**General remarks on the contribution of the paper.** The main conceptual contribution of our paper is to establish a geometric characterization of spins that appear in an integrable spin chain in the following schematic way:

$$\begin{tikzcd}
\{\text{Bow varieties}\} \arrow[rr, "\text{quantization}"] \arrow[dr, leftrightarrow, "\text{phase spaces}"'] & & \{\text{Spins in an integrable spin chain}\} \\
& \{\text{Line operators in 4d CS}\} \arrow[ur] &
\end{tikzcd} \tag{0.1}$$

To argue for this we establish a correspondence between Bow varieties and line operators in 4d Chern-Simons theory, and use the already known correspondence between these line operators and spin chains [1]. We also show that Bow varieties, and not just Nakajima quiver varieties, are natural objects for studying geometric representations of Yangians. We say Yangian because of the following reason. The interpretation of bow varieties as phase space of line operators in 4d CS says that their quantization gives rise to Hilbert spaces of integrable spin chains and therefore should naturally be representations of associative algebras that can be defined by Yang-Baxter relations.

At a computational level, we show that $\Omega$-deformed Kapustin-Witten theory is equivalent (at the level of BRST cohomology) to 2d BF theory. To clarify, this result is neither unexpected nor surprising. One can roughly arrive at this result in the following way: take the holomorphic-topological twist of 6d $\mathcal{N} = (1, 1)$ SYM on $\mathbb{R}^4 \times \mathbb{C}$ from [2]. They show that turning on $\Omega$-deformation along $\mathbb{R}^2$ localizes the theory to 4d CS on $\mathbb{R}^2 \times \mathbb{C}$. Dimensional reduction of the 6d holomorphic-topological theory on $\mathbb{C}$ is the Kapustin-Witten theory, and dimensional reduction of 4d CS on $\mathbb{C}$ is 2d BF. Assuming that dimensional reduction commutes with $\Omega$-deformation we reach the 4d$\rightarrow$2d claim made earlier. In this paper however, we give a much more direct and clear derivation of this, which we believe should make the result more reliable.

We also show, in standard physical language, that bow varieties correspond to vacuum branches of 3d $\mathcal{N} = 4$ theories defined by Hanany-Witten type brane configurations, with or without good quiver descriptions. The authors of [3] proved that Coulomb branches of A-type quiver theories are isomorphic to some bow varieties from a purely mathematical perspective. In our paper we show that the equations defining a bow variety arise as vacuum equations in 3d $\mathcal{N} = 4$ theories defined by some brane configuration. While our appraoch lacks the mathematical rigor of the aforementioned reference, we believe it is well within the physics standard, novel (to the best of our knowledge), and accessible to a physics audience. Note that the authors of [4] also use the language of branes to define a bow variety, but they are simply using the fact that the same combinatorial data can be used to describe both the brane configurations and the bow varieties without using any QFT. Our analysis of the vacuum equations coming from the D-brane theories provides a physical justification for this equivalence.

Lastly, to give some context, we are exploring the consequences of the brane construction of

4d CS theory. We believe that this approach can provide very elementary explanations of various integrable structures in field theories and in this paper we are looking at some particular aspects of this program relating to 3d $\mathcal{N} = 4$.

**Addressing more specific questions.**

1. Is 4d CS really crucial for the connection to bow varieties? Seems like not really. Correct me if I'm wrong. The bow variety describes the Higgs branch of a 3d N=4 theory arising from the brane construction of a codim-3 defect in the 6d (1,1) SYM. Then the fact the upon Omega-deformation, this 6d-3d SUSY system reduces to a 4d-1d coupled system (4d CS with a line defect), is a completely independent fact. Correct?

   Yes, the Higgs branch of the 3d theory coming from the brane construction being a bow variety is completely independent of the 4d CS theory. However, relating the result to 4d CS theory makes a simple conceptual connection to integrable systems.

2. If (1) is true, then can you identify which part of the construction is really novel? Also, what is the connection to reference [54], and can one do anything like that to the general line operators you build? Maybe the 4d CS provides some unique perspective on these lines that goes beyond the brane description of a 3d N=4 theory? If that is the case, it would be interesting and new.

   For comments on novelty, please see our earlier response. Regarding relation to [5] ([54] in the paper), we give a worldvolume perspective on the results of [5, §9] relating Coulomb branches of A-type quiver theories to phase spaces of line operators. The reference further computes the matrix representations of the Q-operators (corresponding to expectation value of crossing line operators in 4d CS) and derives TQ relations as OPE of line operators. We are working on extensions of some of these results to include more general line operators considered in our paper.

Addressing remaining questions, suggestions, concerns, and typos.

1. The paper starts by talking about phase spaces of line operators, but never defines what it is. While the phase space of the entire system is a familiar object, it would help to say a word or two on what is meant by the phase space of a line operator (otherwise a reader has to read the paper first to extract the definition).

   By phase space of line operators we mean the phase space of the quantum mechanical system supported on the line that creates the line operator by coupling to the 4d CS theory, we shall add this in the introduction.

2. Top of page 3: "This fits nicely with the fact that the line operators in 4d CS theory form integrable spin chains which carry natural Yangian actions" – it is, actually, not entirely clear what fits with this fact and why.

   By "This" here we are referring to the result that coulomb branch algebras are given by shifted truncated Yangians. We say that this fits with the results of the paper is simply because we expect the deformation quantization of the phase spaces of line operators in 4d CS to produce algebras with RTT presentations, such as Yangians.

3. Page 3, second paragraph, the reference to [20] and the statement that those authors construct L operators via monodromy lines: monodromy lines in which theory? Then there's a sentence mentioning monodromy lines for the Q operators and lines for the T operators: again, what sort of lines are these? In which theory? Authors of [20] most definitely didn't work with the 4d CS, hence it is not clear what's the context for lines mentioned in connection to that reference.

Here by lines we are not referring to line operators of any kind in any quantum field theory. In an integrable spin chain with Hilbert space $\mathcal{H} := V_1 \otimes \cdots \otimes V_n$ one introduces a monodromy matrix $T : U \otimes \mathcal{H} \to U \otimes \mathcal{H}$ by taking a product of R-matrices $T = R_{UV_1} \otimes \otimes \cdots R_{UV_n}$ where $R_{UV} : U \otimes V \to U \otimes V$. The transfer matrix is then defined as the trace over the auxiliary space $U$ as in $t = \mathrm{tr}_U(T) : \mathcal{H} \to \mathcal{H}$. The vector spaces $V_i$ are often graphically depicted as vertical lines and the auxiliary space $U$ as a horizontal line. In this graphical depiction R-matrices are represented as crossings between vertical and horizontal lines and RTT relations are also depicted graphically. For example, see [6, Fig. 2-5]. These were the lines we were referring to. Note that in the correspondence between spin chains and 4d CS theory these pictures can meaningfully be thought of as line operators but we did not mean to say that [7] ([20] in the paper) referred to any line operators at any point.

4. Last paragraph of the introduction, second sentence: "the" is repeated twice.

Corrected.

5. Last paragraph of the introduction, last sentence: "...algebras related to the T, Q, and L-operators..." – what is meant by an algebra related to the T/Q/L-operator?

Let $T : U \otimes \mathcal{H} \to U \otimes \mathcal{H}$ be a monodromy matrix in some spin chain with Hilbert space $\mathcal{H}$ and $U = \mathbb{C}^n$ is some auxiliary space. Then we can write $T$ as a matrix:

$$T = \begin{pmatrix} T_{11} & \cdots & T_{1n} \\ \vdots & \ddots & \vdots \\ T_{n1} & \cdots & T_{nn} \end{pmatrix} \tag{0.2}$$

where $T_{ij} : \mathcal{H} \to \mathcal{H}$ are some operators that act on the spin chain. Given the R-matrix of the spin chain and using the RTT relations we can find some relations that these operators $T_{ij}$ have to satisfy. Assuming that this monodromy matrix corresponds to some T/Q/L operator we are referring to the algebra of $T_{ij}$ as the algebra related to said operator. This is the same sense in which the algebra defined by [7, eq. 2.14] is related to the L-operator of [7, eq. 2.12].

6. After (2.1): $\vec{\omega} \cdot \mathrm{d}\vec{x}$ is a one-form, hence in $\mathrm{d}U = \star_{\mathbb{R}^3}(\vec{\omega} \cdot \mathrm{d}x)$ the form degrees do not match. In fact, there should be one more d on the right, applied before taking $\star$.

Corrected.

7. Second paragraph on pg.5: I think the statement that Omega-deformed SYM "reduces" to the 4d CS is really at the level of Q-cohomology, or "protected sector". Just a clarification.

True, clarification added.

8. Footnote 2: TQM has zero Hamiltonian, therefore, saying "no additional potential term is present in the Hamiltonian" is misleading. There is simply no Hamiltonian in the first order action, only the symplectic term.

Corrected.

9. Caption of Figure 1 says that complex FI parameters are not visible in the classical brane picture – this seems misleading. There is nothing "quantum" about such FI parameters, it is simply hard to depict them on a planar brane diagram, but they have clear geometric meaning.

We meant that our FI parameters are of the order of $\hbar$. Of course, FI parameters can be interpreted geometrically as separations of certain branes. By "quantum" we meant that these separations are too small compared to all the other separations that appear in our brane diagrams. If the separations defining the FI parameters were of order $\mathcal{O}(1)$ we would get multiple distinct line operators instead of a single one.

10. After (2.3), about dropping the stability condition from the notation: can there be different, inequivalent stability conditions?

Yes. But for generic values of real FI parameters, the stability conditions are determined unambiguously [8, 9]. The space of real FI parameters is divided into chambers and each chamber corresponds to one stability condition. In the paper we simply assume that the real FI parameters are generic and the appropriate stability conditions are (implicitly) chosen accordingly.

11. Page 7: how reliable is the anaysis that uses 6d (1,1) SYM, given that it's an IR-free theory?

We are assuming that the brane constructions provide the right UV completions and that our world-volume theories capture the proper degrees of freedom. Some discussion on this issue can be found on [5, §9, p. 35].

12. About (2.6): why does it have to be a projection to $F_{\mathbb{C}}$ (which seems to require a choice), rather than the canonically available injection $F_{\mathbb{C}} \to \mathrm{GL}_n$? On the one hand, I indeed could project the $\mathfrak{gl}_n$ valued bulk gauge field onto $f_{\mathbb{C}}$ to define the defect coupling. On the other, I could consider injecting the $f_{\mathbb{C}}$ valued current on the defect into the $\mathfrak{gl}_n$-valued current in the bulk. What is the reason to prefer the former?

We think that they should result in the same coupling at the end. A coupling between the 4d CS and the TQM is given by $\int_{\mathbb{R}} \mathrm{d}t \, \mathrm{tr}(\mathcal{A}\mu_{\mathcal{M}_H})$ where $\mathcal{A}$ is the $\mathfrak{gl}_n$ gauge field of 4d CS and $\mu_{\mathcal{M}_H} : \mathcal{M}_H \to \mathfrak{f}_{\mathbb{C}}$ is the (dual of the) moment map for the $F_{\mathbb{C}}$ action on $\mathcal{M}_H$. A modified version of the coupling is $\int_{\mathbb{R}} \mathrm{d}t \, \mathrm{tr}(\mathrm{Ad}_g \mathcal{A}\mu_{\mathcal{M}_H})$ for some constant $\mathrm{GL}_n$ element $g$. Here we are applying the function $\mathrm{tr}(\mathrm{Ad}_g\mathcal{A}–)$ to $\mu_{\mathcal{M}_H}$. This function is the image of $\mathrm{tr}(\mathcal{A}–) \in \mathfrak{g}^*$ under $\mathfrak{gl}_n^* \xrightarrow{\mathrm{Ad}_g} \mathfrak{gl}_n^* \xrightarrow{\text{restrict}} \mathfrak{f}_{\mathbb{C}}^*$. Alternatively we can use the map $\mathfrak{f}_{\mathbb{C}} \hookrightarrow \mathfrak{gl}_n \xrightarrow{\mathrm{Ad}_g} \mathfrak{gl}_n$ to send $\mu_{\mathcal{M}_H}$ to $\mathrm{Ad}_g\mu_{\mathcal{M}_H}$ and then act on it with $\mathrm{tr}(\mathcal{A}–)$, the result is the same. Nevertheless, our present comment in the paper about the coupling is confusing, we shall simplify it by simply mentioning the coupling term.

13. The definition of phase space in (2.7) as a Higgs branch depends crucially on the theory $T^\vee$. Is this notion invariant at all? What if the defect (as a 4d-1d coupled system) has a dual description, with a different theory $T^\vee$, whose Higgs branch is different (but somehow, the defect becomes the same after coupling $T^\vee$ to the bulk)? The definition (2.7) doesn't make it manifest that the phase space defined as the Higgs branch is an invariant. Is there a better definition of the line operator phase space, which makes it manifestly intrinsic and independent of the concrete realization?

We don't have a good answer to this. We are going to investigate further.

14. On the sentence right after the eqn. (2.7): the TQM itself already quantizes $M_H$, one does not need CS. The TQM-CS dynamics seems to further gauge some isometries of $M_H$. Hence a question: does the $\mathcal{A}_\varrho$ from (2.8) just quantize $M_H$, or does it also include the effects of gauging (some kind of quantum Hamiltonian reduction)?

Right, the TQM alone quantizes $\mathcal{M}_H$ and produces $\mathcal{A}_{\boldsymbol{\varrho}}$, language corrected. Further gauge invariant operators in the coupled system on the line would be given by quantum Hamiltonian reduction of $\mathcal{A}_{\boldsymbol{\varrho}}$.

15. Equation (2.9) has $v_j$ without tilde, while in the Figure 3, v's appear with tildes. Seems like a typo?

Yes, corrected.

16. Between (2.11) and (2.12): "...SU(2) symmetry rotating the complex symplectic structures..." – it rotates the complex and structures and it rotates the symplectic structures, but did you really mean to say that it rotates the complex symplectic structures? (well, it does, but maybe you didn't mean precisely that?)

We meant complex *and* symplectic. Corrected.

17. Before eqn. (2.12): symplectic reduction, subject to the stability condition.

Yes, clarification added.

18. Strictly speaking, in (3.2) one has Spin groups.

Yes, added a footnote clarifying that the fermions transform under spin representations.

19. Right after (3.7): "...the associated current must vanish at that boundary." – not the whole current, only its normal component.

Corrected.

20. Section 3.2 starts with an unmotivated assumption that the correct twist must belong to the KW family. Why? The 4d N=4 SYM has a few other inequivalent twists, – why is it only the KW twist that you study?

KW twist follows from analyzing supersymmetry. We use a particular supercharge in IIB for the supersymmetric twist. It has to be the supercharge that induces the holomorphic-topological twist of 6d $\mathcal{N} = (1,1)$ SYM [2, §2.2] since we want it to localize to 4d CS. When seen from the point of view of the D3 branes, it's one of the supercharges that induces the KW twists. We shall add a brief explanation of this in the paper.

21. Right before Section 4: the statement that one obtains 2d BF theory seems umotivated at this point. Need some justification/references.

At this point it was not meant as an evidently true statement, rather a description of what we do next in §4. Added a few words to make it clear.

22. The upper indices on dx's in (4.1) should probably be 0,1,2,3?

Yes, corrected.

23. Remark on the style: between eqns (4.21) and (4.22), it is best to avoid too many "will". Better to say: "We integrate out..... after which we are lead to..."

Changes made.

24. Typo: after (4.47), there should be no "is" after the word "derivative".

Corrected.

25. Page 22: "The actual value of m and t are coordinate dependent and physically irrelevant..." – the statement that physical couplings are physically irrelevant (which, by the way, are relevant couplings in the techincal sense) seems a bit problematic. Please make this statement more precise, reflecting what you really mean.

We meant that differences in positions of the branes, namely $m_i - m_j$ and $t_i - t_j$, are certainly physical, but when all the branes are coincident in these directions, we can take them to be located at the origin of the coordinate system without loosing generality. The term irrelevant is indeed problematic as it has a separate technical meaning, we shall correct the language.

26. In the end of paragraph before (4.87), the representation spaces R and R' appear out of nowhere, without any definition. Please clarify. Also, remove doubled "the" in the last sentence before (4.87).

Corrected.

27. Between (5.1) and (5.2): what does it mean for an algebra to "couple" to a line operator?

We meant that this is the algebra of the TQM, which in turn couples to the 4d CS as a line defect.

28. Page 33, before section 5.1. "monodromy matrices associated with the algebras U, Weyl, ..." – what does it mean to associate the monodromy matrix with an algebra?

Please see our response to question 5.

29. Proposition 5.1: "where the Dynkin labels of lambda and rho" should be separated by commas.

Rephrased.

30. Next paragraph "values of the casimirs" is missing "of".

Corrected.

31. Paragraph before (5.5): "If we turn on all complex twisted masses and treat them as undetermined parameters, then the Coulomb branch is $\mathfrak{gl}_n^*$" – provide a reference for this statement.

Follows from [10, Theorem 2.11] after applying [11, Theorem 7.6.1], added in paper.

32. Statement about the quantum Hamiltonian reduction before (5.6): please also provide a reference.

We are using the definition of the complex Higgs branch as the Hamiltonian reduction of the space of hypers by the complexified gauge group, referring to the quiver in Fig. 12. The bifundamental hypers between U($k$) and U($k+1$) generate they Weyl algebra $\mathrm{Weyl}_\hbar^{\otimes k(k+1)}$. We are putting together all these Weyl algebras, adjoinnig the complex FI parameters as formal variables, and reducing by the complexified gauge Lie algebra $\oplus_{k=1}^{n-1}\mathfrak{gl}_k$. If we should cite something here please let us know.

33. Paragraph before Remark 5.2: "It is known that ... is a normal variety of dimension $n^2$, and ... is the ring of function on a ..." – if it is known, please include the appropriate reference.

Added the footnote: Let $\mathfrak{h}$ be the Cartan subalgebra of the Lie algebra $\mathfrak{g}$ of a reductive Lie group $G$, then the quotient $\mathfrak{g}^*/G$ is isomorphic to $\mathfrak{h}^*/W$ where $W$ is the Weyl group of $G$, and moreover there exists an open subset $\mathfrak{g}^*_{\mathrm{reg}} \subset \mathfrak{g}^*$ such that its complement in $\mathfrak{g}^*$ has codimension 2 and that the natural map $\mathfrak{g}^*_{\mathrm{reg}} \to \mathfrak{h}^*/W$ is smooth, see [12, Section 3.1]. Then it follows from the aforementioned facts that $\mathfrak{g}^* \times_{\mathfrak{h}^*/W} \mathfrak{h}^*$ is Cohen-Macaulay, and it contains an smooth open subset $\mathfrak{g}^*_{\mathrm{reg}} \times_{\mathfrak{h}^*/W} \mathfrak{h}^*$ whose complement in $\mathfrak{g}^* \times_{\mathfrak{h}^*/W} \mathfrak{h}^*$ has codimension 2, thus $\mathfrak{g}^* \times_{\mathfrak{h}^*/W} \mathfrak{h}^*$ is normal [13, Theorem 39]. Since the projection $\mathfrak{g}^* \times_{\mathfrak{h}^*/W} \mathfrak{h}^* \to \mathfrak{g}^*$ is finite and surjective, we see that $\dim \mathfrak{g}^* \times_{\mathfrak{h}^*/W} \mathfrak{h}^* = \dim \mathfrak{g}^*$.

34. Just before (5.17): ".. acts on the Verma module with the highest weight..." – where does the Verma module come from? It appeared unmotivated, out of nowhere, basically.

In general it does not have to be a Verma module. We simply compute the values of Casimirs. As we mention right after Proposition 5.1, it's a Verma module if we specialize to highest weight type. This specialization plays no significant role, just seems to make some of the writings simpler.

35. End of Section 5.1, right before Section 5.2: "...quantized Coulomb branch algebras of 3d N=4 theories act on Verma modules;..." – this statement is a tautology. Indeed, any algebra is going to act on its Verma modules, if there are any. Please clarify what you actually meant to say.

Right, we wanted to point out that the reference constructs an action of the Coulomb branch algebra on its Verma modules in terms of monopole operators acting on vortices. Language corrected.

36. Page 37, last paragraph: "Phases spaces" should be "Phase spaces".

Corrected.

37. Remark 5.4: "The operators $Q_k$ in our paper is denoted" should be "are denoted". Also in "other Q operators $Q_I$ with $|I| = k$ is obtained" – should be "are obtained".

Corrected.

38. Right before (5.22): "algebra of the this quiver" – delete "the".

Corrected.

39. first paragraph in Section 5.4: "And we define the open bow variety associated to an open bow diagram to be similar to that of a bow variety" – looks like "that of" is unnecessary.

Modified.

40. Later on page 39: "oepn" —> "open".

Corrected.

41. Later on page 39: "also hold at the quantum level" should have "holds".

Corrected.

42. Caption of Figure 17: "..whatever configurations of branes is needed..." – should have "are needed".

Corrected.

43. The procedure described on page 40 seems to be simply "gluing by gauging", which is well known in the literature. Is it?

Quite possibly. Any suggestion for some references we can cite would be greatly appreciated.

44. Conclusions: "Brane constructions similar to that of section 5.1 was used in [44]" – should have "were used".

Corrected.

45. Provide references for the Appendix A.

Formulas in Appendix A are original, at least, we were unable to find them elsewhere. We put them in the appendix because, while they are necessary for the Omega deformation construction, the reader does not necessarily need to go through them. To justify the formulas we can see that they reduce to the undeformed supersymmetry transformations (eq. 4.14, 4.19, and 4.23) when the deformation parameter $V$ is turned off and $\delta_V$ squares to a Lie derivative plus a gauge transformation (eq. A.4) which can be easily checked.

## References


[1] K. Costello, E. Witten and M. Yamazaki, *Gauge Theory and Integrability, I*, *ICCM Not.* **06** (2018) 46 [1709.09993].

[2] K. Costello and J. Yagi, *Unification of integrability in supersymmetric gauge theories*, *Adv. Theor. Math. Phys.* **24** (2020) 1931 [1810.01970].

[3] H. Nakajima and Y. Takayama, *Cherkis bow varieties and Coulomb branches of quiver gauge theories of affine type A*, 1606.02002.

[4] R. Rimanyi and Y. Shou, *Bow varieties—geometry, combinatorics, characteristic classes*, *arXiv e-prints* (2020) arXiv:2012.07814 [2012.07814].

[5] K. Costello, D. Gaiotto and J. Yagi, *Q-operators are 't Hooft lines*, 2103.01835.

[6] F. Levkovich-Maslyuk, *The Bethe ansatz*, *J. Phys. A* **49** (2016) 323004 [1606.02950].

[7] V.V. Bazhanov, R. Frassek, T. Lukowski, C. Meneghelli and M. Staudacher, *Baxter Q-Operators and Representations of Yangians*, *Nucl. Phys. B* **850** (2011) 148 [1010.3699].

[8] F. Kirwan, *Cohomology of Quotients in Symplectic and Algebraic Geometry*, Mathematical Notes - Princeton University Press, Princeton University Press (1984).

[9] D. Mumford and L. Ness, *A stratification of the null cone via the moment map*, *American Journal of Mathematics* **106** (1984) 1281.

[10] A. Braverman, M. Finkelberg and H. Nakajima, *Ring objects in the equivariant derived Satake category arising from Coulomb branches (with an appendix by Gus Lonergan)*, 1706.02112.

[11] S. Arkhipov, R. Bezrukavnikov and V. Ginzburg, *Quantum Groups, the loop Grassmannian, and the Springer resolution*, *arXiv Mathematics e-prints* (2003) math/0304173 [math/0304173].

[12] N. Chriss and V. Ginzburg, *Representation theory and complex geometry*, vol. 42, Springer (1997).

[13] H. Matsumura, *Commutative algebra*, vol. 120, WA Benjamin New York (1970).