction, the limited amount of coarse graining available in a finite chain prevents the RG flow from approaching the corresponding fixed point arbitrarily closely, as it would in the thermodynamic limit. Instead, residual perturbations away from the fixed-point theory continue to contribute, with their effects becoming progressively weaker as the system size increases.

'Beyond these detailed comments, my main suggestions for improving the paper are: (i) to more cautiously calibrate the strengths of claims throughout the manuscript, in particular with regard to the TCI and the DSF, or alternatively provide stronger corresponding experimental evidence; and (ii) more clearly separate experimental results from numerical results and numerically supported interpretations.'

'In summary, this is an impressive and important piece of work that introduces a powerful experimental technique and provides compelling results, especially in the Ising regime. With a more careful presentation, I would be supportive of publication in Nature.'

We sincerely thank the referee for the positive assessment of our work and for the thoughtful and constructive suggestions. We have carefully addressed both points:

1. We have revised the manuscript to calibrate the strength of our claims more carefully, particularly regarding the TCI and DSF results. Where the evidence is primarily qualitative or relies on numerical support, we have softened the language accordingly, including framing the quench dynamics as a proof-of-principle demonstration and the DSF measurement as a first indication of access to the dynamical scaling regime.
2. We have gone through the manuscript to clearly distinguish between direct experimental observations, numerically supported interpretations, and theoretical predictions, ensuring that the reader can readily identify the nature of the evidence underlying each claim.

We believe the revised manuscript addresses the referee's concerns while preserving the core conclusions of the work, and we hope the referee finds the presentation improved.

RESPONSE TO REVIEWER 2

'The manuscript reports on the experimental observation of conformal field theory (CFT) spectra. Many important theoretical results, covering a broad range of areas of physics, have been obtained over the years from the powerful tools of CFT since its inception more than 40 years ago. Yet, in the past, only some aspects of select theoretical CFT results have been probed directly in experiment. In particular, the large amount of exact and universal information contained in finite-size spectra of CFTs has never been experimentally accessed. The present manuscript is the first to accomplish this. This is a remarkable, highly original, and very significant achievement of this work. The method the authors use for this purpose is 'modulation spectroscopy' which operates in the time-domain. This method, as well as the results obtained using it, are both clearly explained in the manuscript (including the methods section). Specifically, the manuscript reports results for finite-size spectra of a Rydberg chain tuned to its quantum phase transition in the critical and tricritical Ising universality class, for various boundary conditions. Those experimentally obtained spectra are shown to agree excellently with exact analytical results known for these two critical points. The authors also measured responses at the critical point, allowing them to extract the dynamical structure factor determined by CFT correlation functions.'

'The manuscript provides explicit and understandable summaries of theoretical background on the properties of the considered CFTs, so that non-expert readers can directly appreciate the relationship with the experimentally obtained results. Abstract and introduction provide a clear presentation of the context in which this work is situated, conceptually and historically. The conclusion section provides perspectives of the expected impact of this paper on future questions, involving the use of programmable quantum simulators.'

'In view of these comments, I strongly recommend publication of the manuscript in Nature.'

We thank the referee for appreciating our work.

RESPONSE TO REVIEWER 3

'This paper presents an experimental observation of the spectra and other universal features of various conformal field theories (CFT) realizable in a Rydberg atom quantum simulator, such as the Ising CFT and the tricritical Ising CFT. The authors present a protocol they call the modulation-ramp-probe sequence: prepare the ground state (near the critical point), apply a modulation to couple to excited states with given modulation frequency, ramp deep into the disordered or ordered phases, then image the population transferred onto excited states which deep in the phases correspond to density fluctuations. They demonstrate that one can observe signatures of the CFT spectra, in particular, their universal energy ratios, while performing finite size scaling; they also show how to probe different symmetry resolved sectors and observe the effect of different boundary conditions.'

'Overall, the science demonstrated and methods implemented are impressive — it is nice that one can directly experimentally image some of the predictions from CFT, especially their excitation spectra, which as far as I understand, have remained largely theoretical. Thus I am appreciative on this front.'

'That said, one criticism I have is that the observation relies heavily on known understanding and numerical studies of the effective model realised in the experimental system. For example, the phase diagram of Fig 1 is a numerically obtained figure, from which one determines the critical detuning to stop the experimental adiabatic state preparation at. Without such prior knowledge, would one be able to reliably and robustly prepare (an approximation) of the critical ground state?'

In principle, prior numerical knowledge of the phase diagram is not essential for preparing an approximate critical ground state, as the critical point can be located experimentally, using the same system and techniques developed in this work. A quantum critical point leaves several robust, model-independent finite-size signatures that can be measured directly as the accessible Hamiltonian parameters are swept, without reference to any numerically computed phase diagram.

Concretely, one could locate the transition through any of the following experimentally accessible probes:

First, the excitation gap. Using the modulation spectroscopy developed here, one can track the lowest excitation energy as a function of detuning; in the thermodynamic limit, the critical point appears as a pronounced gap closing. One could extrapolate the gap-closing detuning from performing spectroscopy in different system sizes. This requires no prior knowledge of the critical theory.

Second, the order parameter and its fluctuations. A continuous transition manifests as a rapid crossover of an appropriate order parameter (here, the charge-density-wave order) accompanied by a peak in its susceptibility/variance, with the crossing point of curves taken at different system sizes converging to the critical point, which is a standard finite-size-scaling diagnostic that we employed numerically in this work to determine the critical detuning, which can also be done experimentally (Methods, Ext. Data Fig. 2).

Third, correlation-based signatures, such as the growth of the correlation length or the scale-invariant decay of correlation functions at criticality, which can be extracted from the site-resolved snapshots native to the platform.

Importantly, these procedures are self-contained: they use only experimentally measured quantities and the assumption of an underlying continuous transition, so they would allow one to robustly prepare an approximation of the critical ground state and map the phase diagram, even for a model whose critical properties are not known a priori. This is precisely the regime in which our technique is most valuable, and we view the ability to diagnose criticality directly from experiment as one of the broader strengths of the approach. We note that the numerically obtained phase diagram in Fig. 1 is used in the present work for convenience and to enable quantitative benchmarking against exact predictions, rather than out of necessity.

'Secondly, the modulation spectroscopy appears to have limitations — for example, it cannot resolve the degeneracies in the CFT spectra. Can the authors comment on how this may be overcome?'

We agree that a modulation spectrum obtained from a single modulation operator cannot, by itself, distinguish states that are exactly degenerate in energy. More generally, resolving individual states within a degenerate manifold requires access to a set of perturbing operators $\{\hat{O}_\mu\}$ whose transition matrix elements provide independent information within that subspace. In practice, one needs a collection of operators whose transition strengths to the different degenerate states are not all identical. That is, the quantities, $|\langle e|\hat{O}_\mu|g\rangle|^2$, should depend on both the state e and the operator label μ, so that measurements with different perturbations provide enough information to distinguish the states within the degenerate manifold.

We make theoretical progress in this direction for the Ising critical point by considering spatially structured detuning modulations, $\hat{K}_k = \sum_j \cos(kj + \alpha)\hat{n}_j$, and measuring the response as a function of wavevector k. In the free-fermion description of the Ising CFT, different two-fermion states within degenerate energy manifold can be distinguished through their wavevector-dependent transition matrix elements, $|\langle e|K_k|g\rangle|^2$ (see ED Fig. 4). Whether analogous methods can be employed to resolve degenerate manifolds in more general interacting or unknown CFTs is an interesting open question.

We have added a comment to the "Dependence of spectroscopy signal on modulation wavevector" section of Methods addressing the point that degeneracies can be resolved with k-dependent modulation spectroscopy in the Ising case:

"The small-k peak structure in Ext. Data Fig. 4b–e also reveals degeneracies in the Ising CFT spectrum. Although distinct two-fermion states with the same $k_a + k_b$ are degenerate in energy, their transition strengths under $\hat{K}_k$ depend on $|k_a - k_b|$. Scanning the modulation wavevector therefore provides a way to distinguish states within a degenerate manifold."

'Third, the choice of global detuning to use as the probe operator is, I can understand, the easiest modulation to perform experimentally. But is there a good understanding of how robust or universal such a modulation would be couple to excited states in the CFT spectrum for more generic CFTs? What if there were some reason the operator does not couple to a given excited state, or has very weak overlap? Then the spectroscopy might fail. Of course, for the cases studied in this paper, the modulation seems to work just fine, but "just fine" entails a double checking and confirmation with numerics to declare post facto success.'

We thank the referee for raising this point, and we would like to clarify it, because we believe our results already address the central concern directly and without recourse to numerics.

First, the question of whether the global-detuning modulation actually couples to the CFT excited states is answered unambiguously by the data themselves. We observe a clear series of spectral peaks whose positions follow the analytically predicted universal energy ratios (2:4:6:8 for the even-parity Ising sector, 3:5:7 for the odd sector), and whose energies collapse as $1/L$ across system sizes exactly as required for a CFT. These ratios are fixed analytically by the conformal field theory, rather than fitted or numerically tuned, so their experimental observation directly demonstrates that the modulation couples to the relevant excited states. In other words, the coupling is not something we infer post facto from a numerical comparison; it is manifest in the measured spectrum.

The role of numerics in our work is therefore one of cross-checking and quantitative benchmarking of the specific microscopic Rydberg Hamiltonian (for example, transition matrix elements and exact finite-size energies), not of establishing whether the spectroscopy "works." The universal content of the result, including equally spaced, $1/L$-scaling peaks at analytically known ratios, stands on its own.

That said, the referee raises a legitimate general point: for an arbitrary, a priori unknown CFT, a particular probe operator could in principle have weak or vanishing overlap with a given excited state (for instance, by symmetry, or because it couples only through an irrelevant operator), in which case that state would be suppressed or absent from the spectrum. This is a generic feature of any spectroscopy. The natural and robust strategy in that setting — which our platform readily supports — is to probe with a basis of experimentally accessible operators resolved in both frequency ω and momentum k (as we already demonstrate with global and local detuning modulations to access distinct symmetry sectors). Combining several complementary responses ensures that the full operator content is captured and provides a robust identification of the underlying CFT even when its structure is not known in advance.

'Thus my main question is really how generic and how universal the methods employed in this paper are, especially in light of the need to have prior knowledge (theoretically and numerically) of the CFT spectra to be probed. In which case the question of probing the CFT spectra becomes more like a post facto verification as opposed to an a priori spectroscopy of unknown spectra. Notwithstanding the above comments, I want to stress that I am still very appreciative of the involved and impressive experimental techniques and high quality data presented, as well as the excellent manuscript, so I am still favourable of publication if my questions are well addressed.'

We thank the referee for this incisive question, which gets to the heart of what our technique offers, and we have sharpened our discussion of it in two respects.

First, and most importantly, the dynamical-response measurement presented in the final part of the paper is a fully general method that is independent of any CFT or prior knowledge. It directly measures the dynamical structure factor $S(k, \omega)$, the system's frequency- and momentum-resolved response to a modulation of an experimentally accessible operator — which is a well-defined physical quantity for any Hamiltonian. This is genuine a priori spectroscopy: it requires no input about the underlying theory, no prior location of a critical point, and no numerical benchmark to be meaningful. At a critical point it encodes the universal scaling function of the relevant operator (as we demonstrate for the Ising case), but the measurement itself is agnostic to that interpretation and applies equally in any setting — including regimes such as two-dimensional quantum critical points where classical simulation faces sign problems or entanglement barriers, and where this kind of model-independent dynamical probe is most valuable.

Second, regarding the peak-resolved modulation spectroscopy used in the first part of the paper: we agree with the referee that its generic application is more subtle, and we do not wish to overstate it. Even there, however, the measured excitation energies, their $1/L$ scaling, their dependence on boundary conditions, and the selection rules governing which operators couple to which states, are obtained directly from the physical system without assuming an effective theory; the universal energy ratios we observe are fixed analytically by the CFT and are not numerically tuned. In the present work numerics serve two roles: (i) to locate the critical point, which can in principle be replaced by experimental diagnostics as discussed above, and (ii) to benchmark the measured spectra against known predictions, which demonstrates that the technique works but is not a prerequisite for its use. In a genuinely unknown setting, this operator-resolved spectroscopy would yield primary experimental data, including energy ratios, scaling, and operator content, that could be used to constrain or identify the universality class.

We thank the referee again for their appreciation of the work, and hope that these clarifications, together with the revisions, fully address their questions.