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| [UAI2015](http://auai.org/uai2015/" \t "_blank) **31th Conference on Uncertainty in Artificial Intelligence** July 12-16, 2015, Amsterdam, The Netherlands |
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|  | **Reviews For Paper**   |  |  | | --- | --- | | **Paper ID** | 286 | | **Title** | Fully-Automated Symbolic Gibbs Sampling in Piecewise Algebraic Graphical Models with Nonlinear Determinism |  |  |  | | --- | --- | | **Masked Reviewer ID:** | Assigned\_Reviewer\_1 | | **Review:** |  |  |  |  | | --- | --- | | **Question** |  | | Paper summary. In this section please explain in your own words what is the problem that the paper is trying to address, and by what approach. In what area lie the main accomplishments of the paper. Optionally, a summary of your opinion, impact and significance could be included too. | The paper proposes a method, Symbolic Gibbs sampler, for performing approximate inference in graphical models whose variables are either define deterministically (relative to other variables) or are continuous with polynomial piecewise fractional distributions. Since Gibbs sampling does not handle determinism very well, the method first eliminates deterministic variables from the sampling set so that they can be summed out, resulting in a collapsed Gibbs sampler. However, unlike typical Gibbs sampler, Symbolic Gibbs sampler does not sample x\_i <-- P(x\_i | x \ x\_i) where P(x\_i | x \ x\_i) is computed when all other variables are instantiated. Instead, it exploits the properties of polynomial piecewise fractional distributions to pre-compute a multi-variate sampling distribution for each variable once - it accounts for summed out variables and includes other sampling variables in the function. At the time of sampling, the values of variables in x\ x\_i get instantiated in the P(x | x \ x\_i) while the actual CDF definition does not change. Computing CDF once is an efficiency improvement which saves time. | | Novelty. This is arguably the single most important criterion for selecting papers for the conference. Reviewers should reward papers that propose genuinely new ideas, papers that truly depart from the "natural" next step in a given problem or application. We recognize that novelty can sometimes be relative, and we ask the reviews to assess it in the context of the respective problem or application area. It is not the duty of the reviewer to infer what aspects of a paper are novel - the authors should explicitly point out how their work is novel relative to prior work. Assessment of novelty is obviously a subjective process, but as a reviewer you should try to assess whether the ideas are truly new, or are novel combinations or adaptations or extensions of existing ideas. | Symbolic Gibbs sampler is a twist on standard Gibbs sampler in application to sampling in models with continuous variables. However, it is a very clever adjustment that results in substantive improvement in Gibbs performance as shown in empirical evaluation (Figures 5 and 6). | | Novelty numeric score | 5-One idea that surprised me by its originality, solid contributions otherwise | | Technical quality. Are the results technically sound? Are there obvious flaws in the conceptual approach? Are claims well-supported by theoretical analysis or experimental results? Did the authors ignore (or appear unaware of) highly relevant prior work? Are the experiments well thought out and convincing? Are there obvious experiments that were not carried out? Will it be possible for other researchers to replicate these results? Are the data sets and/or code publicly available? Is the evaluation appropriate? Did the authors discuss sensitivity of their algorithm/method/procedure to parameter settings? Did the authors clearly assess both the strengths and weaknesses of their approach? | Aside from somewhat abrupt Introduction that jump right into details, paper has a good flow and does a good job exemplifying concepts in a simple example that it follow through. The authors did a good job defining Symbolic Gibbs sampler and explaining how it differs from standard Gibbs sampler. Thorough empirical evaluation. Still, extended Introduction and broader coverage of the state of the art for this class of problem would be recommended. | | Technical quality numeric score | 5-Technically adequate for its area, solid results | | Significance. Is this really a significant advance in the state of the art? Is this a paper that people are likely to read and cite in later years? Does the paper address an important problem (e.g., one that people outside UAI are aware of)? Does it raise new research issue for the community? Is it a paper that is likely to have any lasting impact? Is this a paper that researchers and/or practitioners might find useful 5 or 10 years from now? Is this work that can be built on by other researchers? | Symbolic Gibbs sampler applies to a specific but broad class of models. As such, it does contribute a result that would of interest to a large community of researchers working with models with continuous variables. It could be extended to multiple probabilistic inference problems found in practice given a mapping from that problem domain space to the model that satisfy the requirements. | | Significance numeric score | 5-Solid contribution to relevant problem | | Quality of writing. Please make full use of the range of scores for this category so that we can identify poorly-written papers. Is the paper clearly written? Does it adequately inform the reader? Is there a good use of examples and figures? Is it well organized? Are there problems with style and grammar? Are there issues with typos, formatting, references, etc.? It is the responsibility of the authors of a paper to write clearly, rather than it being the duty of the reviewers to try to extract information from a poorly written paper. Do not assume that the authors will fix problems before a final camera-ready version is published - there will not be time to carefully check that accepted papers are properly written. It may be better to advise the authors to revise a paper and submit to a later conference, than to accept and publish a poorly-written version. However if the paper is likely to be accepted, feel free to make suggestions to improve the clarity of the paper, and provide details of typos under 11. | 4-Quality of writing is excellent | | Overall Numeric Score for this Paper: | 5-A good paper overall, accept if possible. I vote for acceptance, although would not be upset if it were rejected because of the low acceptance rate. | | (Optional) Additional Comments to the Authors: please add any additional feedback you wish to provide to the authors here. For example, if the quality of writing in the paper is not excellent, please provide some feedback to the authors on how the writing can be improved. | General comments:  Introduction section feels "shortened" as the paper jumps right into nitty-gritty. It really needs to cover the current state-of-the-art for given class of models. An outline of the paper in the Introduction would also be helpful.  Minor language corrections:  1) Introduction  "momenta" => "moments" ?  2) Section 3.1, just before Theorem 1  "To justify the correctness of correctness of Algorithm 1..." ==> remove one of "correctness of" |  |  |  | | --- | --- | | **Masked Reviewer ID:** | Assigned\_Reviewer\_2 | | **Review:** |  |  |  |  | | --- | --- | | **Question** |  | | Paper summary. In this section please explain in your own words what is the problem that the paper is trying to address, and by what approach. In what area lie the main accomplishments of the paper. Optionally, a summary of your opinion, impact and significance could be included too. | This paper proposes a method of Symbolic Gibbs Sampling, which aims to overcome two limitations of traditional Gibbs Sampling algorithms. They cannot be directly applied to models with determinism nor can the integrals required for deriving conditional distributions be symbolically computed for general piece-wise functions. | | Novelty. This is arguably the single most important criterion for selecting papers for the conference. Reviewers should reward papers that propose genuinely new ideas, papers that truly depart from the "natural" next step in a given problem or application. We recognize that novelty can sometimes be relative, and we ask the reviews to assess it in the context of the respective problem or application area. It is not the duty of the reviewer to infer what aspects of a paper are novel - the authors should explicitly point out how their work is novel relative to prior work. Assessment of novelty is obviously a subjective process, but as a reviewer you should try to assess whether the ideas are truly new, or are novel combinations or adaptations or extensions of existing ideas. | The paper provides an interesting extension of Gibbs sampling for graphical models with nonlinear constraints among variables and piece-wise distributions. The algorithm, SymGibbs, as provided in the paper, also appears to be computationally efficient. | | Novelty numeric score | 5-One idea that surprised me by its originality, solid contributions otherwise | | Technical quality. Are the results technically sound? Are there obvious flaws in the conceptual approach? Are claims well-supported by theoretical analysis or experimental results? Did the authors ignore (or appear unaware of) highly relevant prior work? Are the experiments well thought out and convincing? Are there obvious experiments that were not carried out? Will it be possible for other researchers to replicate these results? Are the data sets and/or code publicly available? Is the evaluation appropriate? Did the authors discuss sensitivity of their algorithm/method/procedure to parameter settings? Did the authors clearly assess both the strengths and weaknesses of their approach? | Through several experiments evaluating quality and the convergence rate, etc., the proposed method shows good advantages, compared with several existing methods. These methods include baseline Gibbs, rejection sampling, tuned Metropolis-Hastings, Hamiltonian Monte Carlo and Sequential Monte Carlo.  The discussion of related work, as presented in the article, is somewhat limited. (Several of the references are to textbooks and monographs rather than the original research.) Thus the value of the work in connection to recent research could be clearer.   In particular, it would be nice if the following questions were investigated in more detail:  1. Has there been any attempts to address the problem of nonlinear constraints and piecewise distributions? 2. In the experimental results, is the performance of SymGibbs significantly better than BaseGibbs? Fig 5(a), 6(a), 6(b) only show marginal improvement. | | Technical quality numeric score | 5-Technically adequate for its area, solid results | | Significance. Is this really a significant advance in the state of the art? Is this a paper that people are likely to read and cite in later years? Does the paper address an important problem (e.g., one that people outside UAI are aware of)? Does it raise new research issue for the community? Is it a paper that is likely to have any lasting impact? Is this a paper that researchers and/or practitioners might find useful 5 or 10 years from now? Is this work that can be built on by other researchers? | The work has the potential of being significant, both inside and outside of the UAI community. | | Significance numeric score | 5-Solid contribution to relevant problem | | Quality of writing. Please make full use of the range of scores for this category so that we can identify poorly-written papers. Is the paper clearly written? Does it adequately inform the reader? Is there a good use of examples and figures? Is it well organized? Are there problems with style and grammar? Are there issues with typos, formatting, references, etc.? It is the responsibility of the authors of a paper to write clearly, rather than it being the duty of the reviewers to try to extract information from a poorly written paper. Do not assume that the authors will fix problems before a final camera-ready version is published - there will not be time to carefully check that accepted papers are properly written. It may be better to advise the authors to revise a paper and submit to a later conference, than to accept and publish a poorly-written version. However if the paper is likely to be accepted, feel free to make suggestions to improve the clarity of the paper, and provide details of typos under 11. | 3-Quality of writing is good, but could be improved with some editing | | Overall Numeric Score for this Paper: | 5-A good paper overall, accept if possible. I vote for acceptance, although would not be upset if it were rejected because of the low acceptance rate. |  |  |  | | --- | --- | | **Masked Reviewer ID:** | Assigned\_Reviewer\_3 | | **Review:** |  |  |  |  | | --- | --- | | **Question** |  | | Paper summary. In this section please explain in your own words what is the problem that the paper is trying to address, and by what approach. In what area lie the main accomplishments of the paper. Optionally, a summary of your opinion, impact and significance could be included too. | This paper proposes a generic inference mechanism for probabilistic models with nonlinear deterministic constraints. The deterministic constraints are removed by eliminating variables to leave behind a set of piecewise polynomial fractions (PPF) with constraints. Further, under benign conditions (e.g. constraints are quadratic) the PPF can be integrated to allow for Gibbs sampling.  This is definitely an advance in generic probabilistic inference. The techniques for eliminating deterministic variables are also quite sophisticated.  However, the authors haven't provided conclusive evidence. The experiments are to do with simple collisions and trivial electrical wiring models and all the experiments finish within two seconds!  This lack of long running samplers and lack of large model sizes makes one suspect that the method is perhaps not scalable for a large number of variables. The authors haven't really provided any results on the growth in the complexity of the PPFs as the variables are eliminated.  The other high-level issue with the paper is the lack of motivation for models with a large number of non-linear deterministic constraints. Most Bayesian models incorporate some degree of observation error as well as randomness injected to allow for an incomplete understanding of the physics (for example collisions may not be completely elastic and electricity can dissipate for other reasons). | | Novelty. This is arguably the single most important criterion for selecting papers for the conference. Reviewers should reward papers that propose genuinely new ideas, papers that truly depart from the "natural" next step in a given problem or application. We recognize that novelty can sometimes be relative, and we ask the reviews to assess it in the context of the respective problem or application area. It is not the duty of the reviewer to infer what aspects of a paper are novel - the authors should explicitly point out how their work is novel relative to prior work. Assessment of novelty is obviously a subjective process, but as a reviewer you should try to assess whether the ideas are truly new, or are novel combinations or adaptations or extensions of existing ideas. | The work is certainly novel. Eliminating variables was easy enough but the part about using the composition of a dirac delta with a function was very clever and something that I haven't seen before. | | Novelty numeric score | 5-One idea that surprised me by its originality, solid contributions otherwise | | Technical quality. Are the results technically sound? Are there obvious flaws in the conceptual approach? Are claims well-supported by theoretical analysis or experimental results? Did the authors ignore (or appear unaware of) highly relevant prior work? Are the experiments well thought out and convincing? Are there obvious experiments that were not carried out? Will it be possible for other researchers to replicate these results? Are the data sets and/or code publicly available? Is the evaluation appropriate? Did the authors discuss sensitivity of their algorithm/method/procedure to parameter settings? Did the authors clearly assess both the strengths and weaknesses of their approach? | Much as I like to believe that this paper has succeeded in solving an important problem I'm extremely dismayed by the lack of experimental evidence.  This paper does not talk about growth in the size of the PPFs as the variables are eliminated. I suspect that the PPFs would grow exponentially making the integral required for Gibbs sampling impractical.  In Figure 4 for asymmetric collisions the experiments finish in 0.3 seconds. In Figure 5 only 4 or 20 objects are considered and the experiments finish in 2 seconds. Similar numbers for Figure 6, electrical wiring.  These experiments should be run for much larger model sizes, for thousands of objects, with running time over hours. Experiments should show running time increase for each of the algorithms with model size.  The use of the MAE for evaluation can mask errors when a large number of variables are involved. It would have been better to consider the variable with the worst error. | | Technical quality numeric score | 3-Claims not completely supported, assumptions or simplifications unrealistic | | Significance. Is this really a significant advance in the state of the art? Is this a paper that people are likely to read and cite in later years? Does the paper address an important problem (e.g., one that people outside UAI are aware of)? Does it raise new research issue for the community? Is it a paper that is likely to have any lasting impact? Is this a paper that researchers and/or practitioners might find useful 5 or 10 years from now? Is this work that can be built on by other researchers? | Eliminating deterministic constraints or near-deterministic constraints in probabilistic inference is an important research problem that is often tackled in many inference papers. This paper addresses the problem for well-behaved polynomials in a very clever way. I would imagine that this would be referenced by a number of papers in this area if the experimental results hold up for larger problems. | | Significance numeric score | 5-Solid contribution to relevant problem | | Quality of writing. Please make full use of the range of scores for this category so that we can identify poorly-written papers. Is the paper clearly written? Does it adequately inform the reader? Is there a good use of examples and figures? Is it well organized? Are there problems with style and grammar? Are there issues with typos, formatting, references, etc.? It is the responsibility of the authors of a paper to write clearly, rather than it being the duty of the reviewers to try to extract information from a poorly written paper. Do not assume that the authors will fix problems before a final camera-ready version is published - there will not be time to carefully check that accepted papers are properly written. It may be better to advise the authors to revise a paper and submit to a later conference, than to accept and publish a poorly-written version. However if the paper is likely to be accepted, feel free to make suggestions to improve the clarity of the paper, and provide details of typos under 11. | 3-Quality of writing is good, but could be improved with some editing | | Overall Numeric Score for this Paper: | 3-A weak paper, just not good enough. I vote for rejecting it, but could be persuaded otherwise. | | (Optional) Additional Comments to the Authors: please add any additional feedback you wish to provide to the authors here. For example, if the quality of writing in the paper is not excellent, please provide some feedback to the authors on how the writing can be improved. | The introduction should better motivate the problem of nonlinear deterministic constraints. It is not obvious that such constraints are highly prevalent in common models. The examples given in this paper of collisions with momentum perfectly preserved and parallel electrical wiring with no dissipation losses seem very unrealistic.  Section 3.1 could be better organized. For example \Phi is used without proper definition in the main text. One has to infer this from the algorithm. Also theorem 1 is referred half a page before it is actually defined. | |  |